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TRANSACTIONS.

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[Read 16th December 1844, and 6th January 1845.]

IT is unnecessary to detail to this Society the various ingenious hypotheses which have been proposed to account physiologically for the accommodation of the eye to distinct vision at different distances. In later years, these different theories have been so circumstantially and correctly recapitulated in systematic works (as for instance in YOUNG's Lectures and in MÜLLER's Physiology), that it would be a waste of time to copy and recite them here. I will only do so, then, so far as may be necessary to justify the attempt I have now to make, and to strengthen my views by those of others, as far as they bear upon them.

The eye being the organ of sense best understood, and constructed upon the most intelligible principles,—being one whose functions, up to a certain point, may be accurately represented by an artificial apparatus, it is impossible to doubt that the ultimate function of vision depends on the formation of a distinct picture of an object upon the retina, and that the circumstances which affect the distinctness of the picture in the instrument or artificial eye, must affect the clearness of vision in the real eye. Such a circumstance is notoriously the distance at which objects are placed from the eye. Now it is known by experience, (1.) That objects at very variable distances may, in the healthy organ, be distinctly seen; (2.) That such variations have limits, beyond which distinct vision cannot, by any effort, be obtained; (3.) This limit varies in different eyes; (4.) The limit may be extended by optical aid, which would, in the model or artificial eye, produce the same effect; (5.) The adjustment of the eye to different distances is felt to be accompanied by a distinct muscular effort. On all these grounds, we conclude that the focal adjustment of the eye is a real mechanical adjustment, tending to

form an optically distinct picture on the retina; and that the opinion of those physiologists is to be disregarded, who have supposed that the distinctness of vision at one distance or another arises merely from a mental effort of attention.

We assume it, then, to be granted that the adjustment of which we are in quest is of a nature such, that when the eye is turned from a distant to a near object, *either* the retina is moved from the refracting apparatus of the eye, so that the less convergency of the rays may be allowed for by the increased distance; *or else*, that the distance of the retina or pictured screen remaining the same, the refraction of the eye is increased, so as to cause the rays to converge more rapidly than they would have done in the previous state of the eye.

If the first alternative be true, the axis of the eye must undergo an *elongation* of about one-seventh part (according to OLBERS* and YOUNG†), in passing from the distinct vision of distant to that of very near objects. Dr YOUNG has described two experiments, by which, he says, he satisfied himself that the elongation of the eye could not be anything like the quantity required by the hypothesis. Dr YOUNG's experiments are obscurely described; but perhaps a not less conclusive and more simple proof of the error of this explanation is found in the fact insisted on by TREVIRANUS and MÜLLER, and which seems to me quite unanswerable,—“that the tendency of the straight (*recti*) muscles is merely to retract the eye, and if resistance were afforded by the cushion of fat behind it, to flatten rather than elongate it; their action would, therefore, have the effect of adapting the eye to the vision of distant objects only, the image of which is formed nearer the lens, than that of near objects; while it is in looking at very near objects, on the contrary, that we are conscious of an effort within the orbit.”‡

It seems difficult to admit with MÜLLER, however, that any conclusion as to the mechanism of the eye can be drawn from the transient and anomalous changes of adjustment which it seems to undergo under the influence of narcotics, such as belladonna.

The other class of explanations turn upon the production of an increased refractive power in the eye, by the altered curvature of one of its numerous refracting surfaces. Every one of these has been in turn fixed upon as the subject of the change, as well as those parts of structure which, by their intimate connexion with the principal parts, might be supposed to influence them. The cornea, the lens, the iris, and the ciliary processes, have each been supposed to be the part immediately affected. Most of the theories have been refuted with consummate skill by Dr YOUNG, in his paper on this subject in the Philosophical Transactions for 1800; and, as is well known, he himself attributed the change of focal adjustment to a *proper muscular power residing in the lens*. This other-

* Quoted by MÜLLER.

† Nat. Phil. ii. 589.

‡ MÜLLER's Physiology, translated, p. 1143, 1144.

wise probable opinion is contradicted by the fact, that the muscularity of the lens is unproved, and that this organ is wholly unprovided with bloodvessels and nerves. The opinion now adopted by several eminent living authors is, that the "first step in the process is the variation of the pupil, which seems, by a mechanism at the base of the iris, to increase the distance of the lens from the retina."* This is very vague; it is shewn by conclusive experiments that the simple contraction and expansion of the iris produces no effect on the focal adjustment;† and it is a mere conjecture that any of the organs connected with the iris, the ciliary body for instance, has, or can have, any influence in pulling the lens forward from the retina in any degree, much less through the considerable space requisite. We may, therefore, accept the *résumé* of a late French writer on Physics, as nearly expressing the opinion of the most candid authors upon this vexed subject: "Tout cela n'est pas très-satisfaisant, et il faut avouer que l'explication de la netteté de la vision à des distances si différentes est encore à trouver."

Such being the present phase of the question, the suggestion of a "possible explanation" yet unthought of, of the manner of the adjustment of the eye, may be received with indulgence, or at least proposed without presumption.

About three years ago, whilst lecturing on the subject of vision, I was struck with the circumstance, that the crystalline lens possesses not only a remarkable gradation of consistence or density from the centre towards the surface, and especially towards the edges, whereby, according to the common explanation, the spherical aberration of the rays of light is completely corrected; but likewise a complex and singular figure, which it is plain might alone produce the same effect by the modified curvature of the surfaces. Here, then, we appear to have two peculiarities of structure to attain one end; and it seems so natural, that the curves should be proper curves for destroying the aberration of sphericity, instead of the spherical curves which are used in our instruments only from our incapacity to form better ones,‡ that it occurred to me that the remarkable variations of density in the lens must be intended to answer another purpose.

This purpose I conceived might be the focal adjustment, and effected in the

* BREWSTER in ART. OPTICS, Encyc. Brit. 7th Edit. p. 513.

† See MÜLLER and BREWSTER.

‡ The forms of curvature of the crystalline lens are said to have been actually ascertained by M. CHOSSAT to be ellipsoidal. It is a curious proof of the vagueness with which this subject has been treated, that, in the clear and able work of Professor LLOYD on Light and Vision, in one page, the form of the surfaces is insisted on as the means of producing distinct vision; and on another, the gradation of density from the centre to the side of the lens; whereas, it is certain, that if the compensation for spherical aberration due to the last cause be correct, the ellipsoidal form will be erroneous. Thus, as in many other cases, the argument for design has been made to prove too much. See LLOYD on Light and Vision, pp. 264-266, who refers to CHOSSAT's paper, *Ann. de Chimie*, vol. x.

following way: The crystalline lens, for example, that of the ox, is composed of a nearly spherical nucleus of compact comparatively dense matter, of a hard pasty consistence, which gradually, yet rapidly, passes into the gelatinous envelope of a lenticular form, which has far less consistence, and less resistance to external pressure than the central spherule. It therefore occurred to me, that any *uniform* pressure applied to the lens, such as might be communicated by the external muscles of the eye to the entire eyeball, and propagated by hydrostatic pressure through the humours, would tend to make the exceedingly flattened ellipsoid of the eye approach in figure to the dense spheroidal nucleus; the obvious effect of which would be, without any change in the position of the lens, to increase its curvature, so as to render the rays from a near object more convergent.

I proceeded, in April 1842, to endeavour to put my hypothesis to the proof, by subjecting the recent crystalline lens of a bullock to considerable hydrostatic pressure, in a suitable apparatus, and endeavouring to observe the change of focal distance produced, making it act as the object-glass of a microscopic arrangement; but, partly owing to the difficulty of suspending the lens in a secure yet free manner, partly from the unfavourable form of the glass vessel used for compression, partly from the small excess of refracting power of the lens above that of the water in which it was suspended, and partly from the essential indistinctness of the picture formed in the dead eye, and the consequent difficulty of determining its precise focal distance;—from all these causes my experiments failed in yielding a positive result;* and though I communicated my views soon after to Dr ALISON, I postponed any farther consideration or publication of the subject, until I should be able to support the theory by decisive experiments. My attention has been wholly diverted since to other inquiries; and I see no prospect, at present, of resuming the experimental part, which, no doubt, would be worth pursuit, and though difficult, is not I think, hopeless. In the mean time, the subject of focal adjustment of the eye having been started at the late meeting of the British Association at York by Sir D. BREWSTER, it occurred to me to state verbally my notions; which having been thought worthy of attention, I have put them into this more definite and permanent shape.

In the absence of a direct proof in favour of my hypothesis (and this, it will be observed, no other theory possesses), I may be allowed to state one or two circumstantial evidences in its favour.

The first has been mentioned already, but is recapitulated for the sake of connection.

1. The crystalline lens possesses, on the common view, a twofold structure

* It may be added, that the bullock's eye is perhaps one of the least favourable on which the experiment could be made. Owing to its very great convexity and thickness, it may be presumed that the action of compression above described will be much less visible than in a comparatively flat lens, such as that of man.

to produce a single end. I assume that each structural condition has a separate end; the variable curvature to correct the aberration, the variable density to alter the figure of it under pressure.

2. The attempt to view near objects distinctly is accompanied by a sensible muscular effort within the orbit. This is expressly stated, incidentally, by MÜLLER, in a passage already quoted; and has been admitted by every one whom I have questioned on the subject. From my own sensation, I have no doubt that it is a simultaneous effort of the four *recti* muscles drawing the eye back within its socket.* Such a retractive muscular action, fatal to the theory of elongation of the eyeball, is just what we require to communicate to the fluid humours of the eye, through the tough sclerotic coat in which they are bound, the hydrostatic pressure which will act simultaneously upon all points of the crystalline lens.

3. This theory is free from the unanswerable objections urged by Dr YOUNG and others, to all theories independent of that which ascribes the adjustment to change of figure in the lens: and it is free from the objection to Dr YOUNG's own theory, which presumes a structure existing in the lens itself, unproved, and, to say the least, improbable,—I mean its muscularity.

4. It is confirmed by the fact, that where the lens is reproduced after the operation for cataract, the power of adjustment is almost or totally lost; for, in that case, it cannot be supposed that the new lens is provided with the requisite gradation of coats for modifying its elasticity.

5. The diminution of the adjusting power of the eye in old age is well explained by the collapse and induration of the lens, to the detriment of its elastic properties.†

6. That the crystalline lens is actually possessed of variable elasticity in different directions is rendered highly probable by Sir DAVID BREWSTER's observations on its action on polarized light, in which tints are produced similar to those in compressed jellies, and in minerals possessing different axes of elasticity.

POSTSCRIPT TO THE PRECEDING PAPER.

When this paper was written, I had not seen CHOSSAT's paper on the Forms of the Refracting Surfaces of the Eye, which I have referred to. I have since read it,‡ and find a remarkable confirmation of the views I entertain.

It is very plain, that, were the gradations of the refrangibility of the coats of

* I am aware that this is opposed to the experiment of Mr RAMSDEN and Sir E. HOME, which seems to shew a protrusion of the eyeball. Supposing it correct, that protrusion must be equally the result of muscular action producing pressure, due perhaps to the oblique muscles antagonising the *recti*; for it is difficult to see where else it can be sought.

† "In all animals the crystalline lens grows firmer with age." BREWSTER Edin. Encyc., Art. OPTICS, p. 475.

‡ In the *Annales de Chimie*, vol. x., published in 1819.

the crystalline intended to correct spherical aberration, this condition presumes the sphericity of the surfaces. If the surfaces have the curve of *no aberration*, for instance, an ellipsoid, with its longer axis parallel to the incident rays, any variation of density of the medium is not only useless but hurtful, producing a contrary error, and causing the central rays to converge too fast. On the contrary, if the variable density of the matter of the crystalline exist (which is an undoubted fact), and has been so arranged for a distinct purpose, the form of the surface of no aberration adapted to it will be a peculiar one, and, very probably, will be more convex towards the lateral parts than even the sphere, much more than the ellipsoid already mentioned, which is the curve of no aberration for a lens of uniform density.

Having perceived this result, it was with no small satisfaction that I found, on examining M. CHOSSAT's paper, that whilst for the cornea (where the refraction is from air into the uniformly dense medium of the aqueous humour), the surface is that of an ellipsoid, with the longer axis in the direction of the incident rays, and which, therefore, destroys aberration by the appropriate curvature,—in the lens, the figure is that generated by an ellipse revolving round its *lesser* axis, and therefore possessing a contrary property to that ordinarily required for correcting aberration; the curvature being greater for the lateral than the central parts of the lens. This is surely an unanswerable proof, that the opinion maintained by, I believe, every modern writer on optics, without exception, namely, that the variable density of the lens is intended to correct *spherical* aberration, is a fallacy, since we find it combined with an appropriate figure for destroying *its* aberration, in which the peculiarities of spherical refraction are exaggerated.

The measures and drawings of M. CHOSSAT appear so minute and correct, as to leave no doubt of the fact of this antagonism to the common opinion.* Accordingly, neither by himself, nor by the few authors who have quoted this singular circumstance, has any explanation been given.

On our theory it is simple. The gradation of density has been provided for the mechanical purpose of varying the elasticity of the lens in different directions; and the form of its surfaces has been then determined so as to render the lens *aplanatic*.

* M. CHOSSAT's experiments were made on the lens of an ox; but Dr ALLEN THOMSON tells me that, without knowing his results, he had arrived at a similar conclusion, as to the opposite kind of curvature in the *cornea* and in the *lens* of the human eye; the surface of the former lying without, and the latter within the surface of the osculating sphere.

II.—*On the Modification of the Doubly Refracting and Physical Structure of Topaz, by Elastic Forces emanating from Minute Cavities.* By SIR DAVID BREWSTER, K. H., D. C. L., F. R. S., and V. P. R. S., Edin.

(Read 20th January 1845.)

WHILE examining, in polarised light, the form and structure of the numerous crystals which I had discovered in the fluid cavities of *Topaz*, my attention was particularly called to certain optical phenomena exhibited in other parts of the specimen. These phenomena, when first presented to me, were very indefinite in their character, and very imperfectly developed; but after a diligent examination of nearly 900 specimens of topaz, I succeeded in obtaining the most satisfactory exhibition of them under various forms, and in various degrees of intensity.

When an elastic force is propagated from a centre, in a soft and compressible medium, an increase of density is communicated to the surrounding mass,—of a temporary nature if the medium is a hard solid, like glass, but of a permanent nature if the medium is soft, and becomes indurated during the continuance of the compressing force. Both these effects may be exhibited experimentally, the first by a pressure upon glass, and the second by the action of an expanded bubble of air upon gum in a state advancing to induration.

The physical change thus produced in the transparent medium, whether it be temporary or permanent, may be exhibited to the eye in two ways, either by the property of the compressed parts in depolarising light, or in the unequal refraction of common light produced by a varying density, and consequently a varying refractive power. In the *first* of these cases, the depolarising action is displayed in the production of four quadrants of light, separated by the radii of a black rectangular cross, similar to the central portion, or the tints of the first order, in the uniaxal system of polarised rings; and, in the *second* case, the inequality of refractive density is shewn by the mirage of a luminous point, in the form of concentric circles surrounding the centre of force, each circle marking successive actions of the central force.

When the four luminous quadrants of depolarised light, shewn at A, B, C, D, in Plate, Fig. 1, first presented themselves to me, I had some difficulty in perceiving the seat of the force, by which I believed that they were produced. The centres, or intersections of the black cross, were either too deep beneath the surface of the topaz, or too much covered by fluid cavities, to be seen; but by removing the part of the crystal which contained these cavities, I succeeded in finding that, in every case there was a minute cavity in the centre of the luminous quadrants, or at the

intersections of the arms of the black cross, from which the compressing force had emanated. One of these cavities is shewn at E., Fig. 2. It is of a quadrangular form, like the section of a rhomboidal prism, sometimes elongated, and sometimes of a slightly irregular shape. When perfectly regular, these cavities are between the 3000th and the 4000th of an inch in diameter. They are always dark, as if the elastic substance which they contained had collapsed into an opaque powder; and I have met with only one case in which there seemed to be a speck of light in the centre. The degree of compression to which the topaz has been subjected is measured by the polarised tint developed in the luminous quadrants. It varies from the faintest pale *blue* to the *white* of the first order. In one case I found the luminous quadrant of one cavity coinciding with a luminous quadrant of another cavity, and thus producing the sum of their separate tints. This effect is shewn in Fig. 3.

In the phenomenon now described, the elastic force has spent itself in the compression of the topaz. The cavity itself has remained entire, without any fissure by which a gas or a fluid could escape. I have discovered, however, other cavities, and these generally of a larger size, in which the sides have been rent by the elastic force; and fissures, from *one* to *six* in number, propagated to a small distance around them. These fissures have modified the doubly refracting structure produced by compression; but, what is very interesting, no solid matter has been left on the faces of fracture, such as that which is invariably deposited, when an ordinary cavity, containing one or both of the two new fluids, is exploded by heat. The form of some of the cavities which have suffered this disruption is shewn in Figs. 4, 5, and 6.

The influence of the compressing forces in altering the density, and consequently the refractive power of the topaz, is so distinctly seen in common light as to indicate the phenomena that are seen under polarised light. When the cavity is most distinctly perceived, it is surrounded with luminous and shaded circles, as shewn in Fig. 7; and traces of these are distinctly seen, as shewn in Fig. 8, when the specimen is examined in polarised light.

The cavities now described have obviously no resemblance whatever to those which I have described in previous papers as containing two new fluids. When any of the latter are either burst by heat, or exposed under high temperatures to the compressing forces of the fluids which they contain, they exhibit none of the phenomena peculiar to the former. The doubly refracting structure suffers no change; and when the cohesive forces of the crystal are overpowered, the faces of most eminent cleavage separate, and are covered with translucent crystalline particles, which the evaporated or discharged fluids leave behind.

The peculiar character of the pressure cavities, as we may call them, is still farther evinced by the nature of the specimens in which they occur. I have never found them accompanying the ordinary cavities with two fluids. The specimens

which contain them have imbedded in them numerous crystals, differing little in their refractive power from topaz, and exhibiting in polarised light the most beautiful colours, varying with the thickness of the crystal, and diminishing in intensity as their axes approach to the plane of primitive polarisation.

It is impossible to review the preceding facts without arriving at the conclusion, that the topaz must have been in a soft and plastic state when it yielded to the compressing force which emanated from the cavities, and that a mineral body thus acted upon could not have been formed, according to the received theory, by the aggregation of molecules having the primitive form of the crystal.

In a letter to Sir JOSEPH BANKS, printed in the Philosophical Transactions for 1805, I deduced, from my experiments on depolarisation, the existence of a new "species of crystallization, which is the effect of time alone, and which is produced by the slow action of corpuscular forces;" and I have remarked that "this kind of crystallization will probably be found to have had an extensive influence in those vast arrangements which must have attended the formation of our globe." These views have been confirmed by various new facts, wholly independent of each other;—by the existence of crystals imbedded in topaz, and having their axes in all possible directions, but especially by the nature and form of the strata of fluid cavities in that mineral. These strata cut at all inclinations the primary and secondary planes of the crystal. They are bent in the most capricious manner, forming planes of double curvature; and, what is also true of individual cavities stretching in every possible direction, they could never have been formed but when the topaz was in a soft and plastic state.

An objection to these views may be drawn from the fissures which proceed from the pressure cavities. The topaz must, doubtless, have been indurated when these fissures took place; but it is equally obvious that the depolarisation produced by compression must have previously existed, and it is probable that the fissures were produced after the crystal had been removed from its matrix, and when, from cleavage or otherwise, its cohesive forces had been diminished.

ST LEONARD'S COLLEGE, ST ANDREWS,
January 16, 1845.

III.—*On the Existence of Crystals with different primitive forms and physical properties in the Cavities of Minerals ; with additional Observations on the New Fluids in which they occur.* By SIR DAVID BREWSTER, K.H., LL.D., F.R.S., and V.P.R.S., Edin. *ref*

(Read Feb. 17, 1845.)

IN 1823 and 1826 I communicated to the Society two papers on the nature and properties of two immiscible fluids, which I discovered, in contact with each other, in the cavities of topaz and other minerals. Although the facts contained in these papers were of so extraordinary a nature as to be received with scepticism by some, and with ridicule by others, yet I am not aware that, during the *twenty* years which have elapsed since their publication, any person has either repeated my observations, or advanced a single step in the same path of inquiry. In shewing to strangers some of the leading phenomena of the two new fluids, my attention has been frequently recalled to the subject ; but it was not till last spring, when I discovered cavities in topaz filled with the most beautiful crystals of various form, that I was induced to undertake a new investigation of their nature and properties. In this investigation I have examined, with various magnifying powers, and both in common and polarised light, more than 900 specimens of topaz from Scotland, New Holland, and the Brazils ; and I have had the good fortune to observe many new phenomena connected with mineralogy, chemistry, and physics, which, in addition to the interest which they may possess as scientific facts, promise to throw a strong light upon the existing theories of crystallization, and to bring before us some of those recondite operations which had been going on in the primitive rocks of our globe, before the commencement of vegetable or animal life.

1. *On the Form and Position of the Strata in which the Cavities lie.*

The cavities which contain the two new fluids, and their accompanying crystals, sometimes occur single, and in groups more or less numerous ; but, in general, they exist in millions, occupying extensive strata, which affect the transparency of the mineral, and render it unfit for the use of the jeweller, or even for the cabinet of the collector, who has not learned that it is in the deviations from her ordinary laws that Nature often discloses her deepest mysteries.

Although the strata of cavities sometimes occur, as in artificial salts, in planes parallel to the primary or secondary forms of the crystal, yet they occupy every

possible position in reference to these planes; and we, therefore, cannot account for them by supposing that certain spaces have been left in the crystal, without the primitive molecules which ought to have been there deposited. The strata of cavities, too, have every possible curvature. From a plane surface they pass into a curved one, sometimes of variable curvature, and sometimes of contrary flexure, cutting and intersecting each other in the most capricious manner.

In the shape of the strata the same irregularity presents itself; their outline is sometimes rectilineal, sometimes curved, and sometimes singularly irregular. In some specimens the whole crystal is intersected with the strata; and it is extremely probable, though it is impossible to determine the fact, that in every specimen some edge or angle of the stratum touches the surface.

The succession of the cavities in composing the stratum, and their form in relation to the character of the stratum, present interesting phenomena. I have found specimens in which the cavities lie in concentric arches, and have their sides concentric, and, as it were, a portion of the same arches, as if they had been formed under the influence of a rotatory force. In other cases they occupy parallel lines, and are sometimes so equidistant that they might be advantageously used as micrometers for microscopes. In one remarkable specimen they radiate from a centre, each radiation having a character of its own. One radiation will sometimes throw off a diverging branch, while two or more radiations will converge and then diverge again, subsequently uniting themselves into a single radiation.

When different strata of cavities lie parallel to each other in the specimen, which they sometimes do, to the number of *four* or *five*, each stratum has generally a distinct character; flat and exceedingly thin cavities occupying one stratum, very deep cavities occupying another, minute cavities which the highest magnifying powers can scarcely resolve occupying a third, while a fourth consists of the most irregular and indescribable forms.

When the forms of individual cavities are related to that of the stratum which contains them, they, of course, cut at all angles the primary and secondary planes of crystallization; and the same is true of insulated cavities of great length, which are sometimes turned, and twisted, and bent, in the most capricious manner. It is impossible to read these details, and still more so to study the phenomena themselves, without being driven to the conclusion, that the strata of cavities must have been formed under the influence of forces propagated through a soft and plastic mass, and carrying along with them gases and vapours which came to a position of rest previous to the regular crystallization of the topaz. This conclusion, which I have been led to draw, in another paper, from a series of entirely different facts, will be still further confirmed by the phenomena of imbedded crystals, to which I shall have to refer in another section.



Fig. 1.

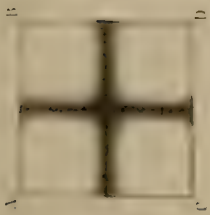


Fig. 2.



Fig. 3.



Fig. 4.

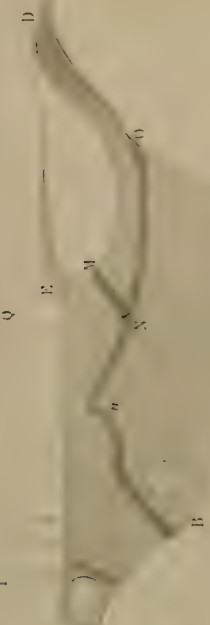


Fig. 5.

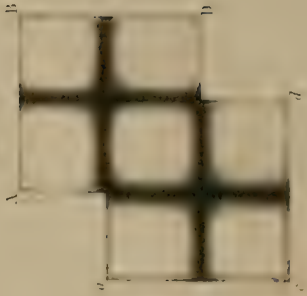


Fig. 6.



Fig. 7.



Fig. 8.



Fig. 9.



Fig. 10.



Fig. 11.



Fig. 12.



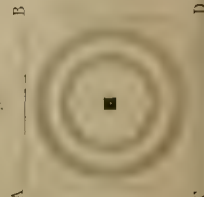
Fig. 13.



Fig. 14.



Fig. 15.



2. *Additional Observations on the Nature and Properties of the two New Fluids.*

In re-examining the phenomena exhibited by the two new fluids, I have found no occasion to modify or to correct any of the results contained in my former papers. In the cavities which appear to contain only one fluid, namely, the dense fluid, I have sometimes found a very small quantity of the volatile fluid, which, with a slight rise of temperature, passes into vapour, and prevents the apparent vacuity from disappearing by the application of a strong heat. When there is no volatile fluid present in such cavities, the vacuity is a real one, and disappears entirely by the application of such a heat. If the heat is not instantly withdrawn on the disappearance of the vacuity, the crystal never fails to burst with great violence.

In some specimens of Brazil topaz I have found cavities with two fluids, and without any vacuity in the volatile fluid at the ordinary temperature of an apartment. In such cases I have generally produced a vacuity by the application of ice. Had heat been applied, the crystals would have burst, as there were no empty spaces into which the fluids could expand.

When the cavities are flat, and have their faces perpendicular to the axis of the crystal, or parallel to the planes of *easy cleavage*, the application of heat does not burst the crystal, but produces a very remarkable phenomenon. The cavity opens at its weakest point, and the fluid passes by starts, through a succession of resting places, to another part of the crystal where it finds the readiest exit. The fluid penetrates, as it were, the solid gem, and the laminæ which it has forced asunder in its passage, again close into optical if not into mechanical contact. If the heat is withdrawn when the first minute drop has passed, the laminæ unite, and we can discharge the rest of the fluid whenever we please till the cavity is exhausted. This phenomenon is represented in Plate, Fig. 9, where A B C D is a shallow cavity in a plate of topaz M N, and E F another cavity, which has been emptied of its fluid contents by reaching the surface at N, where it had been broken through. Upon looking at the cavity A B C D when slightly heated, I observed dark portions of fluid rushing from its sharp termination at D through the cavity at *a*, and then reappearing at *b* and *c*, and then passing into the empty cavity E F. The small lakes, as we may call them, at *a*, *b*, and *c*, disappeared entirely when the discharged portions of fluid had passed, and reappeared with a change of form and size when the operation was repeated.

In a specimen of topaz possessed by Major Playfair, and seen by many individuals, a white ball passed from one cavity to the edge of the specimen, as if projected from a mortar; but by the application of too strong a heat it was shattered in pieces.

In my first paper of 1823,* I have described and figured a phenomenon of an

* *Edinburgh Transactions*, vol. x. p. 11, Plate I. Fig. 5, 6.

analogous kind ; but as it appeared unexpectedly, and was instantly followed by the explosion of the crystal, I could neither observe it accurately, nor confirm what I did observe, by a repetition of the experiment. I have, therefore, some satisfaction in describing a similar phenomenon, seen frequently, and under more favourable circumstances, not only from its intrinsic interest, but because a distinguished philosopher had treated with an air of incredibility an observation which I had made of a similar kind. There can be no higher testimony to the novelty and importance of a scientific fact, than when a competent judge raises it to the supernatural.

I come now to describe a property of the dense fluid, so new and remarkable that it cannot fail to excite the attention of chemists. This fluid occupies the whole of a large cavity *A B C D E*, Fig. 10, with the exception of a bubble at *A*, which must be either a vacuum, as it is in all cavities containing only this fluid, or a bubble of the expansible fluid, or the vapour of the dense fluid, or some gaseous body. It cannot be a vacuum ; because it expands with heat, in place of being filled up by the expansion of the fluid. It cannot be the expansible fluid ; because cold would contract it, and produce a vacuity. It cannot be the vapour of the expansible fluid ; because there is no expansible fluid to throw it off, and it has not the optical properties of its vapour. It cannot be the vapour of the fluid in the cavity ; for it does not disappear by the application of cold, and does not become a vacuity, which fills up by the expansion of the fluid. It is, therefore, an independent gas, which exhibits the following phenomena.

When heat is applied, the bubble *A* expands, not by the degradation of its circular margin passing into vapour, as in the vapour cavities described in a former paper, but by the rapid enlargement of its area. When it attains a certain size, it throws off a secondary bubble *B*, which passes over a sort of ridge or weir *m n o*, in the bottom of the cavity, and settles at *B*. If the heat is continued, these two bubbles increase in size ; but it was instantly withdrawn when *B* had begun to swell. As the topaz began to cool, both the bubbles *A* and *B* quickly contracted. The primary bubble *A* returned gradually to its original condition, and *B*, when reduced to a single speck, would have disappeared, had the cooling not been stopped. This speck swelled again by the application of heat, and so did the bubble *A*. When the speck at *B* was allowed to vanish, which it did on the spot which the bubble occupied, the fresh application of heat did not revive it at that spot, but merely expanded the primary bubble *A*, which again threw off a secondary bubble *B*, which exhibited by heat and cold the same phenomena as before. These experiments I repeated many times with the same result. It will naturally be asked, what was the condition of the fluid itself which has the property of expanding by heat ; and what became of it while a part of the space which it occupied was appropriated by the bubble *B*, and the addition to the bubble *A* ? An accidental circumstance enables me to answer this question, which would have been

otherwise a very perplexing one. Having applied too strong a heat to the specimen, the bubble A threw off beside B two or three smaller ones, which moved along the upper edge A E. My attention having been thus directed to this part of the specimen, I was surprised to observe a great number of capillary lines or pipes P Q, rising from the edge A E of the cavity, and into which the fluid was forcing itself, oscillating in these minute tubes like the mercury in a barometer, and sometimes splitting the laminae between them. The force of cohesion, thus overcome by the expansive efforts of the fluid, predominated over the capillary attraction of the tubes and surfaces, and pressed back all the fluid into the cavity, when the body of fluid had contracted in cooling.

If we now consider the body which occupies the vacuity A as a gas, and, consequently, the other bubble B as the same, it follows, that the whole of the gas in B was absorbed by the fluid while cooling, and again given out by an increase of temperature. The gas, when in the act of being discharged, took its course to the locality of the speck at B, and to the bubble A; but to the bubble A alone when the speck had disappeared.

Upon repeating these observations the cavity burst; and I have now before me its two halves, forming its upper and its under surface. The portion of the cavity at A has the same depth as the portion below *mno*, all the rest of the cavity being much shallower. There was a fine doubly refracting crystal at M N, which polarised the blue of the second order; and its outline is still left on the cavity. There was a sort of crystalline powder disseminated round M N to a considerable distance, and the roof of the bubble B, when the roof of the cavity was entire, was always mottled with this powder.

In a former paper, I have distinguished vapour cavities from common cavities, by the manner in which the vacuity in the expansible fluid disappears. In the one case, the vacuity gradually enlarges by the degradation, as it were, of its margin, as the fluid passes into vapour; in the other, the vacuity gradually diminishes till it disappears. I have since found cavities of an intermediate character, in which the vacuity, on the first application of heat, diminishes, and then, when it has contracted to a certain size, it begins to expand; and its margin becoming thinner and thinner, it finally passes into vapour.

3. On the Form and Position of Crystals in the Cavities of Topaz.

In a former paper I have described a moveable group of crystals of carbonate of lime, which I discovered in a cavity in quartz from Quebec, containing a fluid with the properties of water. The crystals to which I am about to call attention, are of a very different kind, and possess a very different kind of interest.

The crystals which occupy the fluid cavities of topaz are either fixed or

moveable. Some of the fixed crystals are often beautifully crystallized. They have their axes of double refraction coincident with those of the crystal, and, as I have ascertained by the examination of exploded cavities, they actually form part of the solid topaz, though they exist in the fluid cavity. One or two of these are shewn in Fig. 4, Plate XIX., of my paper of 1826,* and they may be distinguished by their attachment to the sides of the cavity. In the same figure, as well as in Figs. 10, 13, 20, and 21 of my Paper of 1823,† I have drawn others which I then believed to be fixed, but which I have no doubt are moveable, and produced from one or other of the new fluids.

In re-examining my specimens of *topaz*, I have been surprised at the great number of cavities which contain crystals. In some there are only one; in very many there are two, three, and four; and in a great number of specimens the cavity is so crammed with them, like a purse full of money, that the circular vacuity has not room to take its natural shape, and often can scarcely be recognised, in its broken-down condition, among the jostling crystals.

The crystals of which I am treating are sometimes found in the volatile, and sometimes in the dense fluid, but chiefly in the latter. They are often found in an amorphous state in the narrow necks and narrow extremities of cavities, positions in which they remain fixed while they continue solid; and sometimes regularly formed crystals remain fixed between the prismatic edges of cavities, in consequence of having either fallen into that position, or of having been formed there.

The crystals in topaz cavities are, in general, beautifully crystallized, and have a great variety of forms. I have observed the following:—

1. The Tetrahedron.
2. The Cube.
3. The Cube, truncated on its edges and angles.
4. The Rhombohedron.
5. The Prism, with plain and pyramidal summits.
6. The Flat Octohedron, truncated on its edges and angles.
7. Rhomboidal Plates.
8. Hexagonal Plates.
9. Long rectangular Plates.

Besides these, there are amorphous crystals and crystallized masses of various characters.

4. *On the Physical Properties of the Crystals in Topaz Cavities.*

Although it would be desirable to submit these crystals, as well as the fluids which contain them, to chemical analysis, yet the task is too difficult to be ac-

* *Edinburgh Transactions*, vol. x.

† *Ibid.*, Plates I. and II.

complished in the present state of chemical science. I must, therefore, limit my observations to such of the physical properties of these crystals as can be rendered visible to the eye.

When I first applied heat to the crystals under consideration, I employed a very fine specimen, with large and numerous crystallized cavities, of a prismatical form, containing both the new fluids. In this specimen, there were seven cavities unlike all the rest, and each of them containing a single crystal, and apparently but one fluid, namely, the dense one. The cavities were exceedingly flat, and irregular in their shape, and very unlike one another. Upon applying the heat of only a lighted paper match beneath the plate of glass on which the specimen lay, I was surprised to see the crystals gradually lose their angles, and then slowly melt, till not a trace of them was visible. In this state, one of the cavities had the appearance shewn in Fig. 11, where V was the vacuity, and *v*, *v'*, other two bubbles, one of which *v* soon joined the principal one V. In all the other six cavities, the crystals were speedily reproduced, always at the point where they disappeared, provided a small speck remained unmelted; but otherwise in different parts of the cavity. In the cavity A B, however, Fig. 11, the crystal was very long in appearing. In the course of an hour, however, a fasciculus of minute crystals appeared in the centre of the vacuity, as in Fig. 12, and to them the principal crystal attached itself, as in Fig. 13, which exhibits a perfect rhomboidal plate, truncated on its obtuse angles. The elliptical vacuity was pressed into the shape of a heart; and, by the application of ice, I succeeded in precipitating the vapour of the expansible fluid, which existed in a very minute quantity in all the seven cavities. The expansible fluid is shewn between the two heart-shaped outlines in the figure, and I repeatedly threw it into vapour, and reduced that vapour to a fluid state. The phenomenon now described, of the melting of the crystals, and their subsequent re-crystallization, I have shewn to various persons; and it is very remarkable that they generally reappear in this specimen of the same form, though with considerable modifications.

Upon applying heat to other cavities, containing several crystals, I obtained very different results. Some of them melted easily, others with greater difficulty; and some were not in the slightest degree affected by the most powerful heat I could apply. When the crystals melted easily, they were as quickly reproduced; sometimes reappearing more perfectly formed than before, but frequently running into amorphous and granular crystallizations.

In some specimens of topaz, all the crystals in the cavities refuse to melt with heat, and seem not to suffer the slightest change in their form. Hence we are entitled to conclude, that the crystals possessing such different properties must be different substances; and this conclusion is amply confirmed by an examination of their optical properties.

In making this examination, I used a polarising microscope, so constructed

that the plane, passing through the optical axis of the topaz, could be readily placed either parallel or perpendicular to the plane of primitive polarisation. In this case, the field of the microscope is wholly obscure, in so far as the depolarising action of the plate of topaz is concerned; but if there is any crystal in the topaz, either imbedded in its mass, or included in its cavities, that crystal will exhibit its doubly refracting structure, if it has any, by its depolarising action. It may, indeed, happen,—and it does happen,—that the plane passing through their optical axes coincides, either accurately, or so nearly, with that of the topaz, that its depolarising action is a minimum; but an experienced observer will have no difficulty in distinguishing this want of depolarisation by position, from the want of it by structure.

When the specimen of topaz is rich in cavities full of crystals, the display of luminous and coloured crystalline forms in the dark field of the microscope, indicating, too, the imprisonment of fluids, and the condensation of gases before vegetable or animal life had visited our primeval globe, was as interesting to the imagination and the judgment as it was beautiful to the eye. Having had the privilege of being the first to see it, I felt the full influence of the sight; and I have again and again contemplated it with renewed wonder and delight. When the cavities are so numerous as to mock calculation, and so infinitely small as to yield no visible outline to the highest powers, the bright twinkle of a crystalline atom within them reveals to us their nature as well as their contents.

In the examination of the individual crystals, many interesting facts present themselves to our notice. The crystals of the tessular class, which are modifications of the cube, are very numerous, and have no action upon polarised light. Many of them melt easily, while others refuse to yield to the action of heat; and hence, there must be two different substances in the cavities which assume the same shape. In like manner, some of the doubly refracting crystals melt readily, others with very great difficulty, and others not at all; so that there must be *three* different substances, which belong to the classes of forms that give double refraction; a conclusion which is confirmed by the different secondary forms which I have already enumerated.

I have seldom found any crystals in these cavities which depolarise white light, or the highest order of colours. I have found some that depolarise *four* orders of colours; and when the crystal which does this is a flat hexagonal plate, it is highly interesting to see it pass through all the tints which these orders include, while slowly melting, and again reproducing them during its re-crystallization.

In a cavity which was so placed as to be entirely black from the total reflection of the light which fell upon it, I observed three *white* openings, *a, b, c*, of a crystalline form, see Fig. 14. These appeared to be fixed crystals, or rather parts of the topaz, surrounded by a cavity. I found, however, that the hexagonal one

C depolarised white light, while the rest had no action upon polarised light. Upon applying heat, the crystal *c* melted, and took up a position at *c* Fig. 15, in a narrower part of the cavity, where it remains of an irregular form, having been repeatedly melted and re-crystallized. Upon turning the cavity into a position where it became transparent, I found that there was no fluid whatever in the cavity; so that we have here an example of a crystal melting and re-crystallizing without having been dissolved in one of the fluids. From the irregular state of the laminæ close to this cavity, there is every appearance of the fluids having escaped from one of its extremities.

In the course of these observations, I observed a phenomenon, produced by heat, of the most novel and surprising kind, and one which I feel myself utterly unable to explain. It presented itself when I was studying the very interesting collection of crystals in the cavity A B, Fig. 16. This cavity is filled with the dense fluid, in which there is a vacuity V: the fluid swells to such a degree with heat as to diminish very perceptibly the size of this vacuity; and as I can find no trace of any portion of the volatile fluid, I have no doubt that this vacuity would disappear by an increased degree of heat. The fear, however, of bursting so rare and interesting a cavity, has prevented me from making this experiment. The cavity contains a great number of crystals of different forms, not one of which melts with heat, and almost all of which possess double refraction. When I first submitted this cavity to the microscope, there were *five* small crystals lying between D and the vacuity V; one a flat prism, another a hexagonal plate, a third amorphous, and a fourth and fifth two irregular halves of a hexagon. Upon the first application of heat, one or two of these crystals leapt from their resting place, and darted to the opposite side of the cavity. In a few seconds, the others quitted their places one after another, performing the most rapid and extraordinary rotations. One crystal joined another, and, at last, four of them thus united revolved with such rapidity as completely to efface their respective shapes. They then separated on the withdrawal of the heat, and took the position which their gravity assigned them. On another occasion, a long flat prism performed the same rotation round its middle point; and I have repeated the experiment so often, in shewing it to others, that the small crystals have been driven between the inclined edges of the cavity, from which I cannot extricate them. I have succeeded, however, in conducting a fine octohedral crystal, truncated on its edges and angles, into the arena at D, where I have just seen it perform its rotation, as indicated by the concentric circles on the right hand of D.

In seeking for the cause of so extraordinary a phenomenon, we are reminded of the rotations of camphor and other volatile substances; but, in this case, no gas or matter of any kind could be thrown off without becoming visible in the fluid. The pyro-electricity of topaz next suggests itself as a moving power; but though it might produce attractions and repulsions, we cannot see how it could

turn a crystal upon its axis. The experiments of Libri and Fresnel, on the repulsions which heated bodies exert upon each other at sensible distances, afford us as little aid. They may enable us to account for the mere displacement of the crystals by the application of heat, or for their sudden start from their places of rest, but they do not supply us with a force fitted to give and to sustain a rapid rotatory movement.

I have already had occasion to state, that the cavities often burst when too much heat is applied to the specimen. This generally takes place by a separation of the laminae, which fly off in splinters; but when the burst cavity is large and insulated, a piece of the solid crystal is scooped out on its weakest side. Sometimes a great number of cavities explode at the same time, and when they are small, or exist in a part of the crystal where there are no large ones, the explosive force is not strong enough to separate the laminae. The fluid is merely driven between the laminae to a small distance around the cavity, and shews itself as a dark brown powdery matter, encircling the cavity as the burr of a comet does its nucleus. When the cohesion of the laminae is great, it resists the explosive force over a large cavity, and the contents of the cavity are thrown to a considerable distance around it, and remains between the laminae, either as a sort of powder, or as a congeries of minute crystals, which are sometimes large enough to shew their depolarising action. When the laminae separate, we find this crystalline matter either fluid or indurated; exhibiting, when fluid, the extraordinary properties described in my former papers. If we breathe upon the indurated matter it becomes fluid, re-crystallizes in new spiculae and crystals; and, on several occasions, I have found fine examples of circular crystallization.

After the explosion of cavities containing only the dense fluid, I have been surprised to find, and that in large cavities, that no trace of matter was left upon the sides of the cavity or around it. Whether this arose, as the fact seems to indicate, from the dense fluid being a condensed gas, or from some other cause, it will require new experiments to determine.

In a very remarkable specimen, in which the cleavage plane passed through a great number of large flat cavities, the brown matter has been lodged near to the edges of each cavity, and marks them them out even to the unassisted eye. These cavities were filled almost solely with the volatile fluid; and since the faces of the cavities are corroded as if by the action of a solvent, developing crystalline forms, there is reason to think that the fluid has exercised this action, and that the phenomenon is analogous to that external action, on the faces of hundreds of Brazil topazes in my possession, which I have described in the *Cambridge Transactions*,* and the singular optical figure formed by which, I have represented in a late volume of the *Transactions of this Society*.†

* *Cambridge Transactions*, vol. ii. Plate i. fig. 15.

† *Edinburgh Transactions*, vol. xiv. Plate x., fig. 1, 2.

The only chemical experiment on the contents of these cavities, which I have had occasion recently to make, is perhaps worth reporting. One angle of a cavity was blown off by its explosion, and though the fluids escaped, a pretty large prismatic crystal remained within the cavity. I introduced *water* and *alcohol* successively into the cavity, and raised them to a considerable heat; but they had no effect in dissolving the crystal.

5. *On Solid Crystals and Crystalline Masses imbedded in Topaz.*

Among the new phenomena which this section embraces, there is at least one intimately connected with the subject of the fluid cavities. How far the other phenomena may have any such connexion, it remains to be seen.

The imbedded crystals to which I refer, presented themselves to me while the specimens which contain them were exposed to polarised light. Mineralogists have been long familiar with the beautiful crystals of Titanium, imbedded in quartz, and I have found the same mineral imbedded under still more interesting circumstances in the Brazilian amethysts.

In topaz, however, the imbedded crystals have never been noticed, and I have fortunately obtained specimens, in which they are displayed with singular beauty. Their axes of double refraction are not coincident with those of the topaz; and hence they are seen in the obscure field of the microscope splendid with all the colours of polarised light. These crystals are equally transparent with the topaz, with a few slight exceptions. They sometimes polarise five or six orders of colours; and, in general, they have very beautiful crystalline forms, which can be seen by the microscope in common light. In some cases, they are mere crystalline masses, often of a reniform shape, but still with regular axes of double refraction.

In some specimens of Brazil topaz, the crystals occur in branches or groups of singular beauty, consisting of prisms and hexagonal plates, connected apparently by filaments of some opaque matter.

I have, occasionally, met with another interesting variety of them, which *have no visible outline* by common light, and which could never have been detected but by the polarising microscope. In one of these cases, the crystalline mass, which is nearly spherical, lies in a crowded group of small fluid cavities, none of which enters its mass; a complete proof that the cavities were formed in the soft mass of topaz, when it encircled the indurated crystal.

Along with these interesting phenomena, another occasionally occurs, which may still require a farther examination. I have observed *apparent* doubly refracting crystals, which differ in some essential points from those which have been described. They depolarise a uniform, or nearly a uniform tint, notwithstanding the different thicknesses through which the polarised light passes; and

that tint is less brilliant than in the real imbedded crystals. I conceive, therefore, that they are crystallized cavities, having their inner surfaces coated with a doubly refracting crust. This is, in itself, a very natural supposition, seeing that the fluid may have discharged its gaseous portion, and left behind it the matters which it held in solution. The cavities, however, of this kind, which I have described in a former paper, have no depolarising action; and I find that those now under consideration have regular axes of double refraction. Hence, the matter which covers them must be a regular crystalline shell, with optical and crystallographic axes—a phenomenon which has no parallel in mineralogy.

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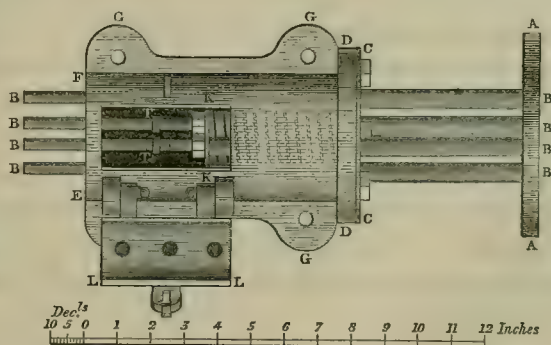
February 15. 1845.

IV.—*Account of Experiments upon the Force of the Waves of the Atlantic and German Oceans.* By THOMAS STEVENSON, Civil-Engineer, Edinburgh. Communicated by DAVID STEVENSON, Esq.

In forming designs of marine works, the engineer has always a difficulty in estimating the force of the waves with which he has to contend. The information on such a matter, which is derived from local informants, who, although intelligent in the departments of trade which they follow, are, nevertheless, more or less prejudiced from being constantly on the spot, is not satisfactory; and it has, therefore, often occurred to me that it would be most desirable if the engineer could be enabled, to some extent at least, to disregard the prejudiced statements of others, and the vague impressions left by them on his own mind, and really to ascertain, by direct experiment, what force, expressed in pounds per square foot, the sea actually exerts upon the shores where his buildings are proposed to be erected.

Notwithstanding the want of all direct experiments* on this subject, and the somewhat unpromising nature of such an enquiry, I was, nevertheless, induced to attempt the construction of an instrument to effect the desired end; and after several fruitless devices had been put to the test, I at length succeeded in forming one whose indications I hope to be able to shew are trustworthy. Before considering the results obtained, however, I shall explain the construction of this simple self-registering instrument.

The letters D E F D represent a cast-iron cylinder, which is firmly bolted at the projecting flanges G to the rock where the experiments are wanted. This cylinder has a flange at D D. L L is a door, which is opened when the observation is to be read off. A A is of iron, and forms a circular plate or disc, on which the sea impinges. Fastened to the disc are four guide-rods B B B B. These rods pass through a circular plate C C (which is screwed down to the flange D D), and also through holes in the bottom E F. Within the



* Sir S. BROWN has indeed stated, that at Brighton he found the impetus of the waves during heavy gales was "equal to 80 lb. to a foot upon a cylindrical column of 12 inches diameter." The hydrostatical pressure of a wave only $1\frac{1}{2}$ foot high is equal to 80 lb. upon a square foot.

cylinder there is attached to the plate C C a powerful steel spring, to the other or free end of which is fastened the small circular plate K K, which again is secured to the guide-rods B B B B. There are also rings of leather T T, that slide on the guide-rods, and serve as indices for registering how far the rods are pushed through the holes in the bottom ; or, in other words, how much the spring has been drawn out or lengthened by the force of the sea acting upon the plate or disc A A. The object of having four leathern rings, where one might have answered the purpose, was merely that they might serve as a check upon each other ; and so perfectly did they answer the purpose intended, that in every instance they were found equidistant from the bottom of the cylinder ; proving thereby, that, after the recoil of the spring, they had all kept their places. The guide-rods are graduated, so as to enable the observer to note exactly the quantity that the spring has yielded.*

This instrument, which may, perhaps, be not improperly termed a *Marine Dynamometer*, is, therefore, a self-registering apparatus which indicates the maximum force of the waves. In the graduation of the instrument, the power of the spring is ascertained by carefully loading the disc with weights, so that when the quantity that the spring has yielded by the action of the sea is known, the pressure due to the area of the disc exposed is known also. The discs employed were from 3 to 9 inches diameter, but generally 6 inches, and the powers of the springs varied from about 10 lb. to about 50 lb. for every $\frac{1}{8}$ inch of elongation. Their respective effects were afterwards reduced to a value per square foot. The instrument was generally placed so as to be immersed at about three-fourths tide, and in such situations as would afford a considerable depth of water. It is not desirable to have the instrument placed at a much lower level, as it has not unfrequently happened during a gale, that for days together no one could approach it to read off the result and readjust the indices to zero. It must, however, at the same time be remarked, that it is in most situations almost impossible to receive the force unimpaired, as the waves are more or less broken by hidden rocks or shoal ground before they reach the instrument.

In connection with the apparatus above described, a graduated pole was erected on an outlying sunken rock, for the purpose of ascertaining the height of the waves ; but the observations were not of so satisfactory a nature as could have been desired, and the poles soon worked loose from their attachments, and disappeared.

With the instrument which has been explained, I entered upon the following train of observations :—

* It has been suggested to me, that the indications of the instrument might be made through the medium of a flexible wire or chain at a considerable distance from the instrument, and thus the impulse of every wave might be observed.

In 1842 several observations were made on the waves of the Irish Sea at the island of Little Ross, lying off the Bay of Kirkcudbright. Since April 1843 till now, continued observations have been made on the Atlantic at the Skerryvore and neighbouring rocks, lying off the island of Tyree, Argyllshire. And in 1844 a series of similar observations was begun on the German Ocean at the Bell Rock. It will be seen, that in selecting these localities a varied exposure has been embraced, comprising the comparatively sheltered Irish Sea, the more exposed eastern shore of Scotland, and the wild rocks of Skerryvore, which are open to the full fury of the Atlantic, the far distant shores of North America being the nearest land on the west.

Referring for more full information to the tables of experiments which are given at the end of this paper, it will be sufficient in this place to state generally the following, as the results obtained.

In the *Atlantic Ocean*, according to the observations made at the Skerryvore rocks, the average of results for five of the *summer* months during the years 1843 and 1844, is 611 lb. per square foot. The average results for six of the *winter* months (1843 and 1844), is 2086 lb. per square foot, or thrice as great as in the summer months.

The *Greatest result* yet obtained at Skerryvore was during the heavy westerly gale of 29th March 1845, when a pressure of 6083 lb. per square foot was registered. The next highest is 5323 lb.

In the *German Ocean*, according to the observations made at the Bell Rock, the greatest result yet obtained is 3013 lb. per square foot.

It thus appears, that the greatest effect of the sea, which has been observed, is that of the Atlantic at Skerryvore, which is nearly equal to three tons per square foot.

These experiments, amounting to 267 in number,* and on the Atlantic alone extending over 23 months continuously, are not intended to prove any thing farther than the simple fact, that the sea has been known to exert a force equivalent to a pressure of three tons per square foot, however much more. Now, when we consider that the hydrostatic pressure due to a wave of 20 feet high, is no more than about half a ton on a square foot, we see how much of their force the waves owe to their velocity. There can be no doubt, however, that results higher than this will be obtained. Were a train of observations made at various points of the coast, the result would not only be highly useful in practice, as they would by reference to existing marine works shew what sizes of stones and proportions of piers were able to resist seas of a given force; but they would form an interesting collection of information with regard to the relative forces of the waves in our contracted bays and estuaries, as compared with those ob-

* It was not thought necessary to give all the observations in the table appended to this paper.

served in the ocean; and would thus supply the want which, as already stated, all engineers labour under, to a greater or less degree, in designing marine works.

It is proper, however, to observe, that there may be some objection to referring the action of the sea to a statical value. Although the instrument might perhaps be made capable of giving a dynamical result, it was considered unnecessary, in these preliminary experiments, to do any thing more than represent the maximum pressure registered by the spring, because the effects of the waves may, from supposing them to have continuity of action, be perhaps regarded as similar to a statical pressure, rather than to the impact of a hard body.* The near coincidence, or indeed almost perfect agreement of the results of the experiments made with different instruments, goes far to shew that the waves act in very much the same manner as a pressure, although both pressure and impact must obviously enter into their effect. In the experiments, begun February 1844, and given at the end of the paper, the three instruments had not only different areas of discs, but very different powers of springs, and yet the results were almost identical. Now, the same force, supposing the waves to act like the impact of a hard body, would, in the Marine Dynamometer, assume very different statical values, according to the spaces in which that force was expended or developed; so that with the same force of impact, the indication of a weak spring would be less than that of a stronger.

In future experiments it may be interesting, however, to test the springs dynamically, by means of the impact of a heavy body dropped from a given height upon the plate or disc of the instrument. In some experiments lately made in this way, by dropping a cannon-ball upon the disc, it appeared, that, within the limits of the experiments, there was for each individual spring a ratio between the value registered by the leathern index and the calculated momentum of the impinging body. These ratios were, of course, found to vary in springs of different power, and to be constant only for springs of the same power. Did the waves, therefore, act by a sudden finite impact, like the cannon-ball employed in this instance, we could scarcely have found such harmony between the results of instruments with different springs, as the experiments alluded to afford. At the same time, the result cannot, perhaps, be in strictness considered correct; but, from the elongation of the spring being very small, the results may be regarded as practically correct,—the more so when we find so remarkable a coincidence of results as that alluded to.

* With reference to the continuous action of water, I may notice the effects produced by the failure of Beith's Dam, a reservoir situated upon the high grounds near Carlsdyke, immediately east of Greenock. This dam had a head of 20 feet of water, and gave way on the night of the 21st November 1835, when the water, after breaking down another reservoir below it, rushed through the streets of Carlsdyke, causing the melancholy loss of no fewer than 41 lives. This continuous flow of water carried away many houses; and, among other instances of its power, it is recorded that a "mass of rock about 16 tons weight was borne along by the torrent to a distance of 30 or 40 yards." This case, then, which almost equals the records of the fury of the sea, shews the effects which continuous action may produce.

I shall now contrast the indications of the Marine Dynamometer by stating a few facts regarding the ascertained effect of the waves in the elevation of spray, and in the transportation of heavy masses of rock. This is more especially important, as to some, the results indicated by the instrument have appeared greater than they could have expected; and it has even been supposed that, were they correct, the stones which constitute our marine works would be scattered. Before passing from this point, it may be well to observe that the stones composing sea-works, are not only wedged and compacted together, but they derive from the superincumbent courses, (independently of the support afforded by the *backing*), a pressure so great as to cause an amount of friction which is in most cases greatly more than sufficient to preserve them in their places.

But to return to the facts of the ascertained effects of the waves, it may be interesting, in the first instance, to give some idea of what may be looked for in comparatively small expanses of water, such, for instance, as the lakes of North America, which, however, exhibit during gales of wind, all the characteristics of an open sea. In the north-eastern corner of Lake Erie, the harbour of Buffalo was constructed at a cost of about £40,000. It is mentioned in the "Civil-Engineering of North America," that the author "measured (at this harbour) several stones which had been moved; and one of the largest of them, weighing upwards of half-a-ton, had been completely turned over, and lay with its bed or lower side uppermost."

In the Firth of Forth, at the Granton Pier works, on 19th December 1836, after a gale from the north-east, one stone was moved measuring fifteen cubic feet, or about one ton in weight, and thrown on the beach, after having been built into the wall; and a stone containing 18 cubic feet was moved 30 feet from its place; while the *pierres perdues* or mound-stones were washed down to a slope of about 4 to 1.

The following instance, which occurred at the landing slip of the Calf Point, Isle of Man, affords a proof of the great force of the waves even in the Irish Sea. During a gale from the north-west, a block was lifted from its place in the wall and thrown landwards, which measured $123\frac{1}{2}$ cubic feet, equal to about 10 tons weight.

In the German Ocean, we can refer to the Bell Rock Lighthouse,* which, though 112 feet in height, is literally buried in foam and spray to the very top, during ground swells, when there is no wind. It is, therefore, a very important station for making such experiments, because the rise of the spray may be regarded as a scale by which the results of the Marine Dynamometer can be checked or compared.

* At such a situation as the Bell Rock, a column of water or of air could be conducted into the interior of the house, and might, in the one case, shew the force of each wave as it struck the building by the rise of the water column; or, in the other, by a pressure-gauge, shew the same result in atmospheres by compression.

In the published account of this work there occurs the following statement :— On the 24th October 1819, the spray rose to the height of 105 feet above the rock. “ It may, perhaps, therefore,” says the author, “ be concluded, that the maximum force of the sea at the Bell Rock is to raise the sprays to the height of about 105 feet above the surface of the rock ;” and deducting 16 feet, which is the height that the tide rises upon the tower, there is left 89 feet, as the height to which the water is raised. This is equivalent to a hydrostatic pressure of about $2\frac{1}{2}$ tons on the square foot. Since that time, however, there have been still greater proofs of the force of elevation. On the 20th November 1827, the spray rose 117 feet above the foundations or low water mark ; and the tide on that day rose 11 feet upon the tower, leaving 106 feet as the height of elevation (exclusive of the trough of the sea). being *equivalent to a pressure of very nearly 3 tons per square foot.*

At the island called Barrahead, one of the Hebrides, a remarkable example occurred during a storm in January 1836, in the movement of a block of stone, which, from measurements taken on the spot, is 9 feet \times 8 feet \times 7 feet = 504 cubic feet, which, allowing 12 feet of this gneiss rock to the ton, will be about 42 tons weight. This great mass was gradually moved 5 feet from the place where it lay, having been rocked to and fro by the waves till a piece broke off, which rolling down, and jamming itself between the moving mass and the shelving rock on which it rested, immediately stopped the oscillatory motion, and thus prevented the farther advance of the stone.

Mr REID, the principal keeper of Barrahead Lighthouse, the assistant keeper, and all the inhabitants of the little island, were *eye-witnesses* of this curious exhibition of the force of the waves ; and Mr REID also gives the following description of the manner in which they acted upon the stone.

“ The sea,” he says, “ when I saw it striking the stone, would wholly immerse or bury it out of sight, and the run extended up to the grass line above it, making a *perpendicular* rise of from 39 to 40 feet above the high water level. On the incoming waves striking the stone, we could see this monstrous mass of upwards of forty tons weight lean landwards, and the back run would uplift it again with a jerk, leaving it with very little water about it, when the next incoming wave made it recline again. We did not credit the former inhabitants of the island, who remarked that the sea would reach the storehouse which we were building ; and when these stones were said to have been moved it was treated with no credit, and was declared by all the workmen at the lighthouse works to be impossible ; yet the natives affirmed it to be so, and said if we were long here we might yet see it. They seemed to feel a kind of triumph when they called me to see it on the day of this great storm.”

Having now detailed the various observations and facts of which I was possessed in relation to this subject, it may be necessary, in conclusion, to consider the general bearing of such an inquiry.

The advantages which may ultimately arise from a knowledge of the energies of the ocean, can only be guessed at in the present state of our information. It is not to be expected that, in the present train of experiments, much will be found that is directly valuable in practice, as time is required before a true maximum result can be discovered. But a very close and promising connection may easily be traced between the present inquiry and the principles of Hydraulic Architecture, as illustrated in the construction of breakwaters, sea-walls, lighthouses, and piers of timber or of stone, and in the calculations for the strength of the booms which are employed for excluding waves from the interior of harbours; also, in trying the power of waterfalls, and in contrasting the action of waves at the surface with that at the bottom, or at various depths along the sea slopes of breakwaters.

Theoretically, there is much also to invite to a prosecution of such observations. In connection with researches so successfully prosecuted by Mr SCOTT RUSSELL in the Mechanism of Oceanic Waves, their height, their velocity, and their distance apart, surely observations on the development of the gradually acquired force of such undulations, when they become waves of translation, will form a very important feature in *Marine Mechanics*. In the science of geology, the most direct bearing of the results of the Marine Dynamometer is on the subject of erratic boulders. It is no easy problem to account for the presence of enormous boulders which are foreigners to the formation where they lie, and often, also, far distant from the formation to which they belong. Accordingly we find that glacial action has been suggested as the cause of transportation. Mr MILNE has, in the Transactions of this Society, suggested that a continuous rush of waters, due to volcanic emersion, might, at any rate, account for the distribution of the largest erratic boulders which are to be found in Roxburghshire. The results of the Marine Dynamometer, and the facts above recorded of the action of different bodies of water, will certainly be admitted to go far in proof of the competency of aqueous action, to effect the distribution of the erratic blocks referred to by Mr MILNE.

EXPERIMENTS.—With reference to the following experiments I have only to observe, that those which were made at Little Ross, upon the Irish Sea, cannot, from the unusual fineness of the weather at the time, be regarded as affording a true value of the effects of a hard gale in these seas. Of the others it is to be noticed, that where two or three instruments were for some time employed as a check upon each other, and only one or two readings are given, the want has occurred either from the instruments being under repair, or being difficult of access in stormy weather, or during neap tides. It often happened also, in consequence of the springs proving too weak, when new ones had to be made, or the area of the disc reduced. Registers of the state of the weather, apparent

height of spray, &c. were generally kept; but it was not considered necessary to complicate the Tables by inserting these, excepting in one or two instances.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
Observations at Little Ross.				1844.			
1842.		1842.		Feb. 3	429	April 19	800
April 25	15	June 25	458.0	457	535
... 28	51	July 25	380.0	... 13	214	481
June 1	36	Aug. 2	570.0	228	... 22	913
... 4	81.5	... 5	665.0	... 15	321	428
... 20	86.5	... 6	380.0	280	962
... 24	840.0	... 9	380.0	321	... 24	1942
The Observations at the Skerryvore Rock, and the neighbouring Island of Tyree, distant 13 miles from the Skerryvore, are as follows:—				... 16	428	1604
1843.		1843.		402	1370
April 24	455	Aug. 9	346	343	... 25	1283
... 25	707	... 22	723	1284	343
May 7	243	... 30	389	1364	321
... 11	182	Sept. 5	866	685	... 27	457
... 12	243	... 21	952	2032	481
... 16	364	Oct. 5	1535	2086	Night tide 800
... 20	{ 495 476	... 6	1606	399	
June 3	182	Nov. 18	1711	... 27	321	642
... 4	519	... 19	1497	321	... 30	229
... 7	428	... 27	1497	342	241
... 8	855	... 29	2353	March 4	3316	May 15	343
... 9	173	Dec. 5	2674	3369	... 14	481
July 2	476	... 8	{ 3421 At least	3427	June 6	571
... 3	{ 723 866	... 14	2460	... 7	1069	... 15	1828
... 30	433	... 26	1947	963	July 11	1028
In January, two instruments were placed beside each other, but not set parallel. These instruments had springs of different power, the one being about double that of the other, and one had a disc of 3 inches diameter, the other 6 inches.				913	... 13	457
1844.		1844.		... 10	1925	... 18	914
Jan. 6	962	Jan. 9	1925	1925	... 23	1532
... ..	928	1000	1713	... 25	571
... 7	2353	... 10	826	... 11	535	... 26	971
... ..	857	1000	481	... 27	457
Both instruments set parallel.				456	... 28	1142
1844.		1844.		... 12	3316	... 29	256
Jan. 16	428	Jan. 16	427	4011	... 30	914
Another instrument was placed beside them, but the two marked thus * were found to be too weak, as the leathers were found flattened, and one of the instruments was broken, and was not repaired till the 15th February.				2970	... 31	1028
1844.		1844.		... 13	1142	Aug. 1	571
Jan. 28	3422*	Feb. 2	429	1283	... 7	914
... ..	2285*	457	1283	... 13	914
... ..	3313			April 10	457	... 14	914
				428	... 21	800
				481	... 30	1713
				... 11	800	Sept. 12	1028
				... 12	343	... 14	914
				321	... 20	457
				... 14	571	... 23	1142
				535	... 25	685
				... 16	571	... 30	1599
				642	Oct. 2	2399
				481	... 3	1485
				... 17	800	... 4	1828
				856	... 11	3427 ¹
				962	... 14	1599
				... 18	571	... 19	1599
				481	... 20	2513

¹ On this occasion, 14 stones were slightly moved, and 14 scattered, all of which had been built into the round-head or end of Hynish Pier, which was still in an unfinished state, and a Dynamometer which was attached to the Pier, registered on this occasion 2557 lb. These stones weighed from 1 to 1½ tons, and exposed, when built into the wall, about 2 square feet of surface. The stone to which the instrument was fixed was turned upside down, although it weighed about 1½ ton = 2800 lb.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1844.		1844.		1844.	
Oct. 22	800	Nov. 10	1028	Nov. 23	3427	Dec. 10	1825
... 24	1827	... 14	1257	... 27	3199	... 13	1925
... 28	1485	... 15	2056	... 28	4112	... 14	1027
... 29	457	... 16	2056	Dec. 7	1369	... 15	1764
Nov. 2	1942	.. 22	2627	... 9	2738		

A more exposed point of the Skerryvore Rock was at this time chosen for experiment; and with the view of ascertaining the effect of the waves at different heights upon the rock, two instruments were fixed, the one (No I.) several feet lower, and about 40 feet seaward of the other (No. II.) It was observed, that about half-flood the force of the waves was a good deal expended before they reached the place where No. I. was placed, from there being so little water on the rocks outside. Whereas when the tide was higher the waves were, from the greater depth of water, not so much broken when they reached No. II. The results of the Marine Dynamometer shew generally about twice the force at No. II. as at No. I.; a result which shews how important it would be to ascertain the relative forces of the waves at different levels upon our breakwaters and other seaworks.

Date.	Remarks.	No. of Instrument.	Pressure in lb. per Square Foot.
1845.			
Jan. 7	Heavy sea.	I.	1714
...	II.	4182
... 12	Very heavy swell.	I.	2856
...	II.	5032
... 16	Heavy ground swell.	I.	2856
...	II.	4752
... 22	A good deal of sea.	I.	2856
...	II.	5323
... 28	Heavy ground swell.	I.	2627
...	II.	4562
Feb. 5	Fresh gales.	I.	856
...	II.	3042
... 21	...	I.	1827
...	II.	3422
... 24	Fresh breezes.	I.	1256
...	II.	3802
March 9	Ground swell.	I.	1256
... ..	{ Waves supposed about 10 feet high.	II.	3041
... 11	Short sea.	I.	1028
... 24	Heavy sea.	I.	2281
... ..	{ Waves supposed about 20 feet high.	II.	4562
... 26	Swell.	I.	1256
... ..	{ Waves about 6 feet high.	II.	3041
... 29	{ Strong gale, with heavy sea, the highest waves supposed 20 feet high, and the spray rose about 70 feet.	I.	2856
		II.	6083

Register of Observations on the force of the Sea, made at the Bell Rock, German Ocean.

Dates.	lbs. to a Square Foot.	Dates.	lbs. to a Square Foot.
1844.		1845.	
Sept. 15	853	Jan. 27	1199
... 20	2260	... 30	2379
Oct. 9	3013	... 31	1559
... ..	2562	Feb. 6	2999
... 26	1142	.. 24	1199
... 27	953	... 25	959
Nov. 12	1680	... 27	839
... 13	1920	... 28	1319
Dec. 13	1560	March 4	959
... 26	1439	... 7	1079
		... 11	1919
1845.		... 20	2519
Jan. 7	1559	... 21	2759
... 10	1439	... 24	1319
... 11	1439	... 25	959
... 15	1559	... 28	599
... 25	959	... 30	1079
... 26	719		





GEOLOGICAL MAP

OF

Cockburn Law and the District North of Dunse

BERWICKSHIRE

Scale. 2½ inches per Mile.

Scale of colours see below.

Lines of Fault

Directions of Strikes.

Changes

Directions of Dip



Sections illustrative of the Geology of Cockburnlaw and the adjoining District (Berwickshire).

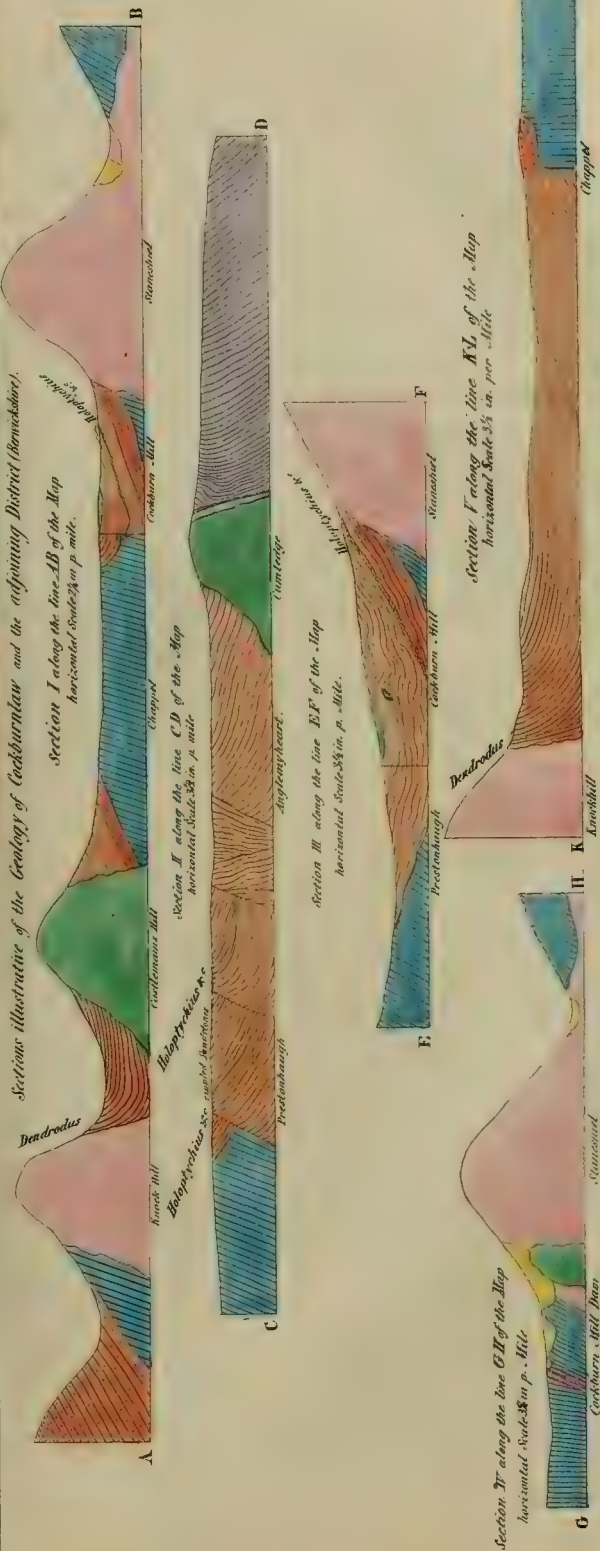
Section I along the line AB of the Map
horizontal Scale 3½ in p. mile.

Section II along the line CD of the Map
horizontal Scale 3½ in p. mile

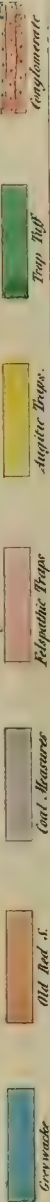
Section III along the line EF of the Map
horizontal Scale 3½ in p. mile.

Section IV along the line GH of the Map
horizontal Scale 3½ in p. mile

Section V along the line IJ of the Map
horizontal Scale 3½ in p. mile



Scale of Colours.





V.—*On the Geology of Cockburnlaw, and the adjoining District, in Berwickshire ; with a Map and Sections.* By WILLIAM STEVENSON, Dunse.

The portion of Berwickshire, of which the object of this paper is to describe the more remarkable geological features, comprises, within a very limited space, an assemblage of phenomena of the highest interest, in relation to the wonderful changes which this part of our island has undergone in the earlier periods of the earth's history. Its situation is immediately to the north of Dunse, and its area is about 16 square miles, within which are comprehended the junctions of the greywacke rocks of the Lammermuirs, with the strata of the old red sandstone formation, and of the latter with the lower members of the coal measures. The chief eminences within the district are Cockburnlaw, which attains a height of 912 feet above the level of the sea ; Dunselaw, 630 feet ; the Knock, Borthwick, and Castlemains Hills ; and the Staneshiel, a hill of the same formation as Cockburnlaw, from which it is separated by a ravine of 300 or 400 feet deep, in which the Whiteadder flows. The general topographical features of the district will be best understood by reference to the accompanying map.

In treating of our subject, we propose to describe, 1st, The *stratified* rocks of the district ; and, 2dly, The *igneous* rocks, and the changes of structure and position which have been effected by their agency upon those of aqueous origin.

I. STRATIFIED ROCKS.

1. *The Greywacke of the Lammermuirs.*

The strata of this important series of deposits (the most ancient of the district under consideration) are finely exposed in many localities, consisting of arenaceous and argillaceous beds, of great, but as yet, unascertained thickness, the former constituting the *greywacke* proper, and the latter the *greywacke slate*. The oxides of iron and manganese are widely diffused among these strata. Magnesia enters pretty largely into their composition ; but there seems to be an almost entire absence of calcareous matter, there being no beds of even impure limestone interstratified with the greywacke, and but a small quantity diffused through its masses. Sulphate of baryta is also very generally met with, in the form of veins of various sizes up to a foot or more in width, and occupying fissures in the greywacke. Near Elmford, the cleavage fissures of the greywacke strata are filled with this mineral.

No decided traces of *organic remains* have hitherto been discovered in the greywacke of this part of the Lammermuirs. On some slabs from a quarry near Hoardweel, some curious relieved markings appear, which may prove to be of or-

ganic origin; but as no trace of organic matter is observable in these specimens, it is uncertain whether they are not merely the effects of a chemical aggregation.

2. *The Old Red Sandstone.*

The formation which in this district immediately succeeds the greywacke, is the *upper division* of the old red sandstone, there being no strata on the flanks of the Lammermuirs, referable to any of the intermediate formations, comprising the Silurian system, and the lower and middle divisions of the old red sandstone. The position of the red sandstones and conglomerates, with reference to the greywacke, is unconformable. This is very distinctly seen at Cockburn Mill, where the conglomerate, which constitutes the lowest member of the formation, is well exposed, overlying the fractured ends of the greywacke strata in such a manner that the planes of stratification of the two sets of rocks are nearly at right angles to each other. The greywacke strata here are of the red variety, and dip at high angles to NW., the vacancies between their uneven and broken ends being filled up by the conglomerate. The latter is chiefly composed of fragments of the subjacent red greywacke, together with pieces of the felspathic rocks of the Stane-shiel; the whole being firmly cemented by the finer arenaceous particles derived from the adjoining rocks.

The variations in the thickness of the conglomerate at different localities of this limited district are very great, and would appear to indicate the proximity of the shore, at the period of its deposition; a view which other circumstances, to be hereafter detailed, tend strongly to corroborate. At Cockburn Mill, its thickness may average about 20 feet; whereas to westward of the Knock hill, it is at least 300 or 400 feet. At the latter place, it contains (besides fragments of greywacke, which are its chief ingredients) several varieties of felspar porphyry, together with rolled masses of quartz, hæmatite, and other minerals. In Kidshiellaugh, and near the Knock hill, it consists almost entirely of fragments of the felspathic rock, which occurs *in situ* at these places. These are cemented by calcareous spar, which has probably been derived from the igneous rock, as the latter contains a considerable quantity of carbonate of lime in its composition. Similar instances of calcareous matter acting as a cement to the conglomerate, are not unfrequent on the borders of the Lammermuirs.

At Cockburn Mill, the conglomerate passes, by a series of alternating conglomerates and sandstones, into the characteristic strata of the formation. These consist of red and greenish-white sandstones, which alternate with red clays, the thickness of the whole of which, as exposed in Prestonhaugh, is probably at least 300 feet; but, on account of the shattered nature of the strata, this cannot be exactly ascertained. Much interest attaches to these strata on account of the organic remains, and the curious markings which appear to be of organic origin, in which they abound. These we shall attempt to describe as concisely as possible.

Organic remains were first discovered among these sandstones, in the summer of 1840, in a fine section exposed by the Whiteadder about half a mile below Cockburn Mill. These chiefly consist of scales, occipital plates, ichthyodorulites, and other bony parts of the *Holoptychius Nobilissimus*. Similar remains also occur in the sandstones directly opposite to Cockburn Mill, and in those to the east of the Knock hill. They are not distributed uniformly through the strata in which they occur, but are found only in particular beds, in which they abound; while very few or none are to be seen in the adjoining strata. At Cockburn Mill, the ichthyolitic beds are situated within a hundred feet of the conglomerate, from which they are separated by a series of intervening beds of unfossiliferous red sandstones, and thin strata of conglomerate. One bed which is exposed here contains the remains of the *Holoptychius* in such abundance, that a chip cannot be struck off without disclosing a portion of a scale or plate. It is of a coarse, sandy, gritty texture, and is generally so brittle from being highly impregnated with animal matter, as to be easily broken between the fingers. It is only a few inches thick. Some of the thicker beds with which it is associated also contain these fossils in considerable abundance, but they appear to be in greater quantity *near the surfaces* of each stratum, few being found in the interior of a thick bed. These strata present beautifully rippled surfaces, and (especially the gritty bed above mentioned) shew other unequivocal marks of littoral deposition. A circumstance which tends strongly to favour the view of their having been formed near the shore, is the fact of their included remains being of such a fragmentary character, that although pieces of scales and plates may be picked up in hundreds, it is very rare to find one of either that is not more or less mutilated. Indeed, it would appear that after those large fishes died, their osseous parts, being separated by decomposition and the action of the waves, were tossed about on the sandy beach, and exposed to attrition among the coarse sand and pebbles, until they were reduced to the fragmentary state in which we now find them. The fact of the remains of the *Holoptychius* being chiefly found associated with appearances indicative of the proximity of the shore, would almost lead one to suppose that these fishes were in the habit of frequenting shallow water, perhaps because of their food being there more abundant. At all events, their remains appear to be confined to these rippled strata; for in the corresponding beds of the old red sandstone above Greenlaw, which are destitute of ripple marks, and bear evidence of having been deposited in deep water, there are no traces of them to be found.

On the east side of the Knock hill, the ichthyolitic beds of this formation are tilted up at high angles by the mass of grey porphyry of which that hill consists. The remains found here are of the same fragmentary character as those of Prestonaugh, and abound most in a few thin sandy beds within a few feet of the porphyry. They consist chiefly of the scales, plates, spines, and teeth of a fish allied to the *Holoptychius*, but differing from it in several particulars, the scales being more

plainly marked, and the plates of a different shape and style of sculpture, being *reticulated* instead of *tuberculated*. The teeth are beautifully fluted, and appear to belong to the *Dendrodus striatus* of Professor OWEN, to which fish the associated scales, plates, and spines probably also pertained.

In Prestonhaugh there are found, besides the remains of the *Holoptychius*, other relics of a more obscure and puzzling, but highly interesting character, and seem to be of organic origin, though all trace of their original organic matter has which disappeared. These present many varieties of size and form, the most common being spindle-shaped bodies, which project in relief from the surfaces of the strata in which they occur. These are generally about half an inch long, detached, or connected by threadlike ridges. Another sort resembles in form and size the *crysalis* of a butterfly, while others present a vermiform appearance. It seems probable that the majority of these curious markings are nothing else than the petrified forms of soft-bodied animals that crawled in the mud of those ancient shores, and which, being covered by a deposit of sand, left the outline of their forms impressed thereon, the gases evolved in the process of decomposition escaping through the sand, and being replaced by the infiltration of the finer particles of mud. Others are perhaps coprolitic, and some, probably, mere concretions. The appearances presented by the bottom of a shallow pool which has been recently dried up, afford an apt illustration of some of these markings, the smooth glazed surface of the mud being marked here and there with worm-pits and castings, and furrowed by the traces of worms and insects, while it is divided into irregular portions by fissures of desiccation. In fact, some of these recent markings are almost perfect *fac-similes* of those which occur among the rippled strata of Prestonhaugh. Nor is evidence wanting among these strata of fissures having been produced by desiccation. There is one bed, in particular, of whitish sandstone, overlying strata of a softer and more clayey character, the under surface of which, wherever exposed, is seen to be entirely covered with relieved mouldings, which ramify in all directions, forming a sort of irregular net-work, and in short, exactly resembling the appearance which would be presented by the under surface of any plastic or molten substance poured into the cracks produced in mud by the heat of the sun. The depressions between these reticulated mouldings are generally smooth and shining, being coated with a fine red clay. Scales of the *Holoptychius* also occur pretty frequently on the same surface.

These interesting strata are succeeded by others in which the clays rather predominate, and which seem to be quite destitute of organic remains. They are, however, profusely marked with greenish-white spherical spots, of various sizes, from that of a pea, up to several inches in diameter. The colouring matter of the sandstone (the peroxide of iron) appears to have been discharged by some powerful deoxidising process, probably the putrefaction of animal or vegetable matter. Scales of the *Holoptychius* are sometimes met with, surrounded by a blanched

space, the breadth of which seems to be *less* in proportion to the *better* preservation of the scale, and *vice versa*. Where organic remains are most abundant, the strata are blanched in large irregular patches. In the centre of the spots a small speck of protoxide of iron may be found, but this is generally so minute, as not to be visible, unless the plane in which the spot is split passes through the centre.

The only decided *vegetable* remains found in Prestonhaugh, occur in some beds of soft red sandstone, overlying the spotted strata above described. The matrix in which they are embedded has been unfavourable to their preservation, so that they are obscurely marked, and when exposed to the weather for a short time, become almost indistinguishable. They appear to be a sort of *Algæ*. At Cockburn mill, some slabs of rippled sandstone present markings in relief much resembling *fucoids*, but shew no trace of carbonaceous matter, so that the vegetable origin of these, though not unlikely, is at least doubtful. Great numbers of the vermiform bodies before described are associated with these markings.

The strata in Prestonhaugh containing the vegetable remains, are succeeded by other beds of red and variegated sandstones and clays, marked with blanched spots and ripples, and containing, at least, one stratum in which the remains of the holoptychius are very abundant. Among these also occur thin beds of coarse sand, mixed with small rounded pebbles of white quartz, and presenting decided indications of littoral deposition. A succession of red sandstones and clays, which appear to have been deposited in deep water, follow, and may be traced along the south bank of the Whiteadder to Preston bridge, where they are interrupted by a large trap-dyke, accompanied by a very extensive fault or dislocation. On the opposite side of this dyke the coal measures appear tilted up at high angles, there being only the breadth of the dyke, which at this place does not exceed 100 yards, between the two formations. In consequence of this dislocation, and subsequent denudation, the upper beds of the old red sandstone, or those which graduate into the coal measures, are wanting at this place, having been entirely swept away on the west side of the dyke; while, on the east, they are buried beneath a great thickness of the coal measures.

The transition rocks thus removed or thrown down at this place, are pretty well developed in other parts of the county; as in the course of Langton burn, about two miles SW. from Dunse, and in that of the Blackadder above Fogo. They consist of thick beds, of a coarse, arenaceous, concretionary limestone, or cornstone, with which are associated beds of shale and clay, also impregnated with a considerable quantity of calcareous matter; and as regards colour and general aspect, blending the characters of the argillaceous strata of the old red, and the shales of the coal measures. They appear to be destitute of organic remains, either animal or vegetable, and exhibit no appearances of ripple marks or blanched spots.

3. *The Coal Measures.*

This important formation is represented in this district, by a small patch east of the Cumledge trap-dyke. Its strata are well exposed in the bed of the Whiteadder, near Preston bridge, about a quarter of a mile below which, are two fine sections. There the sandstones, shales and clays, which characterize the formation, are seen dipping away from the dyke at high angles, and presenting the usual appearances by which this series of strata is elsewhere distinguished. The shales and clays contain a large proportion of carbonate of lime in their composition; so much, indeed, that they might almost be termed *limestones*. The sandstones are of the usual white or yellowish colour, in many places highly micaceous, and abounding in impressions of *Stigmaria*, *Sigillaria*, *Lepidodendra*, and other plants of the carboniferous system. There is no appearance of *animal* remains.* No *coal* seams appear, but there is a thin stratum of ironstone in nodules, which abounds in remains of plants. The geological position of these strata, and the other rocks of the same formation, which prevail in the Merse of Berwickshire is considerably below the *Enerinal limestone*, which crops out near Berwick, and on the sea-shore at Lamberton. Indeed, they properly belong to the mountain limestone series, being situated far below the true coal measures. From their very low position in the series, there is no reason to suspect the existence among the Berwickshire strata of any coal-seams sufficiently thick to be worth working.

II. IGNEOUS ROCKS OF THE DISTRICT, AND THEIR EFFECTS UPON THE SEDIMENTARY ROCKS.

Throughout the district under consideration, trap-rocks are very abundantly distributed, and present a field of speculation, no less attractive than those of aqueous origin. The traps of the Lammermuirs and adjoining districts belong to two great classes, differing from each other, as well in mineral character and general aspect, as in regard to the epochs of their eruption. These are the *Porphyries* and *Greenstones*, or *Felspathic* and *Augitic* traps, both of which classes are very abundant, but (with one or two exceptions, to which we shall afterwards have occasion more particularly to refer) do not occur associated with each other; for it is a remarkable fact, that while the traps which occur among the greywacke of the hills are *uniformly of the Felspathic class*, those which appear in connection

* Within the last few weeks the remains of fossil fishes have been discovered in the course of Langton Burn, about a mile SW. from Dunse, in strata belonging to the lower part of the coal measures. These remains consist of scales, spines, teeth, and other bones, similar to those found at Burdiehouse. They occur in a soft friable sandstone, which abounds also in *Lepidodendra*, and other plants of the coal formation. Some remains of the *Holoptychius* have likewise been recently found in the old red sandstone strata on the estate of Billic, about four miles NE. from Dunse.—21st April 1845.

with the secondary strata along the flanks of the Lammermuirs, *belong as exclusively to the Augitic family*. In considering these, we shall follow the natural order, by describing, 1st, the Felspathic traps, and their effects upon the greywacke strata; and, 2dly, the *Augitic*, or more modern traps, and the effects which they have produced upon the more ancient rocks, both stratified and unstratified.

1. *Felspathic Traps.*

Rocks of this class are very abundant among the Lammermuirs, generally occurring in the form of dykes or veins of various sizes, intersecting the greywacke; or in large masses constituting entire hills. They consist of two or three varieties of granite and syenite, with an almost endless variety of feldspars, claystones, and feldspar porphyries. The following are some of the principal rocks of this kind which occur in the district.

Granite of Cockburnlaw and Staneshiel.—This rock shews a great variety of aspects at different parts of these hills. Near the *outskirts of the mass*, where it is in contact with the greywacke, its constituent crystals are very small, and its cleavage structures rectangular, and according with the strikes and cleavages of the adjoining strata. On proceeding into the interior of the mass, however, the rectangular cleavages due to its refrigeration, in accordance with certain lines, are superseded by the structures resulting from the crystalline tendencies of the granite, which have of necessity had more time for their proper development, in proportion as the distances from the cooling surfaces increased. The rock, in consequence, becomes harder, and its crystals larger and better defined, while it is divided by its structural planes into large, irregular, pyramidal blocks. Its most common character is that of a regular granite, composed of distinct crystals of white quartz, red feldspar, and black mica; being identical, both in regard to geological age and lithological aspect, with a granite which is associated with greywacke near Fassney Bridge; and which, from being intruded among the strata of the latter, in conformable beds, caused much discussion between the Huttonians and Wernerians. In some places, as, for example, on the south side of the Staneshiel, about half way up the hill, sulphate of baryta is added to the usual ingredients of the granite; and near the top of that hill small quartz veins occur, containing *galeua* and *copper pyrites*, though in very minute quantity. On the left bank of the Whiteadder, about 60 yards below Cockburn mill-dam, a mass of syenitic rocks of extreme hardness appears in contact with the greywacke, and projecting into the bed of the river. This is evidently a process from the Staneshiel hill, the granite of which, as well as that of Cockburnlaw (which is indeed a part of the same mass), in many places passes into syenite. In fact, after an attentive consideration of the phenomena presented by the transition of one rock into another, and especially the changes effected upon both aqueous and igneous rocks, at and near their junctions, of which many highly instructive

examples are seen in this district, it seems not improbable that the *syenite* of Cockburnlaw and the Staneshiel is nothing more than *greywacke*, fused by the agency of the molten granite, and the mineral characters of the two rocks thereby blended together. The granite invariably assumes the aspect of syenite, as it approaches the greywacke. At Cockburn Mill dam, the greywacke is considerably hardened, and dips to NNW. at high, but varying angles. Below the dam the hardness increases, the planes of stratification become less distinct, while those of cleavage grow more decided at every step. All these symptoms of metamorphism increase as we approach the igneous rock, the texture of the greywacke being changed to crystalline, and the size of the crystals increasing with the hardness, until we arrive at a point, where it is impossible to decide from the appearances presented, whether the rock should be considered *greywacke* or *syenite*. Beyond this, it graduates into true syenite, which is divided by cleavage planes into large rectangular blocks, arranged in the form of thick beds, having the same dip and strike as the adjoining greywacke. The cleavage planes which run *parallel* to those of the stratification of the greywacke (if, indeed, these are not merely the original planes of stratification of the rock before it was converted into syenite), are distinguished from those *running at right angles* thereto, by being occupied by veins of *heavy-spar*, associated with crystals of quartz. These veins run continuously in a WSW. to ENE. direction. As we proceed further into the mass, the syenite becomes more crystalline, and passes by a regular and gradual transition, into the well characterized granite of the Staneshiel. Two varieties of granite are here seen intruding in the form of veins, which have evidently been poured, in a molten state, into fissures opened in the syenite, during the process of crystallization. One of these is a beautiful and regularly crystallized granite, larger in the grain than is commonly met with in the adjoining hills. The other is small grained, and rather soft, and seems to be connected with a dyke which occurs in the bed of the river a little above the dam, being identical in mineral character. This dyke can be traced, when the water is low, for about 200 yards. It is about five feet thick, and runs nearly NNW., but is frequently interrupted in its course by the greywacke which it crosses. It contains, in some places, fragments of the latter rock, which it has detached and brought up with it, in its passage through the strata. The adjoining strata have been partially fused, and present in some places a syenitic appearance. The alteration extends to a considerable distance from the dyke, the strata being extremely hard, and frequently exhibiting contorted laminae. In the immediate vicinity of the dyke the planes of stratification are very obscure, being, in many places, merely marked by veins of spar. Both the dyke and the adjoining metamorphic rocks are traversed by numerous veins of *heavy-spar*, together with a few of quartz.

The summit of Cockburnlaw consists of beds of metamorphic greywacke, which dip to NW. at an angle of about 65°. The metamorphism of the strata increases as we approach the great body of granite, which lies immediately below

the summit to SE. They here present a highly crystalline and *syenitic* appearance; and, in hardness, exceed even the granite itself, on the shoulder of which they have been elevated to their present position.

Associated with this granite are several varieties of porphyry, belonging to the same geological epoch. These appear at short intervals in the bed of the river, from the eastern boundary of the granite, as far as Abbey St Bathans, a distance of about four miles. The granite of which the steep hill opposite Cockburn-eastfield consists, is seen at the brink of the river to pass into a kind of porphyry, consisting chiefly of whitish felspar, with crystals of dark coloured mica. Another variety of porphyry, which also occurs at a short distance from the eastern margin of the granite, is of a deep red colour, derived probably from the peroxide of iron. But the most common variety is a porphyry, having a basis of cream-coloured felspar, with disseminated small crystals. Of this Blackerstone hill is composed; and it may be seen at intervals between the east side of the granite and Hoardweel, underlying the greywacke. At some places, where the latter rests immediately upon the porphyry, it appears to have been actually fused, having lost every semblance of its original stratified structure, and being divided, like the subjacent porphyry, into extremely sharp pyramids and wedges. In some places, especially where it comes in contact with the whitish porphyry before mentioned, the greywacke has, in the process of fusion, become blended with the igneous rock, forming a curious mongrel sort of compound.

At the bend of the river, below Hoardweel, the porphyry is seen penetrating the greywacke in the form of conformable dykes. From this place to the copper mines, the channel of the river is narrowed by vertical rocks of metamorphic greywacke. At the "Strait Loup" it rushes through a gorge so narrow that it may in general be easily stepped over. The geological phenomena displayed here are very interesting. Within a space of about fifty yards by thirty, the porphyry has forced its way through the strata in eleven or twelve different places. The greywacke is much hardened and contorted; and, near the contact with the igneous rock, becomes cupriferous, and abounds in quartz veins. The copper ore, which is of the green and grey varieties, occurs in the schists which alternate with the greywacke. The porphyry is generally of the same kind as that further down the river; but in some places passes into a greyish-white compact felspar; and, in others, becomes a kind of granite (the felspar, however, predominating), which contains disseminated specks of iron pyrites. In some instances it forms dykes, which are, to a certain extent, conformable with the greywacke; but it commonly occurs in irregular masses of small extent, lying among the disturbed strata, and connected with each other by veins or dykes.

The metamorphism of the greywacke is observed invariably to take place in the vicinity of the granite and associated porphyries; and the process can be traced in a most satisfactory manner, through all its stages, in many places

among these hills. At several localities, series of specimens may be obtained, shewing the gradual transition from the unaltered greywacke, to where it becomes converted into syenite, which again passes into granite; and the latter, in its turn, graduates into porphyry,—the progress of transformation being so gradual, as to render it very difficult to decide where the characters of the *aqueous* rock become merged in those of the *igneous*. In many cases, the only way of distinguishing the original planes of stratification from those of cleavage, which have been superadded in the metamorphic process (unless, indeed, we can trace the strata uninterruptedly from where they are unaffected), is by the schists which alternate with the greywacke. These are sometimes much contorted, and present the appearance of hornblende slates.

While the greywacke is thus altered, wherever it approaches the granite or porphyries, the old red sandstone strata, being of a date subsequent to that of the eruption of these older traps, are unchanged even at the place of contact. This is seen on the SW. side of the Staneshiel, where the red sandstones are *tilted up against the granite at high angles, without any appearance of alteration*,—the granite and overlying sandstones having, in this instance, been elevated (as we shall hereafter shew) by the agency of the *augitic* traps.

Porphyry of the Knock Hill.—This rock is of a grey colour, having a felspathic basis, and containing a considerable quantity of carbonate of lime. The hill to westward of Burnhouses consists of the same rock, and is connected with the Knock hill by a ridge running in a SSE. direction. To westward of the felspathic mass of the Knock hill, the greywacke appears in nearly vertical strata, with a NNW. strike, and is considerably altered by the proximity of the igneous rock, being hard and full of quartz veins. On the east side of the hill, and within a few feet of the porphyry, the red sandstones are seen tilted up at high angles, and even partially retroflexed; but being of more modern date (as in the case of the junction of the granite and sandstones on the west side of the Staneshiel), they shew no traces of metamorphism, but remain quite fresh and soft; and the remains of scales, &c., of the *Dendrodus* which they contain still preserve their original colour. Similar appearances are also observed in the vicinity of the porphyry in Kidshielhaugh.

2. *Augitic Traps.*

Rocks of this class also abound in the form of dykes, beds, and irregular masses, and consist of several varieties of greenstone, basalt, amygdaloid, and trap tuff. These have all been erupted subsequent to the deposition of the old red sandstone and lower coal measures, as is evident from the disturbance and metamorphism apparent in these strata, whenever they approach to traps of this class. With one or two exceptions (to be hereafter noticed) they do not appear in contact with the older rocks, being confined to the secondary strata, which are much

disturbed by their agency. Besides the veins and masses which are exposed, there are, undoubtedly, many veins and extravasated portions concealed among the strata. The principal masses of these traps which occur in the district are the following :—

The *Cumledge trap-dyke* is a large body of trap of irregular thickness, which is seen in the bed of Oxendean Burn, near Cumledge House, and about 200 yards from the Whiteadder. It is here a sort of amygdaloidal greenstone, abounding in veins of zeolite, steatite, and other minerals, and is probably not more than ten yards in thickness. On the west side it appears in contact with beds of a sort of cornstone, which are excessively hard and crystalline at the junction, while the trap becomes soft and earthy. On the east side a similar description of rock occurs. From this spot to the Whiteadder the strata are hid by debris, but appear to belong to the coal measures, of which a fine section is presented a short way below the place where the burn joins the Whiteadder, in a cliff of more than eighty feet high. The shales, sandstones, and clays, are here seen dipping away from the dyke at angles, which increase in proportion to the proximity of the line of disturbance, until they become vertical, and even partially reversed. From this place to Preston Bridge, the strike of the strata is very regular, and parallel to the course of the dyke, which runs in a SSE. direction. At Preston Bridge its thickness is upwards of 100 yards. The strata of the coal-formation come close up to it on the east side; while on the west it cuts off the old red sandstone. On the north bank of the river, about 300 yards above the bridge, it is seen in contact with strata of whitish sandstone and grey calcareous shales, which seem to be equivalents of those strata which elsewhere constitute the transition beds between the old red sandstone and coal measures. From this place it may be traced in the bed of the river, presenting, in general, the aspect of an amygdaloid, until we reach a place called “Anglemyheart,” where it passes into a beautiful columnar basalt. The columns are irregular hexaedral prisms, not arranged vertically, but dipping at high angles to west. The basalt is more crystalline, and the columns more regular towards the interior of the mass. Towards the outskirts it passes into an olive-coloured greenstone; and the latter graduates into a trap-tuff, composed of fragments of greenstone and metamorphic greywacke, agglomerated into a mass.

This trap-dyke appears to run under the granite of the Staneshiel and Cockburnlaw,—not, however, *directly under the centre* of the granitic mass of these hills, but *more to the west side*. In the glen or ravine between these two hills, and exactly in the line of continuation of the Cumledge dyke, a mass of basalt occurs, which has burst through the older rocks. There is every reason to believe that an eruption has taken place from a crater at this locality, and that the basalt now occupies what was formerly a volcanic vent. A considerable quantity of trap-tuff is seen in the bed of the river, in the vicinity of the basalt, and at intervals

for about 300 yards farther down, wherever it has been protected from denudation. It consists of a coarse sand (apparently triturated granite), containing rounded nodules of various sizes, from that of a pigeon's egg upwards, formed of concentric coatings of the granitic sand round a nucleus, which appears to consist of an imperfect sort of basalt.

The basalt is of a dark colour, approaching to black, and is very hard. It is occasionally amygdaloidal, and has a tendency to assume the form of concretionary masses of a spheroidal figure, consisting of concentric coatings. The granite is seen in contact with it on the north side,—the line of junction running in an ENE. direction. On the south side the two rocks are separated by a deep pool in the river, which has probably been formed by the washing out of the incoherent tuff which here enwraps the basalt. On the west side of the basalt is a very hard rock resembling syenite, and which is probably greywacke, fused by the igneous rock. Its cleavages, as well as those of the granite and basalt, are ENE. by NNW.

In the interesting section exposed by the Whiteadder at Cockburn Mill, a bed of trap about 4 feet thick is seen overlying the old red sandstone strata for a considerable extent. It is of a brown colour, and abounds in vesicles generally about the size of small peas, some of which are empty, but the majority are filled up with various minerals. Its texture is earthy, and it is much debased by being mixed with the debris of the adjacent sandstones and clays, portions of which it had taken up in its course when in a molten state. The embedded fragments are hard, crystalline, and cherty, and the adjoining strata are discoloured, and their laminae contorted, shewing very clearly the effects of igneous action. As this bed of trap appears to be destitute of augite, it should, mineralogically, be classed with the porphyries and other felspathic traps of the Lammermuirs. It is, however, more recent than these, having been erupted subsequent to the deposition of at least the greater part of the old red sandstones. At the same time, it seems to be more ancient than the augitic traps by which the second upheaval of the Lammermuirs was effected, as it shews some appearances of having participated in that movement along with the adjoining sandstones.

The trap of Castlemains hill is a mass of greenstone, which has forced its way through the old red sandstone strata. The sandstones are much hardened at and near their junction with the trap, while the same beds are seen within 300 yards to NW., *in contact with, and tilted up by,* the grey felspathic rock of the Knock hill, without the least appearance of alteration. The latter circumstance which seems rather anomalous, is accounted for by a dyke of augitic trap, which runs from under the Knock hill in a SSE. direction to Borthwick, and has upheaved at the same time, both the felspathic rock and the more ancient sandstones.

Borthwick hill is a vast mass of basaltic greenstone, which has been erupted through the old red sandstone strata at the place of intersection of several fissures

which are likewise filled with trap. One of these already referred to, runs in a NNW. direction under the Knock hill, not however *directly under its centre*, but towards the west side, and has thereby thrown the mass of felspathic rock, of which that hill consists, *over to east*, producing a partial retroflexion of the sandstone strata on that side. In this respect, it exactly resembles the Cumledge trap-dyke, which, as formerly mentioned, has thrown the granitic mass of the Staneshiel hill also over to eastward. Another striking point of resemblance between these two dykes, is the occurrence of an insulated mass of *onion basalt*, exactly in the line of continuation of each. The basalt in the line of the Borthwick trap-dyke occurs (in the channel of the burn which falls into the Whiteadder at Elm Cottage) about 3 miles NNW. from the Knock hill. It consists of concretionary masses of various sizes, up to a foot or more in diameter, composed of nuclei of bluish grey basalt, coarse in the grain, and very hard and heavy, surrounded by concentric coats of a tufaceous substance. There is a slip in connection with the line of fissure, from which the felspathic rocks of the Knock hill, and afterwards the augitic traps now under consideration, were erupted. This has caused a downcast of the old red sandstone strata on the west side to an extent of 300 or 400 feet.

At Oxendean Commonhaugh, a mass of basalt is wrought in three quarries. It has been erupted through the old red sandstone, some of the strata of which are seen resting on its surface, much broken and altered by the heat. The original red of these beds has been changed to a dull purple, and the fragments into which they have been shivered are much indurated. The trap immediately subjacent is much debased by having absorbed, when in a molten state, a quantity of the detritus of the sandstone. The result is a curious compound, which is neither trap nor sandstone, but a mixture of both, and which graduates insensibly into the aqueous rock on the one hand, and the igneous on the other. In some places, also, the pulverized sandstone has got into fissures in the trap, where it has afterwards consolidated, presenting the singular phenomenon of *veins of sandstone in basalt*. The sandstone of these veins exhibits *vertical lamination*.

The Basalt of Dunselaw is similar to the above, with which it is connected. It has also been erupted through the old red sandstones, which rise towards it all along the south side, as shewn by excavations in the town of Dunse and neighbourhood. A large block of metamorphic red sandstone, which is known by the name of the "*Covenanters' Stone*," may be seen near the top of the hill. It seems to be a portion of a stratum which has been detached and borne up on the surface of the trap. It is hard and granular, and dips to ENE. at a high angle.

The trap of the Castleknowes runs from Dunselaw in an ENE. direction, and probably joins the Cumledge trap-dyke at right angles. At the New Tile Works, about a mile east from Dunse, it is a beautiful amygdaloid, containing nodules of various minerals, coated with green earth. On the face of the bank south of

the Castleknowes, the old red sandstone strata are seen dipping away from the dyke, and divided by cleavages corresponding to those of the trap which are NNW. by ENE. Nearer the dyke they are much shattered, and are very hard and crystalline.

The trap of Grindean, is a basaltic ridge which runs in a SSE. direction, and appears to be connected with a line of fault similar to that of the Cumledge dyke, which runs parallel to it, the old red sandstone strata appearing on the *east side*, and those of the coal measures *on the west*; while the distance between the two formations is much too small to admit of the strata, occupying an intermediate space in the series, being brought on. The usual effects have been produced by the trap upon the adjoining sandstones. In some specimens the transition from a rubbly sandstone to a beautiful compact jasper is finely shewn.

A very interesting circumstance observable with regard to all the augitic traps in this district, is, that their course is either in a NNW. direction, or in one at right angles thereto, viz. from WSW. to ENE,—and these are invariably the directions of their cleavages. They likewise appear to be all connected together, forming one great system, indicative of one epoch of eruption. Thus the trap of Borthwick hill is connected with that of Oxendean Commonhaugh, while the latter joins that of Dunselaw, by means of a ridge running by St Mary's Cottage. The Castleknowes trap, again, joins the Cumledge trap-dyke to Dunselaw. Another large trap-dyke (a small portion of the course of which is laid down on the accompanying map), can be traced from Raecleughhead hill by Langton, Gruel-dykes, &c., for several miles in an ENE. direction, and seems to join the prolongation of the Cumledge dyke in the neighbourhood of Edrom, about four miles from Dunse. A line drawn from the Castlemains hill in an ENE. direction, passes through the basaltic rocks of Anglemyheart, and meets the trap of Grindean. This line appears to be that of an extensive fault, the effects of which are manifest in the extraordinary disturbance of the old red sandstone strata, at the end of the Prestonhaugh section, near Anglemyheart. Between the latter place and Grindean, this dislocation (which is probably associated with a trap-dyke) appears to separate the old red sandstone and coal measures, the latter being thrown down on the south side; but, unfortunately, no section is exposed in the course of this line.

The consideration of the agencies which have influenced the directions of the trap-dykes and lines of fault within the district, and changed the positions and characters of the sedimentary rocks, is of great geological interest, but the principles involved are of too general a character to admit of being discussed in a memoir, descriptive merely of a small district. We therefore abstain from theorizing upon the facts adduced, the more important inferences which these suggest being for the most part sufficiently obvious. The views of the author on this branch of the subject, are also in part indicated on the Map and Sections, lithographed on Plate II.

VI.—*On the Extraction of pure Phosphoric Acid from Bones, and on a new and anomalous Phosphate of Magnesia.* By WILLIAM GREGORY, Esq., M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh.

[Read 3d March 1845.]

I.—*On the Preparation of Pure Phosphoric Acid.*

THE usual methods of obtaining pure phosphoric acid by the oxidation, with nitric acid, or by combustion, of pure phosphorus, are well known; but, although they yield a pure product, yet, as the phosphorus must be prepared from phosphoric acid, it is obvious that we shall derive a great advantage from any method of purifying easily and cheaply the phosphoric acid from bones, instead of first reducing it to phosphorus, and then re-oxidizing it. In practice, phosphorus is made from the superphosphate of lime, and it is from the same salt that phosphoric acid may be most economically prepared.

Two processes, already given for this purpose, are worthy of notice. It is to be borne in mind, that the superphosphate of lime is the soluble compound obtained by acting on burnt bones with sulphuric acid and water, and filtering to separate the sulphate of lime.

In the first process, the solution of superphosphate is neutralized with ammonia or carbonate of ammonia, which precipitates all the lime in the solution, with about one-fourth of the phosphoric acid, as bone phosphate; while three-fourths of the acid are converted into phosphate of ammonia. The filtered liquid being evaporated, deposits crystals of that salt, which, when purified, are decomposed by heat in a platinum crucible; the ammonia and a great part of the water being expelled, while the phosphoric acid, with one equivalent of water, or metaphosphoric acid, is left. The objections to this process are the following:—The salt, when heated, melts, spirts much in boiling, becomes viscid, and froths up to a most inconvenient degree, requiring vessels of platinum of a large size for small quantities of material. Secondly, a very high and long continued heat is required to expel all the ammonia; and at that temperature, a portion of phosphorus is reduced by the hydrogen of the ammonia, and corrodes the platinum, leaving a blue stain of phosphuret of platinum. Besides this, the purest phosphate of ammonia often contains a trace of organic matter, which causes the glass of phosphoric acid, thus prepared, to be disfigured by carbonaceous particles. At least I have always seen black particles in the phosphoric acid made by this process. It is obvious, that if carbon be present, we have an additional source of reduced phosphorus; and if the black particles are phosphuret of platinum, then they

show that the platinum has been strongly corroded. In short, this method, save on a very small scale, is so objectionable, that it is seldom employed except for illustration.

The second process is one recently proposed, I believe, by LIEBIG, founded on the fact, that the whole lime may be removed from the superphosphate of lime, by the addition of sulphuric acid to the concentrated solution. This causes it to become quite thick from the large quantity of sulphate of lime produced. Cold water being added, the whole is filtered, and the filtered liquid and washings again concentrated and filtered from any sulphate of lime deposited during the evaporation. The concentrated liquid is again tested for lime by sulphuric acid, and if no change ensues, the lime has been entirely removed, as I ascertained by the proper tests. The solution now contains only the whole phosphoric acid of the bones, the magnesia which they always contain, and more or less free sulphuric acid. I have described this process thus far minutely, because, up to this point, it is the same as that which I recommend; and it is in the mode of separating the magnesia that the advantage of my process consists.

LIEBIG acts on the concentrated acid solution, brought to the consistence of syrup, by alcohol, which dissolves the phosphoric acid, leaving undissolved the greater part of the phosphate of magnesia, and depositing the last traces when allowed to stand. I cannot ascertain whether this operation is to be performed before or after the sulphuric acid has been expelled by heat; but I find, that after expelling the sulphuric acid, a transparent and colourless glass is obtained, which dissolves perfectly in boiling water, and the solution concentrated to a syrup, and treated with alcohol, yields a solution containing much magnesia, and which, on standing for weeks, deposits nothing. It is very probable that the alcohol did not succeed in separating the magnesia in my experiments, because the phosphoric acid was in some one of its modifications, different from that in which LIEBIG employed the same method. But I have not yet been able to manage that process so as to answer the purpose intended; and, even if it did succeed better, it is well known that alcohol, at its present price, cannot be used in this country on the large scale. I therefore endeavoured to find means of dispensing with its use, and I began by studying the properties of the soluble glass above mentioned, which contains only phosphoric acid, water, and magnesia.

This glass dissolves slowly, but perfectly, in boiling water; but the solution, when again concentrated, and so far deprived of water, that its temperature in an open platinum capsule rises to nearly 600° , suddenly becomes turbid, from the separation of a powder, while crystals begin to form in the viscid mass, resembling those which form in honey. When cold, water dissolves these crystals instantly, and leaves undissolved only a heavy white powder, which is a peculiar phosphate of magnesia. I shall return to it presently. It is quite insoluble in water.

As this salt was evidently formed at a certain temperature, I could see no reason why the whole of the magnesia might not be converted into that insoluble form, although, in this first experiment, the filtered liquid was found to contain much magnesia. I therefore again evaporated the filtered solution; and, at the same temperature as before, it again became turbid. I kept up the same heat for fifteen minutes; and when the mass, after cooling, was acted on by water, a large quantity of the insoluble phosphate separated, and the filtered liquid was now found absolutely free from the smallest trace of magnesia. It now clearly appeared that the first heating had been too short, and that it was only necessary to heat long enough to bring every part of the mass to the same temperature.

The process which I recommend, therefore, is as follows:—The glass remaining, after heating to redness in a covered platinum crucible to expel sulphuric acid (after the separation by that acid of all the lime), is to be boiled in water, and the solution evaporated, and, finally, exposed in a platinum capsule for a quarter of an hour, to a heat of from 595° to 600° , or to that temperature at which the acid begins to volatilize with the water. It must not be heated more strongly, lest the glass should be reproduced. When cold, the mass is to be softened with water, and the solution of pure phosphoric acid filtered from the insoluble phosphate of magnesia. The acid is pure from magnesia, if, when diluted and super-saturated with ammonia, it forms no deposit, especially after standing for one or more days. I have repeated the process six or seven different times, and on no one occasion did the filtered acid contain magnesia, except when I had purposely heated too strongly. This proves that the degree of heat is not difficult to manage. Indeed, after the first experiment I found it unnecessary to use the thermometer, the appearance of the mass furnishing a sufficient guide. It is evident that the above process has the twofold advantage of simplicity and economy.

The quantity of magnesia which separates in the form of the insoluble phosphate is very considerable. It will be very easy to determine exactly the amount of magnesia in bone-earth, by converting it into this salt.

II. *On a new and anomalous Phosphate of Magnesia.*

This is the salt so often mentioned above, as being separated by a heat of 600° from its solution in phosphoric acid. As the salt is perfectly anhydrous, it is obvious that its formation is owing to the separation of water at that high temperature. The analysis of the salt was made by fusing with carbonate of soda, dissolving in diluted hydrochloric acid, adding, for precaution's sake, a little phosphate of soda, and then precipitating by ammonia, collecting the ammoniacomagnesian phosphate on a filter, washing moderately with cold water, drying and igniting. The residue, pyrophosphate of magnesia, was reckoned to contain

36.67 per cent. of magnesia. In three analyses, the per-centage of magnesia in the new salt was found to be 16.78, 16.92, and 15.94. In the last of these I pushed the washing further than in the first two, and I did the same in three more analyses, which yielded 15.12, 15.34, and 15.12, per cent. of magnesia. The mean of the six analyses, all made with portions of salt prepared at different times, is 15.87 per cent. of magnesia. As the salt lost no weight by ignition, it contained no water, and therefore was composed of magnesia and phosphoric acid alone: the absence of lime being previously ascertained. Its composition is therefore,

Magnesia,	15.87
Phosphoric acid,	84.13

The only formula which at all approaches to this composition is $3 P_2 O_5 + 2 Mg O$, according to which, it is an acid sesquiphosphate of magnesia. The composition, calculated according to this formula, is,

Magnesia,	16.18
Phosphoric acid,	83.82

Considering the imperfection of the means for determining the amount of magnesia in analyses with precision, there can, I think, be no doubt, that the above formula expresses, empirically, the composition of the salt in question.

But what is the rational formula of this salt? As far as I know, there are no known sesquiphosphates of protoxides. Indeed, the very characteristic of the three known classes of phosphates, is their tendency to form salts with 1, 2, and 3 equivalents of base for 1 of phosphoric acid.

Metaphosphoric, or monobasic phosphoric acid, forms salts of the general formula, $P_2 O_5, M O$; and if our salt is to be classed as a metaphosphate, it must be one with the very singular formula $2 (P_2 O_5, Mg O) + P O_5$; in which 2 eq. of metaphosphate of magnesia are combined with 1 eq. of anhydrous phosphoric acid, which has either entered the radical of the acid, without increasing its neutralising or assumed a neutral character.

Pyrophosphoric acid, or bibasic phosphoric acid, forms salts of the general formula, $P_2 O_5, 2 M O$, or $P_2 O_5 \left\{ \begin{smallmatrix} M O \\ H O \end{smallmatrix} \right.$. If our salt be a pyrophosphate, and the temperature appears favourable to its being so, its formula must be $(P_2 O_5, 2 Mg O) + 2 P_2 O_5$, in which 1 eq. pyrophosphate of magnesia is combined with 2 eq. of anhydrous phosphoric acid, in one of the characters alluded to above.

Lastly, common or tribasic phosphoric acid forms salts of the general formula, $P_2 O_5, 3 M O$; $P_2 O_5 \left\{ \begin{smallmatrix} 2 M O \\ H O \end{smallmatrix} \right.$, or $P_2 O_5 \left\{ \begin{smallmatrix} M O \\ 2 H O \end{smallmatrix} \right.$. If our salt belong to this class, its formula will be $P_2 O_5 \left\{ \begin{smallmatrix} 2 Mg O \\ P_2 O_5 \end{smallmatrix} \right. + P_2 O_5$. Here, 1 eq. of phosphoric acid, act-

ing as acid, is united to 2 eq. of magnesia, and 1 eq. of phosphoric acid, acting as base, while a third eq. of phosphoric acid acts in another capacity, possibly as a neutral body, like water of crystallization.

In all these supposed formulæ, we have anhydrous phosphoric acid acting in an unusual capacity; and it is evident that, whichever we adopt, the occurrence of the salt is favourable to the doctrine, that the so-called anhydrous acids are not really acids. It is true that, on the old view of phosphoric acid and phosphates, there is nothing startling in a sesqui-phosphate; but, if we adopt this view, we must cast aside all the knowledge recently acquired concerning the phosphates, and which, to a great extent, is established by experiment.

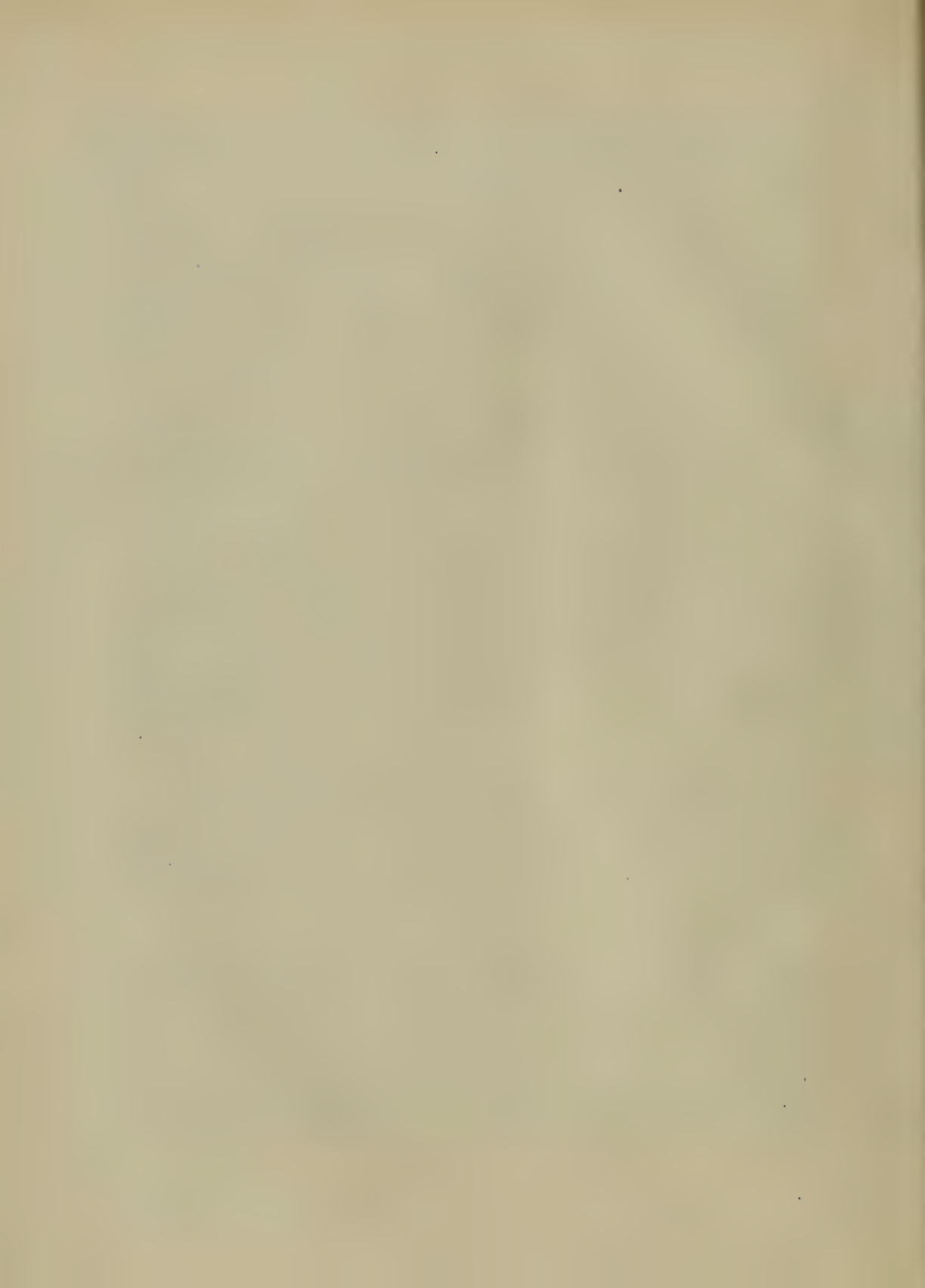
On the whole, the composition of this salt is so anomalous, that we must conclude, either that the received views on the subject of the phosphates are erroneous, or that there exists a fourth modification of phosphoric acid, distinct from the three usually admitted. We might suppose this acid to be $3 P_2 O_5 = P_6 O_{15}$, and to be neutralised by 2 eq. base, yielding the formula $P_6 O_{15}, 2 M O$.

It is worth while to remark, that, on the theory of compound acid radicals and hydrogen acids, according to which the formula of a tribasic phosphate is $P_2 O_8, M_3$, that of a bibasic phosphate, $P_2 O_7, M_2$, and that of a monobasic phosphate, $P_2 O_6, M$, this salt cannot be represented as a sesqui-metaphosphate, although, as we have seen, it may be so on the old view of acids and salts. The latter makes it $3 P_2 O_5 + 2 M g O$; but the former would require 1 eq. of oxygen more to yield the formula of a sesqui-metaphosphate $3 P_2 O_6, M g_2$, the salt containing only $\left. \begin{matrix} 2 P_2 O_6 \\ P_2 O_5 \end{matrix} \right\} M g_2$. In this point of view, the existence of the new salt may

turn out to be a serious objection to the above named theory of salts and acids.

The new phosphate is remarkable, among all the salts of magnesia, for its extreme insolubility in water and acids, which is such, that it may be ranked beside sulphate of baryta. This insolubility has hitherto defeated all my efforts to ascertain the precise nature of the acid it contains, and whether that acid be new or not.

As this salt is not only very insoluble, but easily washed, and less hygrometric than any powder I have ever weighed, it would be well adapted for the determination of magnesia, and also of phosphoric acid, if we could, at pleasure, convert these substances into this particular form of combination when mixed with other bodies. The experiments I have made on this subject have convinced me, that there are very great difficulties in the way of this application; but, I trust, they will not be found insuperable. It will, at all events, be easy to determine the amount of magnesia in bones, by converting the magnesia, as in the above process, into this insoluble form.



VII.—*Miscellaneous Observations on Blood and Milk.* By JOHN DAVY, M.D.,
F. R. S., Lond. and Edin., Inspector-General of Army Hospitals, L. R

(Read April 7. 1845.)

1. *On the State of Combination of the Alkali in the Blood.*

The condition of the alkali in the blood—of that portion on which its alkaline reaction depends—has been the subject of much speculation, and of many experiments. ENDERLING is one of the latest inquirers who has given it his attention. After having made an analysis of the ashes of the blood, he has come to the conclusion, that the alkali in it is in combination with phosphoric acid, the former predominating in the form of the tribasic phosphate of soda.*

Granting the accuracy of ENDERLING'S analytical results on the ashes, does it follow that his inference must be correct relative to the condition of the alkali in the liquid blood? It appeared to me doubtful *a priori*; and the doubt I entertained was confirmed by experiment. The doubt arose from considering the tendency of the alkaline carbonates, when strongly heated with charcoal, to be reduced; and when heated with phosphate of lime in excess, to exchange their carbonic acid for a portion of the phosphoric—the acid gas of course escaping, and compounds of lime and alkali remaining, each with excess of base. In accordance with this, when I have added carbonated alkali to the coal obtained from blood, and have reduced the coal to ashes, I have not been able to detect in the lixivium obtained from them any trace of carbonic acid. Moreover, I find that the carbonate of soda is liable to loss when heated strongly, exposed to the air; and, consequently, when it exists in a small quantity in a bulky coal, the whole of it may be dissipated—carried over much in the same manner as boracic acid is in combination with water as a hydrate, when it is subjected to heat.

If ENDERLING'S view were correct, the blood, after having been acted on by the air-pump, ought not in its fresh state to yield any carbonic acid on the addition of an acid. This is the experiment alluded to, which confirmed my doubt. I find that blood or its serum, after having been so acted on until perfectly tranquil, has effervesced strongly, when mixed either with dilute sulphuric or muriatic acid purged of air, or with a solution of cream of tartar. And, in accordance with this, I have also found that serum, after having been subjected to the air-pump, gives on coagulation, by immersion in boiling water, a different result, whether

* See Mr PAGET'S Report on the Progress of Human Anatomy and Physiology, in the British and Foreign Medical Review for January 1845.

immersed unmixed, or after admixture with a little acid. In the one instance no air bubbles are disengaged; in the other very many.

Some years ago, when engaged in experiments on the blood, especially in relation to the present question—the condition of the alkali in it—I noticed the effect of cream of tartar in expelling carbonic acid, and that both from venous and arterial blood, and from serum; an effect which, with other considerations, induced me then to conclude, that the soda in the blood exists in the form of the sesqui-carbonate; an inference which appears to me still to be most in harmony with the facts.*

In opposition to this view, perhaps, it may be said, that farther proof of its correctness ought to be afforded by the effect of a solution of muriate of lime on the serum,—that, if the latter contain the alkali as stated, a precipitate of carbonate of lime ought to be the result. This experiment I have tried, with the aid of the air-pump, sometimes with a doubtful result, sometimes with a negative one, especially in the instance of serum from venous blood. But in these instances I have also found the result the same, even on the addition of a portion of sesqui-carbonate of soda, as much as $\cdot 2$ of a grain to 316 grs. of serum—a quantity of the alkali, which, when dissolved in the same bulk of water, is more than sufficient to give a precipitate with muriate of lime. Would not this seem to indicate that in the blood and its serum the carbonated alkali is in a peculiar state of combination with the animal matter; and the same remark is applicable to the phosphates or their elements.

The trials referred to have been made on the blood and serum of the ox and sheep, at a favourable time of the year, during the winter season, when the temperature of the air has been little above the freezing point.

2. *On the Viscid Quality of the Blood Corpuscles.*

That the corpuscles of the venous blood of the mammalia, when quite fresh, and in the act of coagulating, collect together in piles, as it were by a kind of attraction, is well known. The viscid, adhesive quality, I am about to notice, is distinct from this, and, indeed, is best seen when the aggregation in piles ceases to be witnessed, as in cruor, procured by breaking up the crassamentum, and separating the fibrin by straining through linen.

The cruor thus obtained is essentially a semifluid, the particles loosely adhering forming a mass in some respects not unlike honey or molasses. I shall notice some appearances connected with and indicating the condition referred to.

When poured into a fluid, such as water or serum, it rapidly falls to the bot-

* *Physiological and Anatomical Researches*, ii. p. 152.

tom ; and, in the instance of serum, if not agitated, remains as a connected mass. If now a glass rod be put into it, and withdrawn through the supernatant serum, it will come out not sensibly coloured by the red particles ; the surface of the cruor round the rod will be seen to be raised a little in the act from adhering to it, and then to return to its former level, shewing that the corpuscles adhered to each other in the mass more strongly than to the glass : and, if the serum through which the rod has been drawn is examined with the microscope, a small number only of blood corpuscles will be detected in it.

If, instead of allowing the cruor to remain undisturbed, it be broken up by agitation with the serum, it will be found to be divided into clusters of corpuscles and detached particles. When one of these clusters is placed under the microscope, between two plates of glass, the adhering corpuscles forming the group are seen to be attached, not by their broad or concave surfaces, as in the instance of aggregation by piles, but by their narrow rims. Now, if graduated pressure be employed, so as to break up the cluster, just before separating, the adhering corpuscles will be seen to be elongated, as if drawn out almost to a fibre, and yet when detached, the adhesion being overcome, recovering, and that suddenly, their circular form : and, on relaxing the pressure, many of them will be seen to reunite, sticking to each other even when in motion.

This adhesive quality of the blood corpuscles is exercised, not only on each other, but also on other substances, though, perhaps, in a less degree. Proof of this is afforded when cruor has been allowed to remain, even but a short time, in a glass tube, or any other vessel. The portion in contact with the bottom of the tube is found to adhere to it, and is not easily detached ; whilst any that may adhere to the sides commonly appears in streaks, the blood corpuscles being attached to each other, and so producing a linear arrangement.

This viscid property of the blood corpuscles must, I apprehend, be considered as specially belonging to them, quite distinct from the fibrin, which appears to be viscid only in its transition state, in the act of coagulating,—previously even more liquid than serum attenuating the blood, and subsequently, as soon as coagulated, constituting the firmest and the cementing part of the crassamentum. The blood corpuscles, as regards this quality of viscosity, are far more constant ; it belongs to them when fresh, probably when circulating in the vessels,—it is exhibited in them long after removal from the living body, and is not even lost with incipient putrefaction, and, connected with that, the change of the particles to a globular form.

3. *On the Tendency of Fibrin in Coagulating to a certain arrangement of its Particles.*

Amongst the many remarkable properties of coagulable lymph, I am not aware that a tendency of its particles to arrange themselves in a certain manner out of the body, representing, as it were, what takes place in the body in the process of growth and of reparation, has hitherto come under observation, or, at least, has been the subject of commentary.

A striking instance of the kind I have witnessed in the buffy coat. When the buffy coat is well marked, as in cases of acute rheumatism, when it is thick and cupped, the blood abstracted having been slow in coagulating, it is easily detached from the soft crassamentum; and this is best done under water. Thus separated, it may be described as a fibrous mass loaded with serum, enveloped in a pellicle or membrane, performing the part of a sac. This pellicle, or containing membrane, is very thin, yet of considerable strength, and with care may be dissected off, especially after maceration in water for two or three days, at a low temperature. It is very like a serous membrane, both as seen with the naked eye, and under the microscope. Under the latter, it bears a strong resemblance to the arachnoid, appears as a tissue of extreme delicacy; hyoloid, without any visible pores or fibres, with a few particles like blood corpuscles, or their remains (according to the method used of separating it), scattered through it. When a force is applied to it, it breaks less readily in one direction than another; and exhibits, when drawn in one direction, more elasticity than in the opposite. When the blood, as is usual, has been received in a circular vessel, and the buffy coat, of course, is of the same form, tearing the membrane with a forceps towards the margin, shreds of it, several lines in length, are easily detached in a line from the centre to the circumference, but not in a line at right angles to this; and in the same direction small portions of the membrane exhibit considerable elasticity, which they do not in the opposite direction.

I may mention another example, also well marked. If the blood, in the act of coagulating, is stirred with a glass rod, or a wooden skewer, or the like, the fibrin, as it is well known, will adhere, with which blood corpuscles will be mixed. The adhering clot, consisting of the two, the fibrin in excess, when pulled off, which it easily is, exhibits a canal with a smooth inner surface. If it be well washed to deprive it of colouring matter, and slit open, it will be found to bear a close resemblance to an artery, especially to its middle coat, being composed of fibres arranged seemingly transversely, that is, at right angles to the axis of the tube. This is to be inferred from the effect of a force applied. If applied in that direction, transverse shreds pretty readily separate; but if in the opposite direction, using a forceps, only small bits. And, in the one, the transverse direction,

the tube is far more elastic than the other, after the manner of the middle arterial coat.

Other instances might be given, tending to shew the same disposition on the part of coagulable lymph to a certain regular arrangement of its parts, as it were, of a *nisus formativus*, in the act of coagulation. In examining the buffy coat, or the fibrinous masses which are so commonly met with after death in the right cavities of the heart, it is not uncommon to find in them, when divided, cavities containing serum resembling cysts. And in the ventricles of the heart, and the aorta and the principal veins, especially the iliac and femoral, fibrinous concretions, as it is well known, are often found after death from lingering diseases, in which a puriloid matter is contained, as in a sac,—a matter which has been imitated by Mr GULLIVER, by the coction of lymph, at about the temperature of the human body, and which, previous to his experiments, had been considered as pus, and, erroneously, as the product of inflammation.

I would ask in conclusion, is not this disposition of coagulable lymph called into play in other occasions during life, and may it not serve to explain certain appearances which are commonly accounted for in a different manner, such as the cysts which so rapidly form in the instance of aneurisms, the consequence of wounds, and the lining membrane of the sacs of false aneurisms, which is hardly in appearance distinguishable from the inner coat of the artery with which it is continuous?

4. *On the Effect of Serum in promoting the Coagulation of Milk.*

There is a marked difference, as is well known, between the albuminous part of the serum of the blood and that of milk,—ordinary cow's milk,—viz., that, whilst the former is coagulated by a temperature below the boiling point of water, the latter, in its fresh state, is not so affected, even by ebullition, but, on the contrary, has its natural tendency to coagulate, connected with the absorption of oxygen and the formation of an acid, retarded. *A priori*, perhaps, it would hardly be expected, as regards the property of coagulation, that the one fluid mixed with the other would have any material effect. But that it is not so, I have found on trial. Milk, I find, when mixed with serum in certain proportions, is coagulated by heat. I shall notice some results obtained, using mixtures of the serum of the blood of the sheep, which coagulated at about 170° Fah., and cow's milk.

Equal parts of the two remained liquid at 170°, and coagulated about 175°. The coagulum was of an opaque white, very little softer than the coagulum of the serum alone. Mixed with water it did not render it milky; and the watery infusion was not rendered turbid by acetic acid, and only in a very slight degree by the nitric acid.

One part of serum and three parts of milk in mixture, coagulated at about 190°, forming a soft tremulous mass, which, by boiling, was rendered firmer. Broken up and mixed with water and filtered, the fluid had the properties of weak whey, and was not rendered turbid by acetic acid.

A mixture of 1 part of serum and 4 of milk did not coagulate after several minutes boiling; but, keeping it on the fire, in about a quarter of an hour the effect was produced. The coagulum formed was very soft. Mixed with water, it rendered the water turbid at first; but after a while, the finer particles subsiding, left the water clear, and it was not affected by acetic acid.

A mixture of 1 part of serum and 5 of milk, immersed in boiling water, at least an hour, was found coagulated. The coagulum was of the consistence of very soft custard. A like result was obtained, using a mixture of 1 of serum and 7 parts of milk.

Lastly, a mixture of 1 part of serum and 10 of milk was liquid after more than an hour's boiling; but after about three hours' boiling, when a portion of its water was expelled, it was found coagulated.

As in these instances, the serum promotes the coagulation of the milk, so also the latter may be considered as favouring the coagulation of the former; that is, viewing the milk as a diluent of the serum, having the effect of removing further apart its albuminous particles, and comparing it as a diluent with water; two parts of which, I find, with one of serum, prevent the coagulation of the latter, even at the boiling point, and thus heated, for many minutes.

The property exhibited in the foregoing experiments is to be witnessed in many other instances; it may be, probably, in every instance in which liquid casein and albumen are heated together in certain proportions. Mixtures of white of egg and milk exhibit it even more strongly than serum and milk. With equal parts of milk and white of egg, a firm coagulum is formed on boiling, the fluid exuding from which is almost transparent, and is not sensibly affected either by acetic acid or by the nitric acid. With 1 of white of egg and 5 of milk a coagulum is formed of moderate consistence; and with 1 of the former and 10 of the latter a very soft coagulum, after immersion in boiling water for about a quarter of an hour. I give these results, because, in no work on chemistry, with which I am acquainted, have I met with any account of the effect of white of egg on milk, even in culinary processes. Using the yolk of the egg in place of the white, the effect on milk has been, as might be expected, very similar; the chief difference in the coagulum being, that it has been somewhat softer.

From similarity of composition, it might be inferred that the roe of fish, mixed and boiled with milk, would have the effect of coagulating it; and this, I find, is the case. The only other animal substance I have tried has been muscle; it, triturated and boiled with milk, did not coagulate it; the muscular fibres, it may be noticed, were found collected together in a mass in a singular manner.

Extending the analogy to vegetable substances, it seemed likely that all those which contain albuminous matter, similar to casein, may be affected when boiled with serum or white of egg, in the same manner as milk; and the result of a trial, with a mixture of serum and a strong emulsion of sweet almonds, has confirmed the inference. After boiling, the fluid expressed from the coagulum was not precipitated by acetic acid.

As the serum of the blood of even the same species of animal is liable to slight variations, affecting the degree of temperature at which it coagulates, and, as milk is subject to some variation, in regard to the same quality, as indicated by the effect of rennet—a variation, perhaps, most of all depending on season of the year, and the time that the milk has been exposed to the atmosphere—should the experiments I have described be repeated, some little difference in the results may perhaps be perceived, depending on the circumstances just referred to.

The action of one animal fluid on another, and those so similar as regards their albuminous part, as milk and serum, offers curious matter for speculation, and may be deserving of special attention, not only in relation to the culinary art, and processes of manufacture in which vegetable juices are concerned, but also in connexion with physiology. It may be found that the principle of rennet exists in the blood, and that the analogous power of both, as regards the coagulation of milk, depends on the same cause; and, if so, then it may be farther deserving of attention, in connexion with pathology and processes of morbid softening.

THE OAKS, AMELESIDE,

February 17. 1845.



VIII.—*On the Advantages to be derived from the Use of Metallic Reflectors for Sextants and other Reflecting Instruments; and on Methods of directly determining the Errors in Mirrors and Sun-Shades used in Reflecting Instruments.* By JOHN ADIE, Esq.

(Read February 17. 1845.)

It has frequently occurred to me that the difficulty experienced by instrument-makers, in obtaining for sextants and other similar instruments, reflecting mirrors perfectly parallel in their polished surfaces, and also the greater difficulty of procuring glass perfectly homogeneous in its structure, might be overcome by the use of metallic reflectors.

It is well known, that, from the want of perfect parallelism in glass mirrors, there arises an error in the reading of such instruments, inasmuch as the emergent ray does not pass out of the glass at the same angle as the incident falls upon it, and that from the want of homogeneousness in the substance, and the unequal refractions caused by the veined structure of the glass.

Whether this structure of the glass arises from the process of its manufacture, or the want of proper admixture of the component parts, before being cast into plates, I am not prepared to say; but in all the plate-glass I have tried, by polishing it on the edges, this structure was observed; so much so, that the plates, on being seen through perpendicularly to the plane of their surface, shewed objects perfectly distinct; while objects, when viewed through the glass at right angles to this plane, were seen with difficulty, distorted and twisted in all directions. Of such glass, the mirrors of sextants, and other reflecting instruments, are made; and it is easy to conceive how very erroneous the angles may be, particularly when the incident ray falls on the mirror at a low angle, as it does when large angles are observed, as in lunar distances and the like; while the indistinctness of the image observed under these circumstances, detracts much from the utility of instruments fitted with such mirrors. As a practical illustration of the above, if we take a number of objects, and observe with a sextant the angles between each, then observe the angle between the extremes, suppose this 120° , it will be found, in the great majority of cases, that the sum of the angles observed does not agree with the observed angle of the extremes, which should be the case.

These errors, and sources of error, are obviated when we make use of metallic reflectors, having their surfaces polished perfectly flat; a matter of no very difficult attainment in practice.

But, besides the avoidance of error, there are direct advantages in the use of such reflectors, which may be stated thus:—In the marine sextant, or reflecting circle, the reflection of faint objects is more easily obtained; in other words, objects are seen reflected by metallic mirrors which cannot be seen by the ordinary silvered ones. Another advantage is, that larger angles can be observed. This applies more particularly to the pocket or box-sextant, used in surveying, both at sea and land. From the small size of the index mirror, we cannot, when glass is used, reflect an angle much above 100° , the thickness of the glass cutting off the incident and emergent rays, when these fall on the silvered surface at low angles; whereas, with the metallic reflector, the reflected angle can be obtained to its utmost limit, or to about 140° , being nearly one-half greater than that which can be obtained by means of a silvered glass reflector.

I am not aware of any account having appeared of the use of metallic mirrors heretofore in the construction of such instruments, although I have little doubt, from the obvious advantages attending them, that the idea must have suggested itself to many others; and that the fact of their not having been brought into use, must be accounted for from the difficulty of obtaining speculum metal possessed of the requisite qualities.

The liability of a highly-polished reflecting surface to be destroyed by tarnish and rust, from exposure to the atmosphere, and more particularly from exposure to the influence of sea air, is an objection that occurs on first view to the use of metal.

All who are acquainted with the reflecting telescope, know how subject the mirrors of such an instrument are to deterioration from tarnish; and that, in many cases, even when due care has been taken of them, they have been altogether destroyed. Yet, it should be stated, that this is not the case with all such instruments. There are many reflecting telescopes, now very old, in which the mirrors are in a state of perfect preservation; those I am best acquainted with, as having stood the test of time (and they are in general very good), having been made by the late James Short of London, who lived about eighty years ago. On the other hand, it is well known, that many speculum metals will not retain their lustre for many weeks under ordinary exposure.

From these facts, we may infer, that it is the composition of the metal which causes the difference in the permanency of the polish.

My attention was therefore directed to procuring pure metals to form the alloy or speculum metal. Tin is not difficult to be had in a state of great purity; but it is otherwise with copper; for, as we advance in commerce, we find, that, day after day, this metal is brought to market more and more impure; so much so, that bar and cake copper of commerce are now so bad that they are nearly unfit for compounding as brass.

The recently discovered process of electrotpe, however, affords us the means

of easily procuring copper in a state of purity; and it is with the metal so procured my experiments have been made.

By compounding copper and tin in their atomic proportions of 16 parts of copper to 14.92 parts tin, a metal of high lustre is obtained; and, so far as my experiments have gone, this metal is not liable to tarnish, if ordinary care be taken to guard against this effect. My course of procedure was as follows:—I first exposed polished pieces of this metal to the free open air, and found, after some months' exposure, that, when the dust and rain stains were rubbed off, the surface was in a very good state of preservation. I next tried exposing the mirrors to the fumes of acids, and watering them with sea water for a considerable length of time. Under the operation of these corrosive agents, they still retained their lustre. But, notwithstanding the encouragement held out from these experiments, feeling yet reluctant to put such instruments into the hands of navigators without first submitting them to the test of actual service; and a favourable opportunity having last spring presented itself, through the kindness of Mr O. MOSSMAN, one of the surveyors on board of H.M. Ketch, Sparrow, then about to engage in a survey of the Pentland Firth, I put into the hands of that gentleman a sextant fitted with these metallic reflectors, of which he politely took charge, promising to give it a fair trial during a season's survey. Mr MOSSMAN amply redeemed his promise; and I shall now take the liberty of quoting the letters which he was good enough to address to me, giving an account of the working of the instrument.

“H.M. KETCH, SPARROW,
THURSDAY, 26th October 1844.

“DEAR SIR,—After you have examined the reflectors you will be able to judge of the durability of them, after being in constant use for most part of the season, during which, they have been exposed to all sorts of weather. As regards the power of their reflection (although dark), they are beyond all the silvered glasses I have ever had in use; also, for measuring large or small angles, they excel the others by far. When we are once properly settled at Portsmouth, I shall be able to make a full descant of the good properties of your metallic reflectors, and shall strongly recommend them to be in all reflecting instruments. I have subjected them to all exposure which I could call fair play, only, at the same time, having been careful not to put the instrument away damp.—Yours truly,

(Signed) “WM. O. MOSSMAN.”

In a second letter, dated Portsmouth, 13th December, Mr MOSSMAN says:—“I am sorry my spare moments are rather scarce, or I should have said something more concerning the merits of your metallic mirrors. However, I should very strongly recommend them to be used in all instruments that are likely to be exposed to much wet; because, if the instrument fall overboard, for instance, and

does not lie so long in the water as to allow the rust to commence, if it be carefully wiped dry, when taken out, there is no danger of spoiling afterwards. Now, this is a very important matter, and more particularly to surveyors, than any other class of nautical men ; for such accidents frequently occur in boat-soundings."

I come now briefly to notice the second subject announced in the title of this paper, viz., direct methods of determining the errors in the mirrors and sun shades used in reflecting instruments. I am not aware of any method adopted by practical men for the discovery of such errors, except that of a careful process of what is termed parallel grinding, and testing the glasses in the instrument when fitted up. This is effected by observing known angles, and noting that a contact of the sun's limb, by reflection, does not vary on changing the sun shades interposed between the direct and reflected images seen in the telescope. This variation is noticed by Mr M'KAY, in his work on Determining Longitude, and he recommends that it should be observed and applied as an index error affecting the several shades. Errors may, however, exist in the reflectors and shades, which, from the particular position they have in their settings, are not discovered by such trials. The methods I have adopted are as follows :—

First, For the mirrors I place a mirror at about an angle of 45° before the object-glass of a telescope, mounted on a divided circle, capable of reading an angle of $10''$; I have, besides, a moveable micrometer wire at the stop holding the cross wires of the telescope, by which an angle of one second may be observed ; the mirror placed before the object-glass rests on three smooth studs, to which it is pressed up by a light spring at the back. In this position we turn the telescope and mirror in azimuth till we obtain the reflection of a well-defined distant object, which is brought to the intersection of the cross wires of the telescope. If we now turn round, or reverse the mirror on these studs, and find the same object in the intersection of the cross wires, we know that in that line there is no want of parallelism ; and if we have the same result on repeating the trial at right angles to the first direction, the mirror is said to be perfect in respect to the parallelism of its surfaces. If, however, we find on turning the mirror that the reflected object is not intersected by the cross wires, then, the glass is not parallel ; and half the amount of error read by the circle or micrometer head, is the error which would arise if such a mirror were applied to a sextant.

Second, For the sun shades I have a telescope whose object-glass is divided, and the one-half moved over the other by means of micrometer screws, having a divided head. The value of the divisions of the micrometer head is obtained by measuring the sun's diameter. In that which I use, two divisions on the head are equal to one second.

Before one-half of the object-glass is placed a fixed sun shade ; and before

the other, an arrangement is made for placing the shades to be tested in such a way that they can be turned round. Having placed in this frame a shade for trial, I bring the two images of the sun seen in the telescope into contact, by means of the micrometer screws; and if, on turning the shade before the object-glass, the contact remains perfect, then we know that its surfaces are parallel; if, on the other hand, the contact is broken, we can, by turning the shade, find the point of nearest contact; and by turning the shade through 180° , we measure the amount of opening of the images, by means of the divided micrometer head. This gives double the error which would be caused by the shade when used in making a direct observation. But as all angles observed by reflecting instruments are double, or, in other words, the divisions on the limbs of reflecting instruments are equal only to half those of an instrument used for direct observation, the double error given by this method goes all to deteriorate the observation, as an unparallel shade affects the angle when reflected by twice the amount of direct observation. Another important object is gained by this method of testing the shades. It being a very difficult matter to obtain these altogether perfect, I reject all where the error would amount to $10''$, which is the usual reading of a sextant; and when an error of a smaller amount does exist, I find, by turning the shade, the line in which the want of parallelism exists; and by placing this line at right angles to the plane of observation in a reflecting instrument, the angles observed by such an instrument are not affected even by the small error of the shade.

IX.—*On the Balance Magnetometer, and its Temperature Corrections* By J. A. BROWN, Esq. Communicated by SIR T. M. BRISBANE, Bart.

(Read 21st April 1845.)

1. THE Balance Magnetometer was imagined by Dr H. LLOYD, of Dublin, for the purpose of observing the variations of the vertical component of the earth's magnetic intensity. It consists simply of a balanced magnetic needle, with a knife-edged axle, resting on agate planes, at right angles to the plane of the magnetic meridian. In the instrument from which the results in this paper are deduced, the position of the needle is observed by means of micrometer microscopes.*

2. If m be the moment of free magnetism of the needle, Y the vertical component of the earth's magnetic force, W the weight of the needle, g the distance of the centre of gravity from the centre of motion, ϵ the angle contained by the line joining these two centres, and the magnetic axis of the needle when horizontal; the equation of equilibrium will evidently be

$$m Y = W g \cos \epsilon \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

By differentiation and division

$$\frac{\Delta Y}{Y} = \tan \epsilon \Delta \epsilon - \frac{\Delta m}{m} \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

The differences $\Delta \epsilon$ are obtained by means of the micrometers, and the differences of Y in terms of Y will be obtained, if we can determine ϵ and $\frac{\Delta m}{m}$, the latter being the variation of the magnetic moment, due to temperature.

3. There are great practical difficulties in the way of rendering the needle capable of giving ϵ accurately by inversion, but Dr LLOYD has shewn† that

$$\tan \epsilon = \cot^2 \theta \frac{T'^2}{T^2} \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

where θ is the magnetic dip, T' and T the times of one vibration of the balance needle in a horizontal and in a vertical plane. We have thus, instead of one, three unknown quantities to determine; and it becomes a matter of importance to shew with what degree of accuracy this may be done.

The dip and time of vibration in a horizontal plane can be obtained with sufficient truth for the purposes of this factor.

* See the Introduction to the Makerstoun Magnetical Observations for 1841-2.

† In his "Account of the Magnetical Observatory of Dublin," where the complete investigation will be found.

Observations of the time of vibration in a horizontal plane ranging through a period of three years, agree within 0.06^s , and this difference must be to a considerable extent due to alterations in the condition of the needle between the different observations.

4. The time of vibration in a vertical plane is in widely different circumstances.

These differences I shall proceed to point out.

1st, The time of vibration in a vertical plane is found increased *after* the needle has been, by any means, vibrated through a large arc.

The strongest evidences of this are contained in the following table; they were obtained either by iron having been brought accidentally near to the magnet, or by the necessary removal of the box which covers it. In the latter case, the magnet was vibrated through large arcs by currents of air.

One or two observations for the time of vibration are given for the periods immediately before and after the disturbance of the magnet.

The last column contains the times of vibration corrected to 50° Fahr.; for reasons that will be shewn, it is only these that are strictly comparable.

The observed time of vibration is generally the mean of two series, which rarely differ one-tenth of a second.

Additional evidence of the above conclusion is furnished by Table II.

TABLE I.

Observations for the Time of Vibration of the Balance Needle in a Vertical Plane, before and after excessive Vibrations.

DATE.	CAUSE OF DISTURBANCE.	Observed Time of one Vibration.	Temperature of Needle.	Time of one Vibration corrected to 50°.
1842.		S.	O.	S.
March 19.	Balance magnet vibrated excessively.	10.14	45.5	10.48
... 22.				
April 2.		11.02	42.5	11.59
... 9.		10.94	44.0	11.40
Oct. 31.	{ A pair of compasses brought inadvertently near the balance needle.	10.05	55.9	9.60
Nov. 10.				
... 11.		10.34	51.0	10.26
... 19.		10.02	50.8	9.96
1843.				
Sept. 18.	{ The box of the magnetometer removed, and the needle exposed to currents of air.	10.60	65.0	9.46
... 22.		10.62	63.7	9.58
... 26.				
... 27.		11.20	51.9	11.06
... 29.	{ Box lifted for the purpose of removing an insect, and replaced immediately afterwards; vibration not excessive.	10.95	46.1	11.40
1844.				
April 29 22		9.04	50.2	9.02
... 30 7		9.50	60.5	8.70
... 30 7+	{ Box lifted, and insect removed from beside the needle.			
... 30 8		10.03	60.5	9.23
... 30 22		8.97	50.5	8.93
July 22 0		8.23	62.3	7.30
... 23 22	{ The magnet vibrated by steel.	9.10	63.0	7.73
... 24 18				
... 25 0		9.80	67.2	8.49
... 25 23		9.67	64.5	8.57
... 26 3	{ Workmen in observatory, who had probably been near the magnet with a hammer.			
... 26 23		10.04	65.4	8.87
... 29 22		9.45	53.5	8.80
Oct. 30 22		6.97	47.9	7.13
Nov. 3 22	{ Workmen brought a hammer near the magnet. After the vibration thus produced, the needle rested in a position differing 1'.6 from its previous position.*	6.72	43.9	7.18
... 3d-7 ^d				
... 7 23		7.30	42.5	7.87
... 8 21				
... 9 1	{ Workmen brought a hammer near the magnet. After the vibration thus produced, the needle rested in a position differing 1'.6 from its previous position.*	8.24	46.8	8.48
... 10 23		8.03	43.7	8.51

5. 2*d*, The time of vibration in a vertical plane depends, to a considerable extent, on the magnitude of the arc of vibration.

3*d*, For the same arc, the time of vibration is greater, if it belong to a series

* This is the only case in which I determined, at the instant, the effect of excessive vibration on the position of the needle; the effect, though small, is considerable, when compared with the hourly changes; for several hours before this vibration, the magnet had not changed its position.

commencing with a large arc, than if it belong to a series commencing with a small one.

These conclusions I had arrived at nearly two years ago, and accordingly only small arcs were used in determining the time of vibration, seldom above 5'.0 commencing.

The following series of observations was made in January 1844, before removing the needle for the purpose of determining its temperature correction. Many other series made previously give the same result; but the following will be sufficient to prove the facts stated above.

TABLE II.

Observations for the Time of Vibration of the Balance Needle in the Vertical Plane, for different Arcs.

Time at the commencement of each Series.	Semi-arc of Vibration.		Number of Vibrations.	Means.	
	Beginning.	Ending.		Partial.	Of the Series.
Jan. 26 22 15	1.8	0.4	14		9.58
22 25	1.4	0.4	14		9.58
22 40	25.0	18.6	6	11.15	
	18.6	6.5	6	10.98	10.95
	6.5	0.5	8	10.71	
22 53	1.3	0.4	16		9.78
23 20	1.6	0.4	16		9.70
23 32	45.5	25.0	6	11.36	
	25.0	11.3	8	11.20	
	11.3	6.5	6	11.07	11.07
	6.5	2.1	6	10.95	
	2.1	0.5	6	10.78	
23 50	1.7	0.4	14		10.17
Jan. 27 0 5	55.0	40.0	6	11.72	
	40.0	30.0	6	11.60	
	30.0	22.0	6	11.57	
	22.0	17.0	6	11.45	
	17.0	12.3	6	11.37	11.35
	12.3	5.8	6	11.33	
	5.8	5.5	6	11.22	
	5.5	4.5	6	11.09	
	4.5	0.7	6	10.80	
0 20	1.7	0.4	18		10.60

The semi-arcs were observed by my assistant Mr WELSH, at one microscope, while the times of each vibration were observed by myself at the other.

It is not my intention, in the present communication, to enter into any examination of the causes of these peculiarities; my object is simply to point them out as sources of error. I shall therefore merely state my conclusions, with their evidences.

6. 4th, The time of vibration in a vertical plane depends, to a considerable extent, on the temperature of the needle.

The following short series, taken at random from a great number of observations, at once prove the truth of this conclusion. From a comparison of a few of the observations, it was found that an increase of 1° Fahr. was equivalent to an increase of 0.076 in the time of vibration.

The last column for each series gives the times corrected by this quantity to 50° Fahr. That the correction obtained is only approximate, will, together with errors of observation, account for much of the discrepancies in the corrected quantities.

TABLE III.

Observations for the Time of Vibration of the Balance Needle in the Vertical Plane at different Temperatures.

Time of Observation.	Observed time of one Vibration.	Temperature of the Magnet.	Time of one Vibration corrected to 50° Fahr.	Time of Observation.	Observed time of one Vibration.	Temperature of the Magnet.	Time of one Vibration corrected to 50° Fahr.
1844.				1845.			
d h	s	$^{\circ}$	s	d h	s	$^{\circ}$	s
Jan. 2 22	8.91	31.4	10.32	Jan. 20 23	7.21	35.4	8.32
3 2	9.48	40.0	10.24	23 23	8.38	46.3	8.66
3 4	9.80	43.5	10.29	26 22	7.42	35.9	8.49
3 5	9.96	45.2	10.32	30 2	6.72	27.0	8.47
3 22	9.74	41.4	10.39	31 2	6.42	21.5	8.59
				Feb. 4 22	7.64	38.7	8.50
April 30 22	8.97	50.6	8.92	12 2	6.93	32.6	8.25
May 1 8	9.77	64.1	8.70	16 23	7.49	38.9	8.33
1 22	9.28	56.3	8.80				
2 21	9.13	55.2	8.73	Mar. 9 22	7.43	41.0	8.11
3 8	9.81	64.4	8.72	12 23	6.87	31.6	8.27
3 23	9.02	53.6	8.75	13 22	7.08	31.9	8.46
				16 22	6.82	31.2	8.25
				23 23	8.07	45.6	8.40
				April 1 11	8.10	48.7	8.20

It should be remarked, that the series for January 1844 is not comparable with the following series, as an adjustment of the instrument occurred in that month; neither, indeed, are the other series comparable with each other, from the circumstances given in Table I.

7. To take one of the most marked cases from this table, it will be seen that the observed times of vibration on January 23d and 31st 1845, differ nearly two seconds, while the corrected times do not differ one-tenth of a second.

8. While an inequality in the expansion of some parts of the needle would alter its sensibility by elevating the centre of gravity, it seems very doubtful if there is any thing in the form of the needle which is at all likely to render this supposition sufficient. An alteration in the position of the centre of motion would produce a like effect; and as the position of the needle depends, to some extent, on its temperature, it is necessary to shew whether position or temperature only is the cause of the differences in the times of vibration. Had the readings for the position of the needle been given with Table III., it would have been evident from these alone that the differences were *not* due to differences of posi-

tion. The following series of observations made during a magnetic disturbance, will, however, prove it more distinctly.

TABLE IV.

Observations for the Time of Vibration of the Balance Needle in a Vertical Plane, the position of rest varying.

Göttingen Mean Time of Observation.			Balance Magnetometer.		Time of one Vibration.	
			Reading.	Thermometer.	Observed.	Corrected to 50° Fahr.
d	h	m	Mic. Div.	°	s	s
April 15	22	52	—148	47.7	8.84	9.01
17	1	50	+101	52.5	9.21	9.02
	2	20	+4	53.2	9.46	9.22
	3	15	+25	54.2	9.46	9.14
	3	25	+23	54.7	9.36	9.00
	5	15	—18	56.4	9.62	9.13
	7	45	—10	56.3	9.66	9.18
	8	45	—90	56.0	9.63	9.22
	10	20	—190	55.2	9.42	9.02
	10	30	—176	55.2	9.62	9.22
	13	40	—310	54.3	9.23	8.90
	13	50	—293	54.3	9.35	9.02
	22	15	—185	51.1	9.23	9.20
	22	25	—185	51.1	9.33	9.30
18	22	30	—134	50.3	9.01	8.99

The positive and negative signs indicate that the north pole of the needle was below or above the horizontal. It would have required a change of 50° Fahr. to have produced *alone* a difference of 400 micrometer divisions. Such a change of temperature, according to § 6, would have been equivalent to a change of 3.8 in the time of vibration. The observed times differ only a few tenths, and the times corrected for temperature agree within the limit of the errors of observation.*

9. It results from these facts, that the time of vibration in a vertical plane cannot be used at present in the reduction of the observations, as theory takes no account of them. The theoretical corrections for differences of arc or the variation of the moment of inertia due to temperature would, in the examples given, be inappreciable.

10. I shall now consider $\frac{\Delta m}{m}$, the temperature correction for the position of the needle.

The method which has been adopted for its determination is as follows:—

The magnet, whose temperature correction is to be obtained, is placed at right angles to a magnet freely suspended, which is thus deflected by an angle u from the magnetic meridian. If m be the magnetic moment of the deflecting magnet,

* The time of vibration throughout the year varies from other causes. The law which regulates these variations I have not yet determined.

and X the horizontal component of the earth's magnetic force, the equation of equilibrium is

$$m = X \sin u \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

The variations of u are observed, while the deflecting magnet has its temperature altered 30° or 40° Fahr., by means of hot or cold water; by differentiating equation (4) and dividing by it, these variations are connected with $\frac{\Delta m}{m}$ by the equation.

$$\frac{\Delta m}{m} = \cot u \Delta u \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

X and the magnetic declination being constant.

11. The chief objections to this method are the following :—

1st, The circumstances under which the magnet is placed are considerably different from its usual condition. It is necessary to raise or lower the temperature 30° or 40° in water, within a few minutes, to obtain satisfactory results, whereas the most rapid changes in the magnetometer-box will probably be under 2° in an hour. It seems doubtful to me whether it has been proved that the changes of magnetic moment occur as rapidly as those of temperature in all cases.

2d, In the event of there being any other source of error due to temperature, it is altogether omitted by this method.

3d, If the correction has not been determined before adjusting the instrument, the series of observations is broken up by the necessity of removing the needle.

12. As it is desirable that the observations of the balance magnetometer should be made as valuable as possible, I shall proceed to consider how this may be best done, as it is my opinion that they will be found ultimately capable of giving diurnal and annual changes with considerable fidelity.

13. The observations of $\Delta \epsilon$, the varying angle formed by the needle and the horizontal, will at present obviously give comparative observations for the variations of vertical force, without reference to the value of the coefficient $\tan \epsilon$, until a good approximate value of the latter can be obtained, *if* the observations in micrometer divisions can be corrected for temperature. In order to do this, it would be necessary to convert the value of $\frac{\Delta m}{m}$, obtained by deflection experiments into micrometer divisions, if this value be q .

$$q = \frac{\Delta m}{m} \tan^2 \theta \frac{T^2}{T'^2} \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

We cannot, however, use T , and therefore the method of deflections is, in this way, insufficient; besides, if the alterations in the value of T from temperature should be caused by changes in the position of the centre of gravity, this change

would probably not be altogether in the vertical, the portion resolvable to the horizontal would affect the *position* of the needle.

14. From these considerations I was induced, about two years ago, to endeavour to obtain the temperature correction from the usual daily observations of the instrument. To most persons acquainted with the irregularities in the magnetical variations, from the changes of the magnetic intensity or its direction, this might appear to some extent chimerical, and as at best only capable of giving a rough approximation to, or verification of, the determinations by deflection. It will, however, I think, be shewn, that a better coincidence of partial results, and a better correction, may be obtained from this than from the usual method.

It will not be necessary to point out the methods which were at first tried; I shall proceed at once to those which have been ultimately adopted.

15. Having selected a series of days during which the readings of the instrument seem regular, and in which the changes of temperature from day to day are considerable, rejecting any day of marked disturbance, the hourly or two-hourly readings for the position of the needle and for its temperature are summed for each day. Let us designate the sum of the micrometer readings for the first day of the series y_1 for the second day y_2 , and so on to y_{2n+1} ; the corresponding sums of the thermometer readings being $t_1, t_2, \dots, t_{2n+1}$, the number of the days, from the beginning to the end of the period, being $2n+1$.

The most simple and probable hypothesis that can be formed, is, that the mean vertical force increases or diminishes gradually throughout the period; let the mean daily change be α .

If q be the temperature correction for 1° Fahr. in micrometer divisions, we may form the following series of equations:

$$\left. \begin{array}{lcl} y_1 = y_2 + \alpha + (t_1 - t_2) q & y_2 = y_3 + \alpha + (t_2 - t_3) q & \\ y_1 = y_3 + 2\alpha + (t_1 - t_3) q & y_2 = y_4 + 2\alpha + (t_2 - t_4) q & \\ \dots & \dots & \\ y_1 = y_{n+1} + n\alpha + (t_1 - t_{n+1}) q & y_2 = y_{n+2} + n\alpha + (t_2 - t_{n+1}) q & \\ \dots & \dots & \\ y_{n+2} = y_{n+3} + \alpha + (t_{n+2} - t_{n+3}) q & & \end{array} \right\} (7.)$$

There will be breaks in each series, as there are no sums for the Sundays. As t_2 may be greater than t_1 and t_3 , the result of the comparison of y_1 with y_3 is not equivalent to the comparison of y_1 with y_2 and y_2 with y_3 .

From these equations the most probable values of α and q might be obtained by the usual methods; but the labour which they demand is probably much beyond the greater accuracy to be attained. The following, it is conceived, will be found sufficient.

First classing the equations in which $t_p >$ or $<$ t_{p+r} , and considering each class separately.

Placing the equations in the form

$$\frac{y_p - y_{p+r}}{t_p - t_{p+r}} = \frac{r \alpha}{t_p - t_{p+r}} + q \quad . \quad . \quad . \quad . \quad (8.)$$

Naming the differences in which $r = 1$, Δy_1 , and Δt_1 , in which $r = 2$, Δy_2 , and $\Delta t_2 \dots \Delta y_n$, Δt_n . Summing separately all the equations for Δ_1 , all those for $\Delta_2 \dots$. It will simplify the investigation, and be sufficiently accurate to take for the divisor of $r \alpha$, the mean of all the values of Δt , naming this Δt_0 .

We obtain the following equations:

$$\left. \begin{aligned} \frac{\Sigma \Delta y_1}{\Sigma \Delta t_1} &= q + \frac{\alpha}{\Delta t_0} \\ \frac{\Sigma \Delta y_2}{\Sigma \Delta t_2} &= q + \frac{2 \alpha}{\Delta t_0} \\ &\dots \dots \dots \\ \frac{\Sigma \Delta y_n}{\Sigma \Delta t_n} &= q + \frac{n \alpha}{\Delta t_0} \end{aligned} \right\} \quad (9.)$$

If the difference of each equation be taken with every one following it, another series of equations of the following form will be produced.

$$\frac{\Sigma \Delta y_p}{\Sigma \Delta t_p} - \frac{\Sigma \Delta y_{p+r}}{\Sigma \Delta t_{p+r}} = \frac{r \alpha}{\Delta t_0} \quad . \quad . \quad . \quad (10.)$$

Summing the equations thus formed, we obtain an equation which may be put as follows:

$$\overline{n-1} \left(\frac{\Sigma \Delta y_1}{\Sigma \Delta t_1} - \frac{\Sigma \Delta y_n}{\Sigma \Delta t_n} \right) + \overline{n-3} \left(\frac{\Sigma \Delta y_2}{\Sigma \Delta t_2} - \frac{\Sigma \Delta y_{n-1}}{\Sigma \Delta t_{n-1}} \right) + \dots = -\frac{\overline{n+1} \cdot \overline{n} \cdot \overline{n-1}}{6} \cdot \frac{\alpha}{\Delta t_0} \quad (11.)$$

Summing equations (9.)

$$q = \frac{1}{n} \Sigma \frac{\Sigma \Delta (y)}{\Sigma \Delta (t)} - \frac{n+1}{2} \frac{\alpha}{\Delta t_0} \quad . \quad . \quad . \quad . \quad (12.)$$

16. The following example, from the Makerstoun observations, will shew the method found most convenient in practice for the summations.

A period of 52 days, from June 1 till July 22. 1843, having been selected as nearly free from disturbances, and containing considerable changes of temperature, the 3d and 7th June being rejected on account of disturbances; the sums for each day of the micrometer and thermometer readings were entered into columns titled Σy and Σt . Each sum was then compared with all the sums up to the 27th day after, and the differences entered into columns titled Δy_1 , Δt_1 ; Δy_2 , Δt_2 ; $\dots \Delta y_{26}$, Δt_{26} . Those differences, the fewest in number, in which $t_p > t_{p+r}$ were marked out, the others summed for each column, and the divisions $\frac{\Sigma \Delta y_1}{\Sigma \Delta t_1} \dots \frac{\Sigma \Delta y_{26}}{\Sigma \Delta t_{26}}$ performed.

From these and equations (11), (12),

$$\Sigma \frac{\Sigma \Delta (y)}{\Sigma \Delta (t)} = 8.338; \quad \frac{\alpha}{\Delta t_0} = 0.0375; \quad \alpha = 2.05; \quad q = 7.832 \text{ Mic. div.}$$

The differences, when $t_p > t_{p+r}$ were too irregular and too few, on some days, to give a good value of a .

17. It is very rare that periods of such magnitude can be found free from considerable irregularities. In general, however, it is conceived that smaller periods will give equally good results, and by a shorter method.

If we consider the equations

$$q = \frac{y_p - y_{p+r}}{t_p - t_{p+r}} - \frac{r a}{t_p - t_{p+r}}; \quad t_p > t_{p+r}$$

$$q = \frac{y_{p+r} - y_p}{t_{p+r} - t_p} + \frac{r a}{t_{p+r} - t_p}; \quad t_p < t_{p+r}$$

it is obvious, that if the temperatures rise and fall considerably throughout the period selected, and no attention be paid to the sign of $t_p - t_{p+r}$ in the summations of the differences, the coefficients of a will nearly destroy each other.

18. In the following cases the sums for each day have been compared with the sums of all the days after it in the period selected. By this means irregularities in the force upon any day have their effect on the final result to a considerable extent destroyed, as it is probable that the results will be as much too great in some cases, as they are too small in others.

The whole differences have been summed without regard to days, and the signs of $t_p - t_{p+r}$ have been disregarded.

The equation is, therefore, simply

$$\frac{\Sigma \Delta (y)}{\Sigma \Delta (t)} = q$$

TABLE V.

Determinations of the Temperature Correction for the Balance Magnetometer, from comparisons of the Daily Observations at different periods.

PERIOD.	$\Sigma \Delta (t)$	$\Sigma \Delta (y)$	q	Time of Vibration corrected to 50° Fahr.	REMARKS.
1843.	°	Mic. Div.	Mic. Div.	s	
Jan. 16—21.	525.3	4315.3	8.21	9.20	In 1843, there were 9 daily observations made at two-hourly intervals, from 5 A.M. till 9 P.M. Sept. 2, the needle was removed, in order to determine its temperature correction by the method of deflections.
23—28.	817.7	5723.5	6.99	9.20	
Jan. 30—Feb. 4.	576.0	4151.5	7.21	9.02	
Feb. 6—11.	609.9	4080.6	6.69	9.25	
June 1—30.	14320.4	114646.9	8.006	9.28	
Sept. 6—16.	1083.7	8730.4	8.04	9.92	
1844.					
May 9—24.	8415.4	66621.7	7.93	8.38	In 1844, there were observations at every hour of the day. The needle was removed between September and February for temperature correction deflections.
Aug. 3—Sept. 6.	21696.9	171460.5	7.902	8.06	
For the series in } 1843.	17933.0	141648.2	7.898		
For all,	48045.3	379730.4	7.903		

From the above table it would appear that neither the removal of the needle and readjustment, nor the alteration of the time of vibration, has affected the temperature correction.

The first values of q shew that periods of a week are insufficient for very accurate determinations.

The mean for 1843 is almost identical with that for 1844.

19. The differences for three periods were also summed without regard to days, but paying attention to the sign of $t-t_{p+r}$. The following table contains the results.

TABLE VI.

Determination of the Temperature Correction for the Balance Magnetometer, regard being paid to the signs of the differences of temperature.

PERIOD.	Preceding temperatures greater than the succeeding.			Preceding temperatures less than the succeeding.			Mean Value of q
	$\Sigma \Delta (t)$	$\Sigma \Delta (y)$	q	$\Sigma \Delta (t)$	$\Sigma \Delta (y)$	q	
1843. June 1—30.	3350.2	Mic. Div. 29096.6	Mic. Div. 8.68	10970.2	Mic. Div. 85550.3	Mic. Div. 7.80	Mic. Div. 8.24
1844. May 9—24.	5404.3	37559.9	6.95	3011.1	29061.8	9.65	8.30
Aug. 3—Sept. 6.	4726.1	34249.3	6.68	16970.8	137211.2	8.09	7.39
For all the periods,	13480.6	100905.8	7.49	30952.1	251823.3	8.14	7.813

The result No. 16, and the mean results in Tables V. and VI., for the whole periods, agree very closely. As the value of one micrometer division, in parts of the whole vertical force, is about 0.00013, the greatest difference of the three final results, 7.83, 7.90, and 7.81, is 0.0000012.

The final results, from five days' observations, by the method of deflections, were .000085, .000077, .000079, .000062, .000073, differing 0.000023.

The results, from the comparison of daily observations, in parts of the whole vertical force, will be about .000134, the time of vibration being about 9 seconds; if 11 seconds were adopted, the result would be .000095; in either case considerably more than the result obtained by deflections.

20. The satisfactory determination of q for the Balance magnetometer, led me to determine the correction for the Bifilar magnetometer by the same method.

Besides the variation of the magnetic moment, temperature also affects the length and interval of the suspending silver wires; it probably also affects their elasticity.

The determination of the correction from the daily observations, at once sums up all the effects of temperature. When the suspending threads are of silk, these sources of error are avoided; but I conceive that much graver errors are introduced, due chiefly to varying humidity affecting the torsion of the thread.

I shall give simply the results of the comparisons of the daily observations for the Bifilar magnetometer.

TABLE VII.

Determination of the Temperature Correction for the Bifilar Magnetometer, from comparisons of the Daily Observations.

PERIOD.	Preceding temperatures greater than the succeeding.			Preceding temperatures less than the succeeding.			Mean of the two values of q in parts of force.	Value of q independent of the sign of $\Delta(t)$ in parts of force.
	$\Sigma \Delta(t)$	$\Sigma \Delta(x)$	q	$\Sigma \Delta$	$\Sigma \Delta(x)$	q		
1844.		Sc. Div.	Sc. Div.		Sc. Div.	Sc. Div.		
May 9—24.	5334.9	13066.8	2.45	2359.9	4033.0	1.71	0.000270	0.000289
May 29—June 28.	11938.2	24597.2	2.06	26719.2	45966.3	1.72	0.000246	0.000238
July 17—30.	1843.1	3004.0	1.63	4637.8	8470.4	1.83	0.000225	0.000230
Sept. 2—25.	27322.6	53684.3	1.96	622.1	1260.8	2.03	0.000259	0.000255
Nov. 26—Dec. 13	17855.4	36791.6	2.06	2143.7	3104.5	1.45	0.000229	0.000259
For all the periods,	64294.2	131143.9	2.04	36482.7	62835.0	1.72	0.000244	0.000250

When it is considered that the daily range of the Bifilar readings, in parts of the whole horizontal force, is to the daily range of the Balance readings, in parts of the whole vertical force, as 7 or 8 to 1, it will be seen that the results for the Bifilar magnetometer are equal to those for the Balance. It should also be remembered that the results for May and July are from short periods.

The results obtained by deflections on two days were 0.000291 and 0.000298, the partial results agreeing very well.

Taking into account the expansion of the wires, the total temperature correction is 0.000304.

It will be observed, in this case, that the results by deflections are greater than those from a comparison of the daily observations.

MAKERSTOUN, April 18. 1845.

X.—*On Wollaston's Argument from the Limitation of the Atmosphere, as to the Finite Divisibility of Matter.* By GEORGE WILSON, M.D., *Lecturer on Chemistry.*

[Read 21st April 1845.]

In the year 1822, Dr WOLLASTON published a remarkable paper “on the finite extent of the atmosphere.”* Its object is to establish, by observations on the motions of certain of the heavenly bodies, that our atmosphere does not extend into free space, and to deduce from this limitation in its extent, the conclusion, that the air necessarily consists of particles “no longer divisible by repulsion of their parts;” *i. e.* of true atoms. From this there is the further inference, that, “since the law of definite proportions discovered by chemists, is the same for all kinds of matter, whether solid or fluid, or elastic, if it can be ascertained that any one body consists of particles no longer divisible, we then can scarcely doubt that all other bodies are similarly constituted.” In other words, the existence of a limit to the earth's atmosphere is declared to supply a demonstration of the finite divisibility of matter.

In pursuing this argument, WOLLASTON first discusses the question, What is the probable height to which the earth's atmosphere extends? And after stating, that, from the known laws of the elasticity of the atmosphere, we should infer that it extends to the height of 40 miles, with properties yet unimpaired by extreme rarefaction, he proceeds to say, “Beyond this limit we are left to conjectures founded on the supposed divisibility of matter; and if this be infinite, so also must be the extent of our atmosphere. But if air consist of any ultimate particles no longer divisible, then must expansion of the medium composed of them cease at that distance where the force of gravity downwards, upon a single particle, is equal to the resistance arising from the repulsive force of the medium.” WOLLASTON, it will be observed, takes for granted two things. *1stly*, He assumes that the law which is known to connect the density of the air with the compressing force, near the surface of the earth, prevails, without change, to the limit of the atmosphere. *2dly*, He identifies the divisibility of the mass with that of its component parts or molecules. If the molecule be infinitely divisible, the mass will be so also, and *vice versa*; so that if the divisibility (finite or infinite) of either be ascertained, that of the other will thereby be ascertained also. Now, the atmosphere is not merely divisible, but, consisting like other gases of mutually repulsive particles, contains within itself a power of division. We have only, therefore, to

* Philosophical Transactions, 1822, p. 89.

permit this self-dividing force to come into play, and the result, according to WOLLASTON, will shew whether the mass undergoing spontaneous division is infinitely divisible or not. This experiment we cannot try; but it has long ago been performed for us by the hand of Nature. Our atmosphere has divided itself to the utmost limit which its susceptibility of division permitted, and has thereby tested or ascertained that divisibility for us. Either that is infinite, in which case, the atmosphere must have spread into space, and portions of it will be found surrounding the different heavenly bodies, varying in amount according to their respective dimensions, temperatures and the like. Or it is finite, and the air has found a limit at no great distance from the earth; for the particles of which it consists, although free, so far as their mutual repulsiveness is concerned, to recede from each other, are not equally free to recede from the earth, to which the force of gravitation binds them. They must come to rest accordingly at the point where the attraction of gravitation is equal in amount, while it is opposite in direction to the force of repulsion among them; so that they are balanced in equilibrium between them. Now it appears on making the necessary observations, that probably the Sun, and certainly that Jupiter, is devoid of an atmosphere of the same nature as our own. Therefore, concludes WOLLASTON, our atmosphere is of finite extent, and consists of particles only finitely divisible. And as the air cannot be supposed to be peculiar in this respect, the conclusion is immediately extended to every other substance, and all matter is inferred to consist of finitely divisible particles, or *bonâ fide* atoms.

It cannot surprise us that so remarkable a paper as WOLLASTON's should have excited the greatest attention among men of science. If the argument pursued in it were just, the vexed question of the finite or infinite divisibility of matter, which, for some thousand years, physics and metaphysics had alike sought in vain to decide either way, had all the while been answered for us. Every attempt towards the solution of that problem by experiment had failed, not perhaps, because ultimate atomic particles had not been arrived at, by the dividing forces our command, (for this length the inquiry never reached); but because, long before the divisibility of a body could be supposed to be exhausted, the products of its division had become invisible to us, and we had no test by which to tell when the atoms of a substance had been attained to. Both of these difficulties, according to WOLLASTON, were taken out of the way, by the mode in which Nature made the experiment. A dividing force co-ordinate with the divisibility on which it took effect—finite, if it were finite, infinite, if it were infinite—was brought into play. The result of this division, moreover, could be ascertained, could, in truth, literally be seen; for it did not take place in a vacuum, but in a space containing bodies, each of which would infallibly indicate the extension of a self-dividing medium at least to itself; and as that, if it were infinitely divisible, must reach to them all in its progress towards infinite division, should it certainly

appear that it had not extended to any one, still more, if not to several, it would suffice to prove that it was not infinitely divisible. In short, our atmosphere being the self-dividing mass, and all the stars standing between it and infinity, the absence of an atmosphere like the earth's, from any one of them, shews that that it can only be finitely divided; and the decision in the negative of the question of infinite divisibility should have dated from the discovery of the telescope, and GALILEO's earliest observation of the eclipses of Jupiter's moons.

My object in the following remarks, is to shew that WOLLASTON's identification of the divisibility of the molecule, with the observed division of the mass of which it is a part, is altogether unwarrantable; that he takes for granted the very thing to be proved; and that his whole discussion leaves the question of the finite or infinite divisibility of matter exactly where it found it. Before doing so, however, I am anxious to refer very briefly to the criticisms already offered on this part of the paper under discussion.

The opinions hitherto expressed as to WOLLASTON's argument may be arranged, I believe, under four heads. 1st, A few natural philosophers have entirely assented to the truth of the conclusion contained in it. Among these was DAUBENY,* who has lately, however, withdrawn his assent;† and it is still advocated by DUMAS, who, whilst he objects to WOLLASTON's arguments, on other grounds which will be referred to immediately, appears to consider the conclusion of the latter unavoidable, if his premises are granted him.‡

2d, A greater number, including FARADAY,§ GRAHAM,|| and TURNER,¶ have implied, by the terms of commendation in which they have referred to it, that at least they detected no fallacy in the argument.

3d, It has been objected to by DUMAS (following out the views of POISSON), on the ground that the low temperature which is known to prevail in the upper

* Introduction to the Atomic Theory, 1831, pp. 103-5.

† Supplement to Introduction, &c., 1840, p. 11.

‡ DUMAS' assent was entirely negative, but was strongly manifested, and is the more remarkable, that he has directed special attention to the phenomena presented by those gases which combine without undergoing diminution of their volume, as irreconcilable with the idea of the chemical equivalents of these bodies being represented by single atoms, such as DALTON assumed, (*Leçons sur la Philosophie Chimique*, p. 263). Had this view been carried out and applied to the atmosphere, it would have struck at the root of WOLLASTON's whole train of reasoning, and would have obviated the necessity of appeal to the questionable views of POISSON, as to the cause of the limitation of the atmosphere. As WHEWELL's discussion of WOLLASTON's speculations, which was specially intended to meet the argument of DUMAS, has appeared since the latter published his views, it may have led to some modification of his opinion. But that distinguished chemist has not had occasion, so far as I am aware, to refer again to the subject in public; so that, in the meanwhile, I include him among the acknowledged supporters of the intrinsic validity of WOLLASTON's views.

§ On the existence of a limit to vaporisation, *Phil. Trans.*, 1826, p. 492.

|| Elements of Chemistry, pp. 68 and 273.

¶ Elements of Chemistry, 7th edition, p. 207.

regions of the atmosphere, may be such at its boundary as to destroy the elasticity of the air, and even to liquefy or solidify it.* DAUBENY,† KANE,‡ and others, have replied to this, that the temperature of planetary space, according to FOURIER, SCHWANBERG, and others, is much higher than that to which air has been exposed in experiments with solid carbonic acid and ether, without destroying its elasticity. DUMAS, in anticipation of such objections, has declared, that we are not to consider the temperature which a thermometer would exhibit if placed in the upper strata of the atmosphere, as necessarily identical with that of the air around it.§ By which statement he means to enforce, if I understand him aright, that non-elastic (liquid or solid ?) air may, like other diathermanous bodies, transmit heat without being thereby raised in temperature itself, so that the outer shell of air may be colder than the layers within it, or space beyond it. In allusion to such a view, Professor JAMES FORBES has pointed out the difficulty of understanding “ how it is possible that the higher strata of the atmosphere can remain permanently colder than the strata beneath and the sky above them, without admitting a paradox of the same kind with a mechanical perpetual motion.”||

In reference to DUMAS' mode of disposing of WOLLASTON'S argument, I would only further observe, that natural philosophers are not at one as to the temperature, either of planetary space or of the upper strata of the atmosphere; so that it is impossible at present to say what is the exact value of the objection I have been discussing.

4th, Finally, several physicists have denied the justness of WOLLASTON'S conclusion, on the ground of its intrinsic invalidity. Among these are Professor JAMES FORBES¶ and Dr KANE,** who have not, however, so far as I am aware, stated in what way they dispose of the argument. Professor WHEWELL is likewise an objector, and dissents from WOLLASTON'S inference, on the plea that the latter was not at liberty to assume that the law which connects the density of the air with the compressing force at the upper boundary of the atmosphere, is identical with that which is known to prevail near the earth. His own words are—“ We know nothing of the law which connects the density with the compressing force in air so extremely rare, as we must suppose it to be near the boundary of the atmosphere. Now there are possible laws of dependence of the density upon the com-

* Leçons, &c., p. 239.

† Supplement to the Introduction to the Atomic Theory, p. 11.

‡ Elements of Chemistry, p. 441.

§ Leçons, &c., p. 241.

|| Report of British Association, 1841, p. 79.

¶ Op. cit., p. 77.

** Elements of Chemistry, pp. 15 and 358.

pressing force, such that the atmosphere would terminate in virtue of the law without any assumption of atoms. This may be proved by mathematical reasoning. If we suppose the density of air to be as the square root of the compressing force, it will follow that, at the very limits of the atmosphere, the strata of equal thickness may observe in their densities such a law of proportion as is expressed by the numbers 7, 5, 3, 1. For the compressing force on each being as the whole weight beyond it, will be for the four highest strata 16, 9, 4, and 1, of which the square roots are as 4, 3, 2, 1, or as 8, 6, 4, 2; and, though these numbers are not exactly as the densities 7, 5, 3, 1, those who are a little acquainted with mathematical reasoning will see that the difference arises from taking so small a number of strata. If we were to make the strata indefinitely thin, as to avoid error we ought to do, the coincidence would be exact; and thus, according to this law, the series of strata terminates as we ascend, without any consideration of atoms.”*

My object in the succeeding argument is to shew, that, although the law which WOLLASTON assumed to prevail in the higher regions of the atmosphere were in operation, it would not justify the conclusion which he supposed it to warrant. The discussion which follows differs from WHEWELL’S mode of disposing of the subject, in conceding to WOLLASTON his own law; and from that of POISSON and DUMAS, in permitting him to take for granted as high a temperature as he pleases, provided only the atmosphere have reached a limit.

On a little consideration of WOLLASTON’S reasoning, it will appear, that all that he succeeded at the utmost in establishing was, that the atmosphere consists of a finite number of repelling molecules. He seems to have conceived that this was sufficient, and that no one would dispute his subsequent assumption, that these repelling molecules were ultimate particles or true atoms.

But such an assumption is, on a twofold ground, inadmissible. The more important chemical components of our atmosphere are, water-vapour, carbonic acid, oxygen, and nitrogen. Let us set aside for the time, as we are at liberty to do, the influence of the low temperature of the upper regions of the air in condensing the water, and perhaps also the carbonic acid; and suppose our atmosphere, with a temperature at its boundary sufficient to retain all its constituents as elastic fluids, to find a limit, in virtue of the prevalence of WOLLASTON’S law. Each gas would cease to expand for the same reason, and present a row of bounding molecules, which were prevented from falling towards the earth by the repulsion of the particles between it and them, and from receding from the earth by their own weight. But the molecules of water-vapour, and carbonic acid, brought in this way to a stand, would certainly not be ultimate particles or indivisible atoms. The molecule of water, on the simplest view of its constitution, namely, that the chemical equivalent corresponds to a single atom, would consist of at least two

* *Philosophy of the Inductive Sciences*, vol. i., p. 420, and *Athenæum*, 1839, pp. 724-7.

atoms, one of hydrogen and one of oxygen, with a centre of repulsion common to both. The molecule of carbonic acid, for similar reasons, would consist of three atoms, one of carbon and two of oxygen. And who shall assure us that oxygen and nitrogen are not compounds with binary chemical molecules like those of water, or ternary ones like those of carbonic acid? It is true that the chemist names these gases simple substances. But the simplicity he attributes to them is only, as he is careful to define, *quoad analysis*; and the physicist is not at liberty to convert this negative and relative simplicity into an absolute one, and make deductions therefrom, as WOLLASTON has done. If we argue from analogy, in reference to this point, we should infer that oxygen and nitrogen are compounds; for we know a much greater number of gases in which the molecule is a group of chemically distinct atoms, than we do of elastic fluids, where, on the most favourable view, it can be supposed to be a single one. But it is not necessary to pursue any argument of this kind; nor is the objector called upon to shew that oxygen and nitrogen are chemical compounds. It is sufficient for his purpose to decline assent to WOLLASTON's conclusion till he, or those who agree with him, supply proof that the molecules of oxygen and of nitrogen are chemically simple. The *onus probandi* clearly lies, not with the denier but with the asserter, of a positive proposition like the one before us.

In so far, then, as WOLLASTON assumed the chemical simplicity of two of the gases of the atmosphere, he employed an *argumentum ad ignorantiam*. He was guilty also of a *petitio principii*. For even, if it could be shewn, that oxygen and nitrogen are chemically homogeneous, and do not, on that account, admit of comparison as to the constitution of their gaseous molecule with water and carbonic acid, it would not warrant the conclusion, that that molecule was an atom. Does it follow as a necessary inference, that because a body is simple, its gaseous repelling molecule must consist of but one atom? The answer is assuredly in the negative. The molecule might, on the other hand, be made up of a pair of atoms, like a binary star, with a centre of repulsion common to the two; or of 10, or of 100, or 1000 atoms (if such bodies there be), grouped together into a compound whole. We have no means whatever, in truth, of estimating what the complexity of the molecule may be. Without insisting at greater length on this, it is at least manifest, that we are not even at liberty to identify the combining chemical molecule with the repelling gaseous one, much less to identify either with the ultimate atom. Yet, unless WOLLASTON was at liberty to do so, his argument was useless towards settling the question of the divisibility of matter. To prove that the atmosphere consisted of finite molecules, was only to reach the threshold of the difficulty: for each molecule supplied as good a text whereon to dispute the question of infinite divisibility, as the whole atmosphere out of which it was taken. The point which most of all demanded proof, namely, that the molecule was an atom, was the very one which he took for granted.

WOLLASTON, in truth, erred, in assuming that the self-dividing power present in the atmosphere was able to divide, to the uttermost, the divisible mass subjected to its action; in taking for granted that the divisibility was co-ordinate with the actual division, so that the latter was the exact index and measure of the amount of the former. The fallacy of his argument will at once appear if the latter be thrown into a syllogistic form. It will then run thus :—

1. An atmosphere consisting of an infinite number of mutually repulsive particles, must be infinitely extended.

2. But our atmosphere is not infinitely extended.

3. Therefore our atmosphere does not consist of an *infinite number of particles*. Whereas it should have been.

Therefore our atmosphere does not consist of an infinite number of *mutually repulsive* particles.

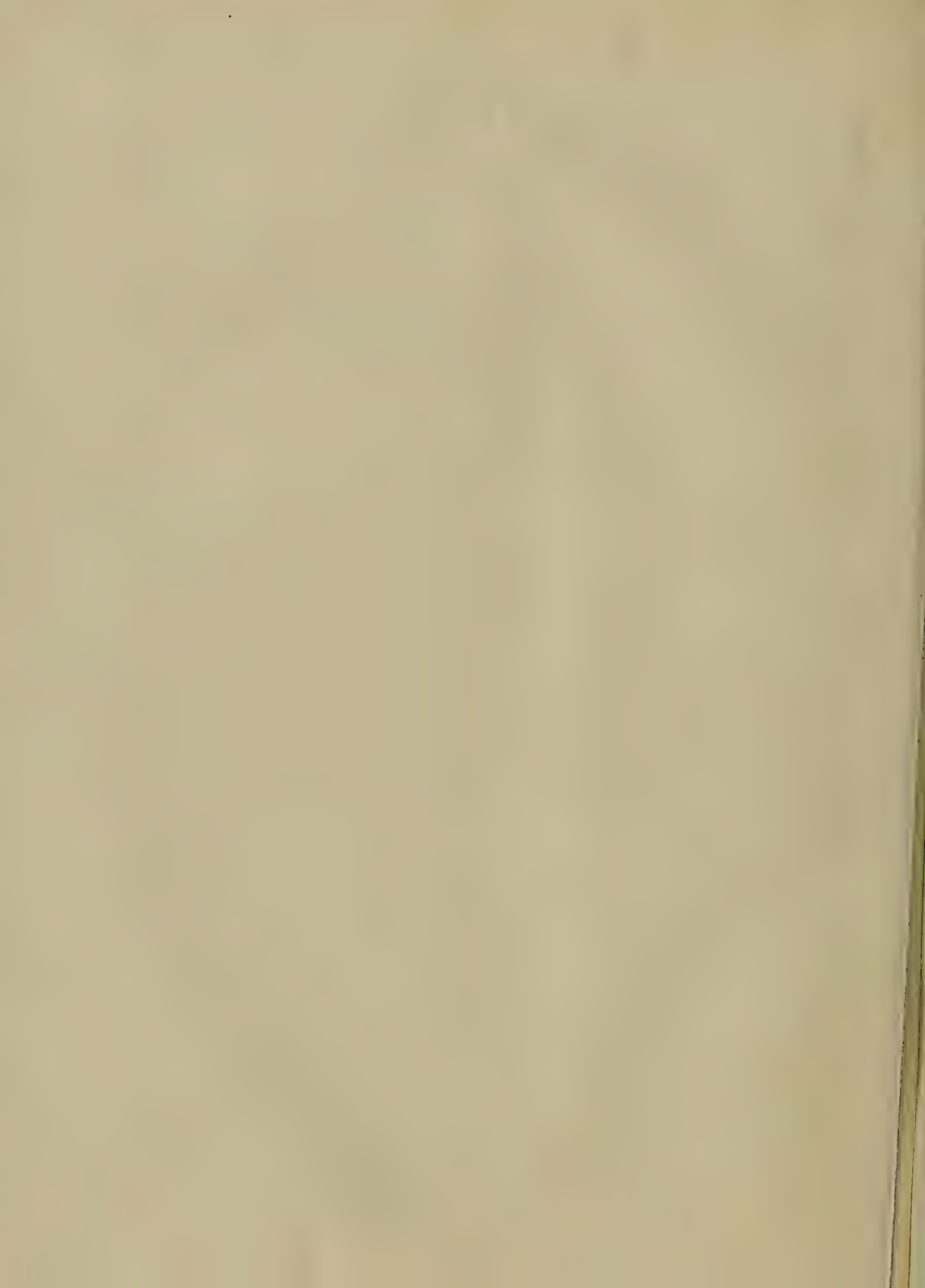
The premises fully warrant the conclusion that our atmosphere does not consist of an infinite number of *mutually repelling* particles, but throw no light on the question, whether or not it may contain an infinite number of *mutually indifferent*, or *mutually attractive* ones.

WOLLASTON'S argument, then, supplies no decision of the question of the divisibility of matter. That problem still presents the same twofold aspect of difficulty which it has ever exhibited. If we affirm that matter is infinitely divisible, we assert the apparent contradiction, that a finite whole contains an infinite number of parts. If, pressed by this difficulty, we seek to prove that the parts are as finite as the whole they make up, we fail in our attempt. We can never exhibit the finite factors of our finite whole; and the so-called atom always proves as divisible as the mass out of which it was extracted. Finiteness and infinity must both be believed in; but here, as in other departments of knowledge, we cannot reconcile them.

It seems surprising that fallacies so palpable as those we have been discussing, should not have been detected long ago by the able philosophers who have noticed WOLLASTON'S argument. It is especially singular, that DUMAS, who holds that, in the combination of gases, a division of the chemical equivalent frequently occurs (so that he represents the latter as expressed physically by a group of many molecules), should not have applied his views, as he could so easily have done, to its full refutation.

As it is, I trust that the discussion I have laid before the Society will not prove unacceptable to its members. WHEWELL'S reasoning cannot be appreciated by those who are ignorant of mathematics; and the views of POISSON and DUMAS, even should they be fully established, leave unconsidered the question of the intrinsic validity of WOLLASTON'S conclusions. I am not without hopes, accordingly, that a demonstration of a fallacy in the argument in question, on purely physical grounds, which can be understood by every one, and which, so far as I am aware,

has not been offered already, will be of service in removing doubtful knowledge. In particular, it may save beginners from seeking for a demonstration where none is to be found, and from blaming themselves because they cannot acquiesce in a conclusion, the truth of which great names have appeared to warrant. I would, observe, however, that, although I have employed the words *molecule* and *atom* in the preceding discussion, it has been for the sake of simplicity and convenience, and to meet WOLLASTON on his own ground. I do not wish to be understood as offering any opinion as to the ultimate constitution of matter, except in so far as I deny the success of the only attempt which has been made in modern times, to establish, by direct observation, the existence of indivisible atoms.



XI.—*On the Sums of the Digits of Numbers.* By the Right Reverend BISHOP TERROT.

[Read 2d December 1845.]

THE general properties of numbers, considered without reference to the notation in which they are expressed, have been very fully investigated by several of the most distinguished mathematicians. Little attention, however, has been paid to the particular properties resulting from the principle of the modern notation, which is the expression of every number in a series, $a + bn + cn^2$, &c. where a, b, c , are the digits, and n the local value or root of the notation. Having been led to examine some of these results, and to account for them, I am now desirous of laying them before the Society. I do not flatter myself that they possess any great practical importance; but as I have reason to believe that they are *new*, I trust the Society will not think them entirely unworthy of their attention.

If, then, we look at the multiplication table, and examine, in the first place, the multiples of *seven*, we find them—

7, 14, 21, 28, 35, 42, 49, 56, 63, 70, 77, &c.

Sums 7, 5, 3, 1, 8, 6, 4, 2, 9, 7, 5, &c.

If we also take, as above, the ultimate sums of the digits of these multiples, that is to say, the sum of the digits of each if that sum be a single digit, or, if not, the sum of the sum of those digits, till in each case we arrive at a single digit, it appears, that, for the first nine places, these sums range through all the digits of our notation, without any recurrence, and then commence over again in the same sequence as before.

On looking at the adjacent line of the multiples of *six*, we find the case very different. The multiples are,

6, 12, 18, 24, 30, 36, 42, 48, 54, 60, 66, 72, &c.

and their sums 6, 3, 9, 6, 3, 9, 6, 3, 9, &c. &c.

Here only three digits occur in the series of sums, and these repeated over and over in the same order. Farther, we may observe, that what is true of *seven* is true of five, eight, and all numbers which are prime to nine; and that what is observed of the multiples of six, occurs also in the multiples of three, the only other digit which has a common divisor with *nine*.

I began with accounting for these facts; and, proceeding from simple multiples to the consideration of other integer series, such as the series of squares, cubes, &c., the successive powers of a given root, the polygonal and figurate numbers, I found that wherever there is a fixed law of relation between the succes-

sive numbers, there is also a definite sequence and recurrence in the sums of the digits which express them ; and the results of these inquiries, with the requisite demonstrations, I will now, as briefly as possible, lay before the Society.

PROP. I.

If m and n are prime to one another, am cannot equal bn , unless a and b be equimultiples of n and m respectively. For, if $am=bn$, $\frac{m}{n}=\frac{b}{a}$. But by hyp. $\frac{m}{n}$ is a fraction in its lowest terms, therefore $b=pm$, and $a=pn$.

PROP. II.

If $N=P \cdot \overline{n-1} + r_m$, n being the local value of the notation, and $P \cdot \overline{n-1}$ being the greatest multiple of $\overline{n-1}$, which is less than N ; then r_m is the ultimate sum of the digits of N .

Let $N=a+b \cdot n+c \cdot n^2+d \cdot n^3, \&c.$

$$\frac{N}{n-1} = b+c \cdot \overline{n+1} + d \cdot \overline{n^2+n+1} + \&c. \quad . \quad . \quad . \quad . \quad + \frac{a+b \cdot c, \&c.}{n-1}$$

$$N=p \cdot \overline{n-1} + a+b \cdot c, \&c. = p \cdot \overline{n-1} + r_1$$

Again, let $r_1=q \cdot \overline{n-1} + r_2$, where r_2 is the sum of the digits of r_1 , or the second sum of the digits of N .

Then $N=p+q \cdot \overline{n-1} + r_2$. Let this operation be continued till r_m becomes a single digit, we have $N=P \cdot \overline{n-1} + r_m$, where r_m is the ultimate sum of the digits of N .

Ex. In our notation $n=10$, and $\overline{n-1}=9$.

Let $N=567434=\overline{63068 \times 9+2}$

here 1st sum =29

2d do. =11

3d do. = 2

COR. If $r = \overline{n-1}$, then N is a multiple of $n-1$. And, conversely, if N be a multiple of $\overline{n-1}$, $r_m=n-1$.

PROP. III.

If a be a number prime to $\overline{n-1}$; and p, q be two numbers, whose difference is neither $\overline{n-1}$, nor a multiple of $\overline{n-1}$, then pa and qa cannot have the same ultimate sum.

If possible let $pa=m \cdot \overline{n-1} + r$ and $qa=m_1 \cdot \overline{n-1} + r$, and let $s=q-p$, then $sa=q \cdot a - p \cdot a = \overline{m_1 - m} \cdot \overline{n-1}$; but by hyp. a is prime to $\overline{n-1}$, and s is neither $\overline{n-1}$, nor a multiple of it; therefore, by Prop. I. sa cannot equal $\overline{m_1 - m} \cdot \overline{n-1}$, and therefore pa and qa cannot have same ultimate sum.

PROP. IV.

If a have a common divisor with $\overline{n-1}$ as v , then pa and qa will have the same ultimate sum if $q-p=\frac{n-1}{v}$.

Let $pa=P.\overline{n-1}+r$, therefore $qa=pa+\frac{n-1}{v}a=P.\overline{n-1}+r+\frac{n-1}{v}.a$. But v is a divisor of a ; therefore $qa=P.\overline{n-1}+r+b.\overline{n-1}=P_1.\overline{n-1}+r$, that is, pa and qa have same ultimate sum.

PROP. V.

If a be a divisor of $\overline{n-1}$, or $\frac{n-1}{a}=v$, then pa and qa will have the same ultimate sum if $q-p=v$.

Let $pa=P.\overline{n-1}+r$, $qa=pa+va=P.\overline{n-1}+r+n-1=(P+1).\overline{n-1}+r$.

PROP. VI.

If $P=Q+R$. The ultimate sum of P = ultimate sum of (sum Q + sum R).

Let $Q=m.\overline{n-1}+r$, $R=m_1.\overline{n-1}+r_1$
 $P=Q+R=\overline{m+m_1}.\overline{n-1}+r+r_1$

But r and r_1 being single digits, their aggregate is either a single digit, or $\overline{n-1} +$ a single digit. In the former case, the ultimate sum of P = sum of Q + sum of R . In the latter, sum of P = sum (sum of Q + sum of R).

COR. If R be a multiple of $\overline{n-1}$, or $r_1=n-1$, sum of P = sum of Q .

PROP. VII.

From these propositions it follows, that in any arithmetical series, whose common difference is prime to $\overline{n-1}$, the ultimate sum of any term (the p^{th}) = the ultimate sum of $(p+q.\overline{n-1})^{\text{th}}$; but that no two terms at any other interval can have the same ultimate sum; and hence, that all the terms from the p^{th} to the $(p+n-1)^{\text{th}}$ range, as to their ultimate sums, through all the digits of the scale. For if the p^{th} term $=\overline{s.n-1}+r$, then the $(p+q.\overline{n-1})^{\text{th}}$ term $=\overline{s.n-1}+r+q.\overline{n-1}.b=\overline{s+bq}.\overline{n-1}+r$.

Again, let p^{th} term $=a$, $q^{\text{th}}=a+q-p.b$; but by Prop. I., since b is now taken prime to $\overline{n-1}$, and $q-p$ is neither $\overline{n-1}$, nor a multiple of it, the q^{th} term must have an ultimate sum different from the p^{th} .

Ex. 1. The successive multiples of any number prime to 9, are an arithmetic series whose common difference is that number. Thus, the multiples of 5 are,

5, 10, 15, 20, 25, 30, 35, 40, 45, 50, 55, 60, &c.

Sums 5, 1, 6, 2, 7, 3, 8, 4, 9, 5, 1, 6, &c.

Ex. 2. But let the number whose multiples are taken have a common divisor with $9(n-1)$ as 6.

The series is 6, 12, 18, 24, 30, 36, 42, 48, 54, 60.

Sums . . . 6, 3, 9, 6, 3, 9, &c.

Where the sums recur at every third term, because 6 and 9 have a common divisor 3, and $\frac{9}{3}=3$, that is $\frac{n-1}{v}=3$. (Prop. V.)

Ex. 3. The recurrence of the sums, according to Prop. V., may be more strikingly illustrated, if we use a notation whose root is 13, and, consequently, $\overline{n-1}=12$. If we express the successive multiples of 6 in this notation, we must adopt three additional characters for 10, 11, 12. Let these be $1_0, 1_1, 1_2$. The successive multiples in this notation are,

6, 1_2 , 15, 11_1 , 24, 21_0 , 33, 39, 42, 48, &c.

Sums . . . 6, 1_2 , 6, 1_2 , 6, 1_2 , &c., where we see the sums recur after two terms, because $n-1=12$, and $\frac{12}{6}=2$.

PROP. VIII.

If n be even, and $\overline{n-1}$ consecutive terms of an arithmetic series be taken, the ultimate sum of the digits of their aggregate is $\overline{n-1}$. But if n be odd, the ultimate sum will be $\overline{n-1}$, or sum $\left(\frac{b.n-1.n-2}{2}\right)$ according as b , the common difference, is even or odd.

For aggregate of $\overline{n-1}$ terms of arithmetic series $= (2a + \overline{n-2}.b) \cdot \frac{n-1}{2}$
 $= a.\overline{n-1} + \frac{\overline{n-1}.n-2.b}{2}$. If n be even $\frac{n-2.b}{2}$ is integer, whether b be even or odd.

Therefore the latter term is a multiple of $\overline{n-1}$, and, consequently, the whole expression being a multiple of $\overline{n-1}$, has $\overline{n-1}$ for its ultimate sum (Prop. II., Cor.)

But if n be odd, $\frac{n-2.b}{2}$ is integer only when b is even.

PROP. IX.

If we assume as bases two numbers whose sum is $s.\overline{n-1}$, and take a series of the successive powers of each, then of the two series expressing the sums of digits of successive powers, the even terms are identical, while the odd terms are complementary, that is, their sum is $\overline{n-1}$.

Let $m+m_1=s.\overline{n-1}$, $m_1=s.\overline{n-1}-m$

$m_1^p = (s.\overline{n-1})^p - p(s.\overline{n-1})^{p-1}m \dots \pm m^p$ according as p is even or odd. Here every term except the last is a multiple of $s.\overline{n-1}$.

Therefore if p be even, sum of $m_1^p = \text{sum of } m^p$.

But if p be odd, sum of $m_1^p = \text{sum of } (Q.\overline{n-1}) - \text{sum of } m^p = \overline{n-1} - \text{sum of } m^p$.

To illustrate this and some of the succeeding propositions, I shall here introduce a table of successive powers of digits prime to 9, with their ultimate sums.

Base 2.	$\left\{ \begin{array}{l} \text{Powers } 2, 4, 8, 16, 32, 64, 128, \&c. \\ \text{Sums } 2, 4, 8, 7, 5, 1, 2, \&c. \end{array} \right\}$	Sums recur after 6 terms.
Base 4	$\left\{ \begin{array}{l} \text{Powers } 4, 16, 64, 256, 1024, \&c. \\ \text{Sums } 4, 7, 1, 4, 7, \&c. \end{array} \right\}$	Sums recur after 3 terms.
Base 5	$\left\{ \begin{array}{l} \text{Powers } 5, 25, 125, 625, 3125, 15625, 78125, \&c. \\ \text{Sums } 5, 7, 8, 4, 2, 1, 5, \&c. \end{array} \right\}$	Sums recur after 6 terms.
Base 7	$\left\{ \begin{array}{l} \text{Powers } 7, 49, 343, 2401, 16807, \&c. \\ \text{Sums } 7, 4, 1, 7, 4, \&c. \end{array} \right\}$	Sums recur after 3 terms.
Base 8	$\left\{ \begin{array}{l} \text{Powers } 8, 64, 512, 4096, \&c. \\ \text{Sums } 8, 1, 8, 1, \&c. \end{array} \right\}$	Sums recur after 2 terms.

In this table, we may observe that in every case the sum of the digits recurs, but at different intervals. Next, if we take two complementary bases, as 5 and 4, we find in the lines expressing the sums, that the first terms are respectively 4 and 5, the 2d terms 7 and 7, the 3d 1 and 8, and so on; as was proved generally in the last proposition. Lastly, we may observe that the digits 3, 6, 9, that is $n-1$, and the digits having a common divisor with $n-1$, never occur among the sums. It remains, then, for us to point out the reason of this last mentioned fact, and to discover the principle which fixes the period of recurrence.

PROP. X.

Every power of a number prime to $n-1$, must have the sum of its digits also prime to $n-1$.

Let m , which is prime to $n-1$, be reduced to its prime factors, or let $m=a.b.c, \&c.$

Then $m^p = (a^p.b^p.c^p \&c.)$ Here m^p has no possible divisors except $a, b, c, \&c.$, and by hyp. none of these are divisors of $n-1$, therefore m^p is prime to $n-1$.

Now let $m^p = q.n-1 + r$. Here $q.n-1$ contains all the divisors of $n-1$. If, therefore, r contains any of those divisors $q.n-1 + r$, or m^p contains such divi-

sors; but m^r has been proved prime to $\overline{n-1}$; therefore r contains no divisor of $\overline{n-1}$, or is prime to it.

PROP. XI.

To determine the recurrence of ultimate sums in the series $m, m^2, m^3, \&c.,$ m being any single digit.

If we can determine what term will have 1 for its ultimate sum, the problem is solved. For if $m^q = p.\overline{n-1} + 1$, $m^{q+1} = p'.\overline{n-1} + m$, or has same sum as first term, $m^{q+2} = p''.\overline{n-1} + m^2$, and so on, or sums recur after q terms.

Every number (m) is of the form $3p$ or $3p \pm 1$.

1. If m be of form $3p$, every power of m after the first is a multiple of 9, and consequently the sum of every power $= 9$.

2. If m be of form $3p+1$,

$$m^q = \overline{3p}^q + q.\overline{3p}^{q-1} \dots + q.\frac{q-1}{2}.\overline{3p}^2 + q.3p+1.$$

In this expansion, every term is divisible by 9, except the two last, or $m^q = 9s + 3pq + 1$.

Consequently m^q will have 1 for its ultimate sum, if $3pq =$ a multiple of 9; but since (m being one of the digits) p cannot $= 3$, or a multiple of 3, q must. If, then, $q=3$, $3pq=9p$, and the sum 1 will recur at every third term.

3. If $m=3p-1$, $m^q = \overline{3p}^q - q.\overline{3p}^{q-1} \dots \mp (3pq-1)$, the sign being $-$ if q be even, $+$ if q be odd.

α . Let q be even. $m^q = 9s - 3pq + 1$; and this, as before, will give the ultimate sum 1, if $3pq$ be a multiple of 9, or $pq =$ a multiple of 3. If p be prime to 3, then q must be an even multiple of 3, as 6, 12, &c., or the ultimate sum 1 recurs at every 6 terms. But if p be 3, or a multiple of 3, the sum will recur at every second term, for in that case q may be any even number.

β . If q be odd, $m^q = 9s + 3pq - 1$, but by hyp. $m^q = 9r + 1$, $\therefore \overline{r-s}.9 = 3pq - 2$, or $3pq - 2$ is a multiple of 3, which is absurd. Therefore the sum 1 can never recur at an odd power, when m is of the form $3p-1$.

If, now, we refer to the table given in Prop. IX., we see that of the bases there employed, 4 and 7 are of the form $3p+1$, and in them the sum 1 recurs at every third term. 2, 5, and 8 are of the form $3p-1$. In 2 and 5, p is prime to three, and therefore the sum 1 occurs at 6th term. In 8, $p=3$, and therefore the sum 1 recurs at every even term.

COR. Hence, if m be not a multiple of 3, *i. e.* if it be prime to 9, m^r has 1 for its ultimate sum, for 1 must occur at 2d, 3d, or 6th term, and 6 is a multiple of 2 and 3; therefore in any case the 6th power must have 1 for its ultimate sum.

Hence $m^e = 9r + 1$ or $9r$; the former when m is prime to 9, the latter when it has a common divisor with it. This is a form not given by BARLOW.

PROP. XII.

In the series of m^{th} powers of successive integers, beginning from 1, the ultimate sums recur after $\overline{n-1}$ terms.

If m be odd, the ultimate sums of any two terms, whose roots together equal $\overline{n-1}$, are either together equal $n-1$, or are each $\overline{n-1}$.

If m be even, the ultimate sums of such complementary terms are identical.

After what has been proved, the demonstration of these is so easy that it is unnecessary to give it.

Ex. Series of 2d powers 1, 4, 9, 16, 25, 36, 49, 64, 81, 100, 121.

Ultimate sums 1, 4, 9, 7, 7, 9, 4, 1, 9, 1, 4.

Series of 5th powers 1, 32, 243, 1024, 3125, 7776, 16807, 32768, 9^5 , 10^5 .

Ultimate sums 1, 5, 9, 7, 2, 9, 4, 8, 9, 1.

Here, in the ultimate sums of the squares, we have 1st and 8th, 2d and 7th, &c. identical. In the ultimate sums of 5th powers, the 1st + 8th = 9, 2d + 7th = 9, and so on.

It is worthy of notice, though rather out of place, that if, in the series of 5th powers, instead of taking the sums, we take the difference between the sums of the odd and even digits, the difference will in every case be 1. This property is proved generally by BARLOW, in his Theory of Numbers, p. 172, in this form

that $x^{\frac{m-1}{2}}$, where m is a prime number, is of the form $am \pm 1$.

Ex. To illustrate this, and the property of sixth powers mentioned in the XIth Prop., we shall take the 5th and 6th powers of 5 and 8.

$$5^5 = 3125 \text{ therefore } d_1 = (5+1) - (3+2) = 1.$$

$$5^6 = 15625 \text{ therefore } S_1 = 19, S_2 = 10, S_3 = 1.$$

$$8^5 = 32768 \text{ therefore } d_1 = (8+7+3) - (6+2) = 10, d_2 = 1.$$

$$8^6 = 262144 \text{ therefore } S_1 = 19, S_2 = 10, S_3 = 1.*$$

COR. From the property above demonstrated of the sixth powers of numbers prime to 9, it follows, that for every such base the seventh power has its ultimate sum equal to the base; that is, that $a^7 = m.9 + a$. For $a^6 = p.9 + 1$, $\therefore a^7 = p.a.9 + a$.

Ex. $5^7 = 78125, S_1 = 23, S_2 = 5.$

$$8^7 = 2097152, S_1 = 26, S_2 = 8.$$

* In these equations, d_1, d_2 , &c., express the 1st, 2d, &c., differences between the sums of the odd and even digits; S_1, S_2 , &c., express the 1st, 2d, &c., sums of all the digits.

PROP. XIII.—*Of Polygonal Numbers.*

In any series of polygonal numbers, n the root of notation being even, the sum of the digits of the $(s + \overline{n-1})^{\text{th}}$ term = sum of s^{th} term.

For every polygonal number is of the form $P = \frac{\overline{m-2} \cdot s^2 - \overline{m-4} \cdot s}{2}$, where m is the number of the order, and s that of the term.

For s substitute $s + \overline{n-1}$,

$$\begin{aligned} P' &= \frac{\overline{m-2} \times (s + \overline{n-1})^2 - (\overline{m-4}) \cdot (s + \overline{n-1})}{2} \\ &= \frac{\overline{m-2} \cdot s^2 - \overline{m-4} \cdot s}{2} + \frac{\overline{m-2} \cdot (2s\overline{n-1} + \overline{n-1}^2) - \overline{m-4} \cdot \overline{n-1}}{2} \\ &= P + \overline{n-1} \cdot \frac{\overline{m-2} \cdot (2s + \overline{n-1}) - \overline{m-4}}{2}. \end{aligned}$$

But n being even, the fractional expression is integer, whether m be even or odd. Therefore P and P' have same ultimate sum. (Prop. VI. Cor.)

If n be odd, the fractional expression is integer only when m is even.

The same inference might at once be drawn from the consideration, that the s^{th} term of any order of polygonals is the sum of s terms of an arithmetical series.

PROP. XIV.

If, as in our notation, n be even, the s^{th} , $(s + p \cdot \overline{n-1})^{\text{th}}$ and $(p \cdot \overline{n-1} - s + \overline{1})^{\text{th}}$ terms of a triangular series have all the same ultimate sum.

In this case, $m=3$, and s^{th} term = $\frac{s \cdot s + \overline{1}}{2}$

Therefore, $(s + p \cdot \overline{n-1})^{\text{th}} = \frac{2s \cdot p + p^2 \cdot \overline{n-1} + p \cdot \overline{n-1} + \frac{s \cdot s + \overline{1}}{2}}{2}$. Here the co-efficient of $\overline{n-1}$ is integer, whether p be odd or even: and therefore sum of $(s + p \cdot \overline{n-1})^{\text{th}}$ term = sum of s^{th} .

$$\begin{aligned} \text{Again, } (p \cdot \overline{n-1} - s + \overline{1})^{\text{th}} \text{ term} &= \frac{(p \cdot \overline{n-1} - s + \overline{1}) \cdot (p \cdot \overline{n-1} - s)}{2} \\ &= \frac{p^2 \cdot \overline{n-1} - 2s \cdot p + p \cdot \overline{n-1} - s \cdot \overline{n-1} + s^2 - s}{2}, \end{aligned}$$

and as the coefficient of $\overline{n-1}$ is again integer, whether p be odd or even. Sum of $(p \cdot \overline{n-1} - s + \overline{1})^{\text{th}} = \text{sum of } s^{\text{th}}$.

Ex. The triangular numbers are,—

1, 3, 6, 10, 15, 21, 28, 36, 45, 55, 66, 78, &c.

Sums, 1, 3, 6, 1, 6, 3, 1, 9, 9, 1, 3, 6, &c.

Here we observe that the 1st, 7th, and 10th, have the same sum ; so also have 2d, 6th, and 11th, and so on.

But the same series expressed in the tredecimal notation, and continued to 13 terms, is

	1, 3, 6, 1 ₀ , 12, 18, 22, 21 ₀ , 36, 43, 51, 60, 70
Sums,	1, 3, 6, 1 ₀ , 3, 9, 4, 1 ₂ , 9, 7, 6, 6, 7

Here the 13th term has a sum, 7, different from the first.

But if we take $p=2, \overline{n-1}=12$, and $s=11$,

then $p.\overline{n-1}-s+1=24-12=12$; therefore 11th and 12th have same sum.

If $s=10, 26-11=13$, therefore 10th and 13th have same sum, and so on.

Note.—In the decimal series it may be observed, that not only the 1st, 7th, and 10th terms have one for their ultimate sum, but also the 4th, 13th, &c.

This happens, because in the decimal scale, $4=\frac{n-2}{2}$;

but the $\frac{n-2}{2}$ th term $=\frac{\frac{n-2}{2} \cdot \frac{n}{2}}{2} = \frac{n^2-2n}{8} = \frac{n.\overline{n-2}}{8} = n$, and, consequently, its ultimate sum is 1.

PROP. XV.

If the general term of any series be $ax^m + bx^{m-1} + cx^{m-2} \dots l$; then evidently, if $x+n-1$ be substituted for x , the result will be the original term + a multiple of $n-1$. Or, as in all the preceding forms, the same ultimate sum will recur after $\overline{n-1}$ terms.

If the general term be quadratic $= ax^2 + bx + c$.

Let $x_1=y-x$, then $ax_1^2=ay^2-2axy+ax^2$
$bx_1=by-bx$
$c=c$

Therefore $ax_1^2 + bx_1 + c = ax^2 + bx + c - 2\overline{a}y + 2\overline{b}x + \overline{a}y + \overline{b}y$
 $= ax^2 + bx + c + \overline{a}y + \overline{b}x - 2x$.

Now, let y be assumed such, that $ay+b=p.\overline{n-1}$, then the x^{th} and x_1^{th} terms will have same ultimate sum.

Ex. Let x^2+3x+1 be the general term. Substitute for x successively 0, 1, 2, &c., we have the series,

	1, 5, 11, 19, 29, 41, 55, 71, 89, 109, &c.
Ultimate sums,	1, 5, 2, 1, 2, 5, 1, 3, 8, 1, &c.

Here $a=1, b=3$. If, therefore, $y+3=9$, or $y=6$, the two terms in which the

numbers substituted for x are together equal to *six*, will have the same ultimate sum. Thus, in the above series, the 1st and 7th terms, in which 0 and 6 are respectively substituted for x , have the same sum; so also the 2d and 6th, in which 1 and 5 are substituted, and so on.

PROP. XVI.

In the series whose general term is $\overline{m.m+1} \dots \overline{m+r-1}$, if $m+n-1$ be substituted for m , the ultimate sum of digits will remain as before.

If to each factor we add a , the term becomes

$$\overline{a+m} \times \overline{a+m+1} \times \overline{a+m+2} \dots \overline{a+m+r-1} \\ = a.r + p.a^{r-1} + q.a^{r-2} \dots + \overline{m.m+1.m+2} \dots \overline{m+r-1},$$

where a is a factor of every term except the last. Let $a=n-1$ then term

$$\overline{m+n-1.m+n}, \&c. = \overline{m.m+1}, \&c. + s.\overline{n-1}, \text{ whose ultimate sum} = \text{that of} \\ \overline{m.m+1.m+2}, \&c.$$

Taking the same general term, if $m+m_1=n-r$, $m^1=\overline{n-1-m+r-1}$.

$$\text{Therefore } \overline{m_1.m_1+1.m_1+2} \dots \overline{m^1+r-1} = \overline{n-1-m+r-1} \\ \times \overline{n-1-m+r-2} \\ \times \&c. \\ \times \overline{n-1-m}$$

In this product, $\overline{n-1}$ will enter as a factor into every term except the last, which is $\overline{m.m+1} \dots \overline{m+r-1}$ with the sign \pm according as r is even or odd.

If r be even, the m^{th} and m_1^{th} terms will have the same ultimate sum; but if r be odd, the sums will be complementary.

All the terms from the $\overline{n-r}^{\text{th}}$ to the $\overline{n-1}^{\text{th}}$ must have $\overline{n-1}$ for their sum; because $\overline{n-1}$ must manifestly be a factor in each of them.

Ex. Let $r=2$. Series is 1.2, 2.3, &c.

$$= 2, 6, 12, 20, 30, 42, 56, 72, 90, 110$$

Sums are, 2, 6, 3, 2, 3, 6, 2, 9, 9, 2, &c.

Let $r=3$, or series 1.2.3, 2.3.4, &c.

$$= 6, 24, 60, 120, 210, 336, 504, 720, 990$$

Sums, 6, 6, 6, 3, 3, 3, 9, 9, 9, &c.

In the 1st example, r being even $=2$, $m+m_1=10-2=8$; therefore the 1st and 7th, 2d and 6th sums ought to be identical.

In the 2d, r being odd $=3$, $m+m_1=10-3=7$; therefore the 1st and 6th, 2d and 5th, &c. sums are complementary.

PROP. XVIII.—Series of Figurate Numbers.

If the series be m , $\frac{m.m+1}{1.2}$, $\frac{m.m+1.m+2}{1.2.3}$, &c., where each term is the m^{th}

term of the 1st, 2d, 3d, &c., order of figurates, the whole may be reduced to a common denominator, and represented thus :—

$$\frac{2.3.4 \dots \overline{m-1}.m}{1.2.3 \dots \overline{m-1}}, \frac{3.4.5 \dots m.\overline{m+1}}{1.2.3 \dots \overline{m-1}}, \&c.,$$

of which the numerators follow the law of the series treated in the last proposition. If, therefore, in the series of figurates, the successive sums be taken, and each multiplied by $1.2.3 \dots \overline{m-1}$, the products will form a series recurring after $\overline{n-1}$ terms.

Ex. $4 + \frac{4.5}{1.2} + \frac{4.5.6}{1.2.3}, \&c.$

$$= 4 + 10 + 20 + 35 + 56 + 84 + 120 + 165 + 220 + 286 + 364 + 455$$

Sums, = 4, 1, 2, 8, 2, 3, 3, 3, 4, 7, 4, 5,

Multiplying by $1.2.3=6$, the sums of products of sums become

$$\underline{6, 6, 3, 3, 3, 9, 9, 9, 6, | 6, 6, 3.}$$

PROP. XIX.—*Of the Ultimate Difference of Digits.*

It is a well known property of digits, that the remainder, when any number is divided by the root of the scale employed + 1, is equal to the ultimate remainder of the even digits subtracted from the odd; or, using a notation similar to that we have before employed, that $N = p.\overline{n+1} + d_n$. As, however, d_n must always be +, if at any step the sum of the even digits be greater than that of the odd, $\overline{n+1}$, or such a multiple of $\overline{n+1}$ as will make it the greater, must be added to the latter.

From this fundamental proposition, a series of propositions analogous to the preceding may be deduced, relating, not to the sums, but to the differences of the digits. The demonstrations are so similar to those already given, that I shall merely illustrate the matter by examining the succession of differences in the series treated in Prop. XV:

The general term was $ax^2 + bx + c$.

Let x become $x + p.\overline{n+1}$, the term becomes

$$ax^2 + 2apx.\overline{n+1} + a.p^2.\overline{n+1}^2 + bx + b.p.\overline{n+1} + c = ax^2 + bx + c + q.\overline{n+1}.$$

Hence the remainder, after dividing by $\overline{n+1}$, will be the same in both cases, or the ultimate difference will recur after $\overline{n+1}$ terms.

Next, let $x + x_1 = y$, then

$$ax_1^2 + bx_1 + c = x^2 + bx + c + \overline{ay + b.y - 2x}.$$

Assume $ay + b = n + 1$, then the two terms will have the same ultimate difference.

Ex. Take as before for the general term $x^2 + 3x + 1$,

here $a=1$, $b=3$, $\overline{n+1}=11$, therefore $y+3=11$, or $y=8$.

If, therefore, the sum of the two numbers substituted for x in the expression $x^2 + 3x + 1$, be 8, the differences in the two cases will be identical.

Substituting successively 0, 1, 2, &c., we have the series

$\overset{0}{1}, \overset{1}{5}, \overset{2}{11}, \overset{3}{19}, \overset{4}{29}, \overset{5}{41}, \overset{6}{55}, \overset{7}{71}, \overset{8}{89}, \overset{9}{109}, \overset{10}{131}, \overset{11}{155}, \overset{12}{181}, \&c.$

Differences, 1, 5, 0, 8, 7, 8, 0, 5, 1, 10, 10, 1, 5, &c.,

where we see, 1st, that the differences recur after 11 terms; 2d, that if 0 and 8, 1 and 7, 2 and 6, &c., be substituted for x , the differences are identical.

EDINBURGH, *December 2. 1845.*

XII.—*Results of the Makerstoun Observations, No. I. On the relation of the Variations of the Horizontal Intensity of the Earth's Magnetism to the Solar and Lunar Periods. With Two Plates. By J. A. BROWN, Esq. Communicated by Sir T. M. BRISBANE, Bart.*

(Read January 5. 1846.)

1. THE following communication is intended to be the first of a series, in which I propose to consider the results of observations made at Makerstoun, near Kelso, Roxburghshire, in the Observatory of the President of this Society. These observations, and the tabular results, will be found ultimately in the volumes of Makerstoun Observations, constituting volumes of the Society's Transactions.

2. It has been found convenient to separate the observations of the varying intensity of the earth's magnetism into two parts, namely, its resolved components in the horizontal and vertical planes. I shall treat at present of the variations of the horizontal component. These variations are observed by means of the bifilar magnetometer, an instrument devised by M. GAUSS, and modified by Dr LLOYD, described in the Introduction to the Makerstoun Observations for 1842. It consists simply of a magnetic bar, suspended by two silver wires, the latter being twisted out of a vertical plane, the magnet is forced from the magnetic meridian; the variations of its position afterwards are due to two causes, namely, variations of the horizontal component of the earth's magnetic force, or of the moment of free magnetism of the bar; the former are due to changes of the total force or of its dip, the ordinary variations of the latter are due to temperature; and it is, accordingly, a point of much importance to determine the correction for temperature with accuracy, in order that the simple effect of varying intensity may be obtained. I have pointed out, in a paper read before this Society last session, the imperfections of the method usually adopted for the determination of this correction, and the method which has been adopted for the correction of the Makerstoun Observations. I shall afterwards exhibit an example of the very different results to be deduced, after correcting observations by the two methods (11).

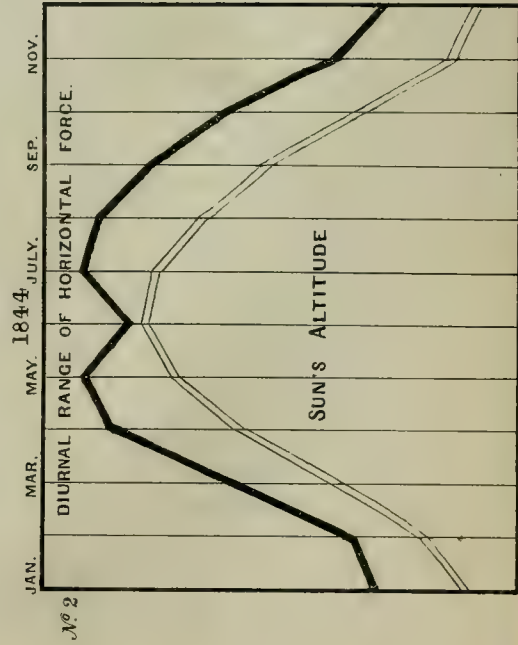
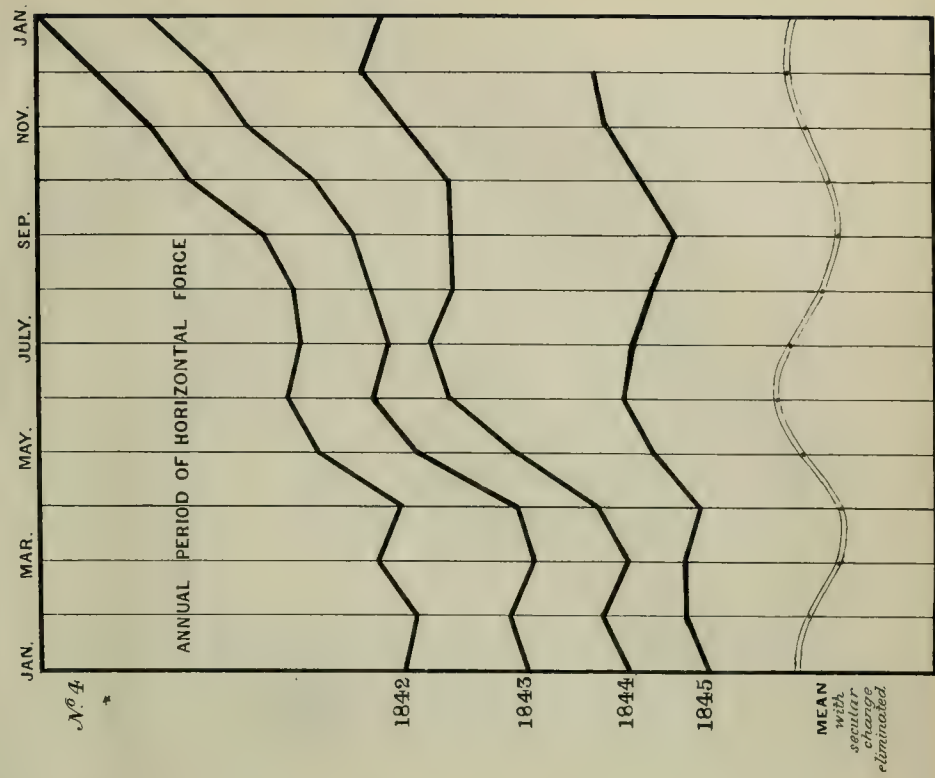
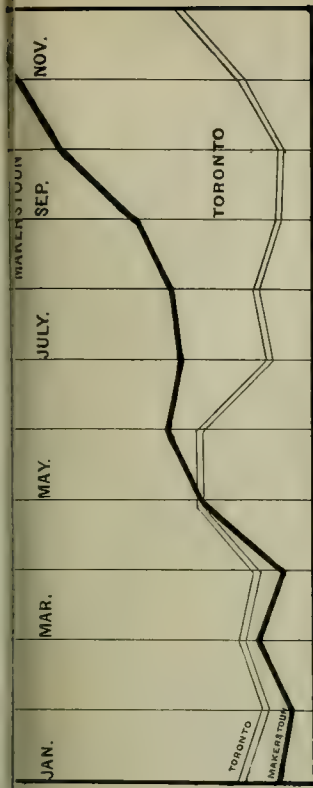
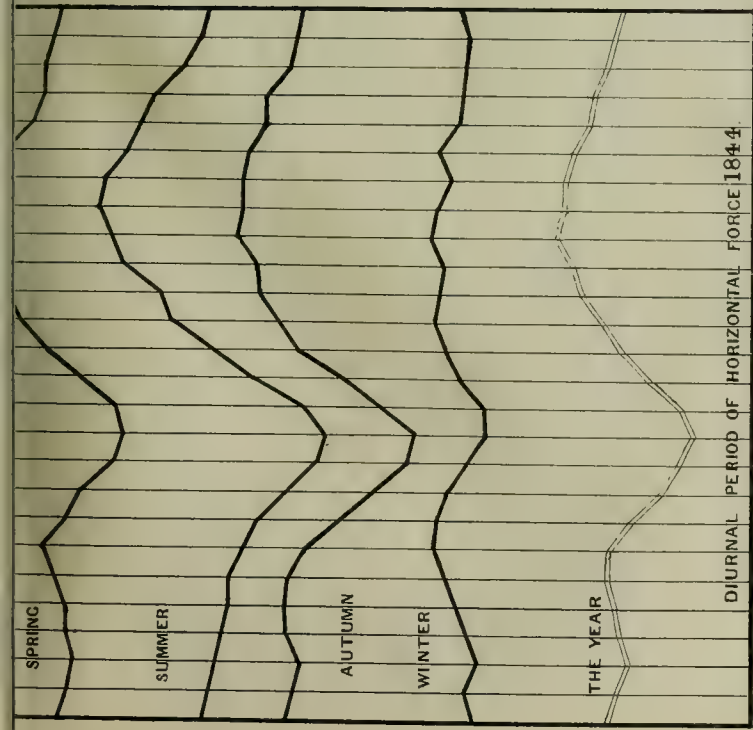
3. The horizontal force varies throughout the solar day, having, in that period, two maxima and two minima. The hours of the principal maximum and minimum were first pointed out by M. HANSTEEN, but I am not aware of the first determinations of the secondary points. In the years 1844 and 1845, observations were made at Makerstoun every hour excepting on Sundays. (See Curve, No. 1, Plate III.) From the means of the whole observations for the year 1844, the principal minimum occurs about 20^m past 10 A.M. (Makerstoun mean solar time is used throughout), or exactly when the sun is on the magnetic meridian of Makerstoun; the force then increases rapidly till between 3 and 4 P.M., when there is a slight inflexion; again it increases with its previous rapidity till about 5½ P.M., when the

maximum is attained. It now commences to decrease slowly till 8 P.M., more rapidly from 8 till 9, causing another inflexion in the curve, slowly again till 2^h 20^m A.M., when there is a minimum; the force then increases slightly till 5^h 30^m or 40^m when a maximum occurs, after which it diminishes rapidly till 10^h 20^m A.M., the period of *the* minimum. These hours differ somewhat from the periods obtained at other observatories; and while some part of these differences may be due to errors of temperature correction, I do not think that such errors will altogether account for them, but that the accurate periods of maxima and minima will be found to differ at different places. At Toronto in Canada, for example, *the* maximum occurred a little after 4 P.M. in 1842; and as the mean temperature of the magnet at the succeeding observation hour differs but little from that at 4^h, the period cannot be affected by temperature. Some observatories shew the maximum as late as 7 P.M. It does not, however, seem improbable, that the periods of maxima and minima should differ at different places, when it is known that these periods vary at the same place in the course of the year; at Makerstoun, in 1844, the afternoon maximum occurred as early as 3^h 10^m in December and January, and as late as 6^h 50^m in June; the minimum at 10^h 20^m A.M. in the winter months, and at 9^h 40^m A.M. in June; the A.M. maximum occurs at 6^h 40^m in December, and about 5^h in the summer months, while the earliest minimum occurs nearer midnight in winter than in summer. In this way the periods of the principal maximum and minimum approach to each other, and to noon in *winter*, and remove from each other, and from noon in summer. (See Curve, No. 1.) The reverse to some extent takes place with regard to the periods of the secondary maximum and minimum, which remove from each other in winter, and approach each other in *summer*, till in June the maximum and minimum seem to destroy each other.

The morning maximum is greater than the afternoon one in December;—in November, January, and February, they differ but little from each other; and in December, January, and February, the two minima are nearly equal.

4. The inflexions noted in the mean curve about 3 P.M. and 9 P.M., become minima in the winter months, so that there are then three or four maxima and minima; the smaller ones nearly compensate each other in the mean of the winter months, as they occur at different hours in each month.* I shall consider the cause of these secondary afternoon maxima and minima on another occasion. With regard to the 2 A.M. minimum and 6 A.M. maximum, these seem nearly to vanish in the summer months. In the means for the months of June and July they cannot be detected, excepting that the intensity decreases more rapidly after 6 A.M. than before it; it should not be concluded on this account that this maximum and minimum do not exist. Having projected the hourly observations made in each day of June and July, I have not found one day in ten on which the secondary maxi-

* November, December, and January, have been taken as the three winter months.





mum and minimum do not exist; the periods, however, are variable, and in this way they seem to balance each other in the mean; the maximum of one day occurring at the time of the minimum of another. (See Curve, No. 1.) It will be remarked also, with regard to the mean of the summer months, that the descending branch, after the 6 P.M. maximum, has a strongly marked concavity, indicating a tendency to a minimum which does not decidedly shew itself.

5. It has been shewn by Drs LAMONT and LLOYD, that the morning maximum seems to occur throughout the year a little before sunrise, and the afternoon maximum a little before sunset. I would mention another coincidence; the times of maximum atmospheric pressure and minimum intensity are the same throughout the year, and also the times of minimum atmospheric pressure and maximum intensity. There is also a secondary minimum of pressure occurring about the same hour as the secondary maximum of intensity, and the secondary maximum of pressure occurs about 10 P.M., the period of an inflexion in the intensity curve.

6. The diurnal range of intensity is least in January, and greatest in July, being 0.000641 in January (the whole horizontal force being unity), and 0.003396 in July, or five times greater in July than January; the mean diurnal range for the year is 0.002041, being almost exactly the mean of the ranges for January and July, and the same as the ranges for March and October. I have projected the mean range for each month in 1844, and also the mean altitude of the sun for each month. (See Curve, No. 2, Plate III.) There is a considerable similarity in the forms of the two curves; a marked inflexion occurs in June in the range curve. As June is remarkably free from irregularities, it seems to me probable that the range for June is the true diurnal range freed from those irregularities termed disturbances. If so, we may perhaps consider the deviation of the ranges for other months from the curve of altitudes as due to disturbances; in which case we might conclude that disturbances increased the diurnal range most in April and October, and least in February and June. This, however, requires other proof.

7. At Makerstoun, in 1844, each degree of the sun's altitude was equivalent to a diurnal range of about 0.00006 of the horizontal intensity.

8. The mean intensity at midnight and at 1^h P.M. are each equal to the mean of the year, and the means at these hours for each month differ very little from the mean of the month. This leads me to the monthly means, and their relation to the period of a year.

9. In June 1845, I shewed to the Physical Section of the British Association, that the Makerstoun observations of horizontal intensity for 1842 indicated well-marked double maxima and minima in the course of the year; and that as this result had been obtained from the means of only four observations in each day, I had determined the temperature correction approximately for the Toronto bifilar magnet, by the method already referred to, and applied this correction to

the monthly means, as deduced from the observations of horizontal force made at Toronto in 1842, at the following hours:—First, the hours corresponding to the Makerstoun hours; this could not be done exactly, as the observations were made at Toronto every two hours only, and for each of two of the Makerstoun hours, the mean of two of the Toronto hours had to be taken; thus, one of the observation hours at Makerstoun was 7 A.M.; the mean of the Toronto observations at 6 and 8 A.M. was taken as equivalent (see the open curve, No. 3, Plate III.) Second, from the whole two-hourly observations (see the open curve, No. 5, Plate IV.) The monthly means from both these methods, gave the same, or nearly the same, annual period as the Makerstoun observations.

10. I have since then verified this period by the monthly means of the observations made at Makerstoun in the years 1843, 1844, and 1845. I may first refer to the Makerstoun and Toronto curves for 1842, which were exhibited to the Physical Section of the British Association at Cambridge. (See Curves, No. 3, Plate III.) From these I concluded that the horizontal force of the earth's magnetism has an annual period, consisting of a maximum at each solstice, and a minimum at each equinox; both curves present a curious inflexion in March, which I then considered due to some irregularity. The monthly means for the years 1842, 3, 4, and 5, have also been projected together; (see Curves, No. 4, Plate III.) the whole speak the same language, excepting that the inflexion in March 1842 does not occur in the other years, unless it may be said to do so in 1845. All the curves shew a considerable *secular* change, the horizontal intensity increasing throughout the whole period. Whether this be really an increase of the earth's horizontal intensity, or due to some instrumental cause, cannot be positively stated; it is not at all likely to be due to an increase of the free magnetism of the bar, which is suspended at right angles to the magnetic meridian; the only apparent and possible cause is a stretch of the suspension wires; it must be a matter for consideration, whether such a cause is likely to operate for such a period, and to nearly an equal amount for two years. Whether a secular change of horizontal intensity, or due to a stretch of the wires, it is evident that it may be considered as a regular increase throughout the year. Upon this hypothesis I have eliminated this increase from the monthly means of the last three years, and projected the mean below the others. This curve shews more strikingly the annual period of solstitial maxima and equinoctial minima. The minima have nearly the same value; the summer maximum is greater than the winter maximum, but so little, that an error of a thirtieth in the amount of the temperature correction, would account for the difference. The annual range from the mean of the three years is 0.000724, or about the mean diurnal range for the three winter months.

11. M. HANSTEEN concluded from his observations, that there was a maximum of horizontal intensity in December, and a minimum in June. Colonel SABINE con-

cludes from the Toronto observations for 1842 (corrected by the usual method), that there is a maximum in June, and a minimum in December. I have projected the monthly means of the Toronto observations as corrected by the usual method. From these Colonel SABINE draws his conclusion. Under it I have projected the temperature of the magnet in a broken line, and below both, the means from the two-hourly observations, as corrected approximately by myself. (See Curves, No. 5, Plate IV.) These will shew how much depends on the accuracy of the correction in arriving at sound conclusions. I conceive that the consistency of the results at which I have arrived, independently of other considerations, will leave little doubt as to which method of obtaining the corrections should be adopted.

12. It has been already mentioned (10), that the apparent secular change consists of a considerable increase of horizontal intensity. Throughout the whole period, the rapidity of increase has been diminishing, and it is much less in 1845 than in any of the previous years. Of all the puzzling problems in terrestrial magnetism, that of connecting the secular change with some known or observed phenomenon has been the most difficult; any fact, therefore, tending to this, will have interest. One of the first questions which I proposed to myself, connected with it, was whether all hours of the day were equally affected by the secular change? In order to answer this more distinctly, the annual period was eliminated from the monthly means, or, which is nearly the same thing, the mean of each month was reduced to the straight line passing through January and December 1844.* I then found that the mean horizontal force in the first six months of the year 1844, was almost constant one hour *after* the period of the morning maximum, and also that it was almost constant for the last six months, one hour *before* the period of the evening maximum. When the diurnal curve for each month was projected, I found the curves for the first six months to pass through a space of 3-4ths of a scale division in the ordinate of 6^h 40^m A.M., with the exception of the curve for February, which is very irregular there. The curves for the last six months pass through a space of 1½ scale divisions, in the ordinate of 4^h 40^m P.M.; the increase of horizontal force from January till December was 18 scale divisions. I next eliminated all the larger disturbances from the monthly means of each hour, but this neither affected the periods of the nodes, nor the values of the ordinates in which they were contained. In this way, then, the horizontal force in its secular progress, seems to rest one foot during the first half of the year about an hour after

* The line should have been drawn through January 1844 and January 1845, but that there is an irregularity in the progress of the horizontal force from December 1844 to January 1845, compared with the previous years. I have, however, also reduced the means to the line passing through January 1844 and January 1845, and find the ordinate of the morning node slightly increased, but that for the evening node diminished.

the morning maximum, and extends the other forward at all other hours of the day, making the greatest strides at the time of the afternoon maximum. During the second half of the year, it rests the previously advancing foot about an hour before the evening maximum, and brings the lagging foot forward at the other hours, but with the greatest rapidity at the time of the principal minimum.

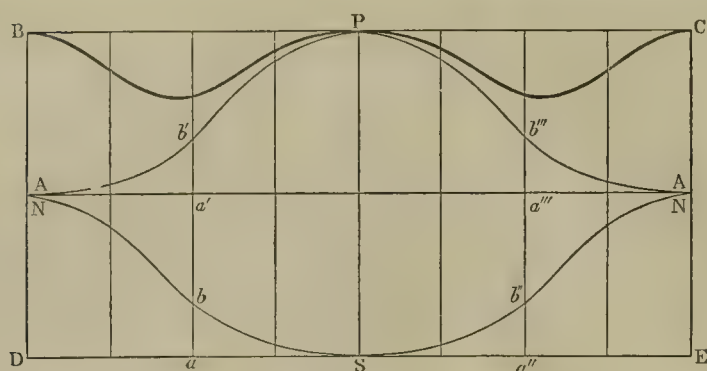
13. There is perhaps nothing more difficult in groping for the laws which regulate certain phenomena than the separation of the effects due to different causes; but it is quite obvious that, before we can arrive at any sound conclusion as to simple laws, this must be done. In the determination of the diurnal period all the observations at each hour for a calendar month or year are summed, and the means taken; in these summations are included several irregularities named disturbances; if the disturbances occurred equally positive and negative at the same hour, or were equally distributed over the twenty-four hours, a large enough series of observations would serve to eliminate them; neither of these suppositions seems to hold, and accordingly, certain hours in some months are more affected by disturbances than the same hour in other months, or than the other hours of the same month; the diurnal curve, therefore, is complex. There are other causes, as will be seen afterwards, which render it more so.

14. In the attempt to determine whether the horizontal intensity varies with the moon's declination, the days were numbered from the day of the moon's greatest N. declination, counting that day 0 till it returned to the greatest N. declination again; and, as 13 of the moon's revolutions, with regard to node, are equivalent to 12 lunations, and nearly to a year, the 13 revolutions, with regard to declination, were selected for summation; as, by this means, any effect due to varying phase, or to annual period, would be eliminated. The mean intensity for each of the 13 days on which the moon had its greatest N. declination were then summed together; the means for the 13 days numbered 1, in which the moon was moving south, and so on. For the purpose of verifying the result thus obtained, similar summations of the observations for 1845 were made; in this case, however, only 12 revolutions with respect to declination, were obtained, so that any effect of phase will not be perfectly eliminated. No attempt has in either case been made to eliminate disturbances. The results of these summations were projected, having previously eliminated the effect of secular change. (See Curves, No. 6, Plate IV.) The curve, from the observations of 1844, indicates a maximum about 2 days after the moon has attained its greatest S. declination, and a maximum about a day after it has attained its greatest N. declination—the maxima have nearly equal values, so also have the minima. The branches ascending to and descending from the period of greatest S. declination are greatest; so that the periods of minima are nearer, the greatest N. declination being about 5 days before and after it. The curve deduced from the observations of 1845 shews the maxima nearly at

the same periods as in 1844; but the branch ascending to and descending from the period of greatest N. declination is greatest, the periods of minima being nearer the greatest S. declination, namely, about 5 days before it and after it. The curve for 1845 is, however, more irregular after the S. declination maximum than in any part of the other curve. Besides the non-elimination of the effect connected with varying phase and disturbance, there is another possible cause of difference, namely, the varying distance of the moon; the period of perigee is about two days before the greatest S. declination in 1844, and two days after it in 1845. It should also be remembered that each point in these curves is a mean of only 12 or 13 days; as for the minor irregularities in the positions of the points, it is obvious that, as there are 27 days between the periods of the moon's greatest N. declination, if the full moon occurs on the day of greatest N. declination in one month, it will occur on the second day after the greatest N. declination on the next month, the fourth day on the next, and so on. It will be seen afterwards that this will cause a slight irregularity. It is on this account that I have projected the curves among the points, giving a preference to the mean positions of each two points.

15. The similarity of the positions of maxima and minima in these curves, having the moon's declination for abscissæ to the annual curve, or that having the sun's declination for abscissæ, is at once evident; by taking the mean of the two lunar curves, however, the cases will be identical, for then the moon's perigee will occur at the time of its greatest S. declination, and its apogee at the time of the greatest N. declination; this is the case with us for the sun. The resulting means have been projected below the other curves. By comparing the mean curves of No. 4 and No. 6, it is at once obvious that the facts are the same for both the sun and moon. I conceive, then, that I am justified in stating that the same relation exists for the moon as for the sun between the variations of the horizontal component of the earth's magnetic intensity, and the variations of declination and parallax.

16. We have, then, a law connected with two periods, namely, distance and declination. To which does it belong, or does it belong to both? It will take a few years' observations to determine this for the moon: it may be determined for the sun by observations for the annual period made in the Southern Hemisphere. Is there a maximum at the greatest N. declination, and also at the greatest S. declination; or have changes of declination no effect? and are the maxima due to the moon's or sun's distance solely? The supposition that at first sight seems most probable is, that these variations are due to both; that a maximum occurs at the time of perigee, a minimum at the apogee, a maximum at the greatest N. declination, and a minimum at the greatest S. declination. It may easily be shewn that two regular curves having these arguments, when superposed, would



produce two minima. Thus, if APN be the curve due to distance, NSN that due to declination, the curve BPC , produced by the superposition of their ordinates will have two minima, if the sum of the ordinates $ab + a'b'$ be less than the sum $DN + AB$, and $a''b'' + a'''b'''$ be less than $EN + AC$.

The fact, that in both the solar and lunar curves the maxima are nearly equal, is against the supposition that both distance and declination are equally concerned, as it seems rather improbable that the effect of increasing distance should precisely counterbalance the effect of increasing N . declination. We have, however, much more singular cases of compensation in the motions of the heavenly bodies.

17. The range of the lunar declination curve for 1844 is 0.000455; for 1845, 0.000390; and, for the mean of both years, 0.000380.

18. I have already mentioned (14), that, by taking 13 revolutions of the moon, with respect to its declination, we eliminate any effect due to the varying phase of the moon. Similarly, if we take 12 lunations, and sum the mean intensity for the twelve days on which the moon was full, the twelve days on which it was one day old, and so on, we eliminate the effect of varying declination, and also the annual period very nearly. If, however, we may consider the intensity with respect to N . declination similar to that with respect to S . declination, it is evident that 6 lunations will be sufficient to eliminate the effect of declination. (See Curves, No. 7, Plate IV.) I have had the observations during the six summer lunations for 1844 summed by themselves, and also those during the six winter lunations; the mean intensities for both, for each day of the moon's age, have been projected, and also the mean for the year. All indicate a maximum of intensity about two days after the new moon, and a minimum perhaps two days after the full moon; the summer curve has an irregularity before full moon, and its range is only half that for the winter months. The minor irregularities may be accounted for in the same way as for the declination curve. The range for the

winter months is 0.001040. It appears to me, however, that it is exaggerated, owing to the curious fact, that the chief negative disturbances in 1844 occurred about the time of full moon.

19. It has not appeared to me necessary to verify this law by the result of another year's observations. Each of the winter months of 1844 shews the facts as completely as the mean; in the summer months, the result is not so evident. It would appear as if the effect of phase swallowed up the effect of declination in the winter, while the reverse occurred in summer. I have projected the means of the horizontal intensity for each day from January 4th till April 3d, 1844, including three synodical periods. (See Curves, No. 8, Plate IV.) In each period the curve shews the facts most completely; and the lunations in September, October, November, and December, shew them perhaps better. The periods of greatest N. and S. declination, and of the syzygies, are indicated on the curves, the open O being full moon. There are several curious facts, in connexion with the observations projected, which I cannot enter fully into at present; I may remark, however, the appearance of a weekly period. No observations being made on Sundays, breaks occur in the curve, where the intensity for these days should appear. A great disturbance spoils somewhat the form of the curve in March; the point belonging to the 29th of March would occur about $1\frac{1}{2}$ inches below the margin.

20. The law of the variation of the earth's horizontal intensity with the moon's phase, is one productive of many speculations. There is an evident connexion of the great diurnal variations of the horizontal intensity, with reference to the sun's hour angle; there is also a strongly marked connexion between the diurnal range and the sun's altitude; and we have a certain connexion between the sun's declination and the annual period. Are these connected with the heating power of the sun, its light, or its magnetism? Sir JOHN HERSCHEL has stated, that, as the sun's rays shine with their whole force on the moon's surface for a fortnight, unstopped by an atmosphere, the heat of the surface must be much more intense than that of a tropical summer; while, after the next fortnight, the cold must be more severe than that of a polar winter. M. COURNOT, the French translator of Sir JOHN HERSCHEL's Treatise on Astronomy, opposes this opinion, and argues that, as there is no atmosphere to prevent radiation, our knowledge of the laws of radiant heat would lead to the conclusion, that the temperature of the moon's surface would differ little at the times of new and full moon.* Supposing Sir JOHN HERSCHEL's opinion accurate, if we could conceive the moon as a magnetic body acting by induction on the earth, then, according to our knowledge of the effect of heat on magnetic bodies, its intensity would be greatest when it was coldest, and least when warmest: the period of greatest cold we should expect

* Quoted by M. FRANCOEUR; *Uranographie*, p. 97.

to be a day or two after the new moon, and of the greatest heat a day or two after full moon, in the same way as our periods of greatest cold and heat are after the winter and summer solstices. This seems to agree with the periods of maximum and minimum horizontal intensity. If M. COURNOT be right, or if Sir JOHN HERSCHEL's supposition be insufficient, then we must look to the solar emanations reflected or radiated from the moon for the causes of the variations of the earth's magnetism, and to our atmosphere for a cause of the supposed retardation of epoch.*

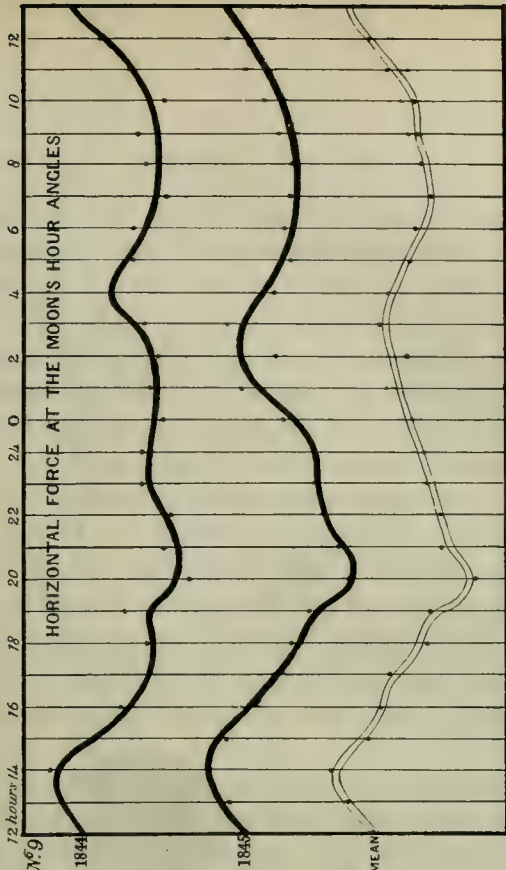
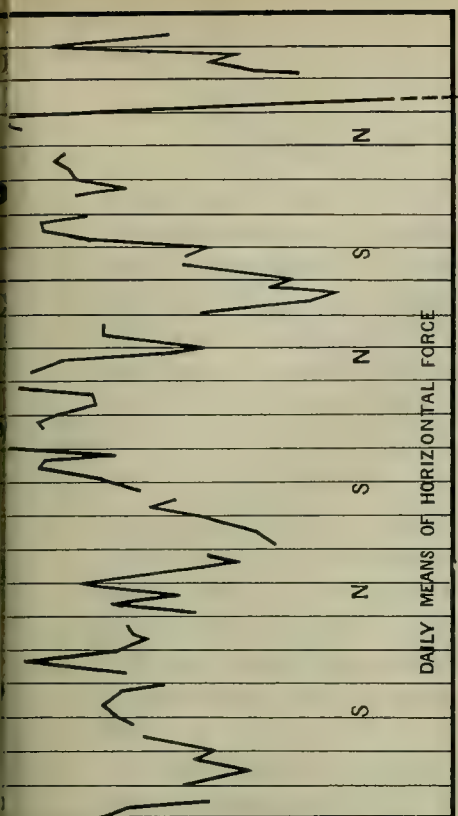
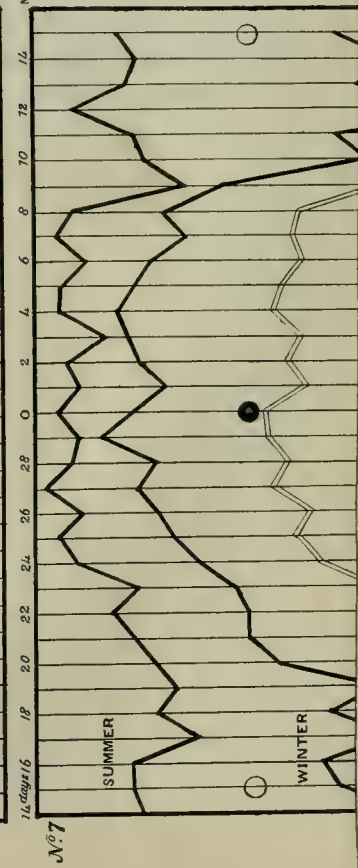
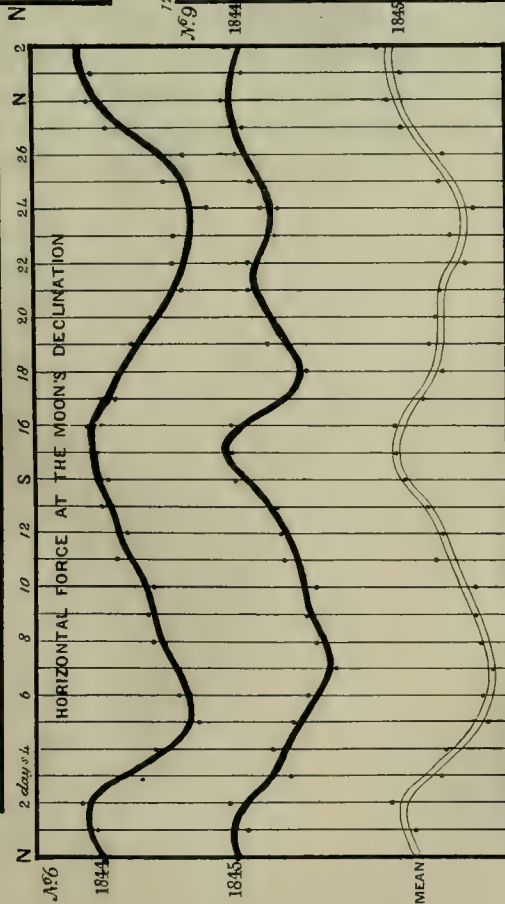
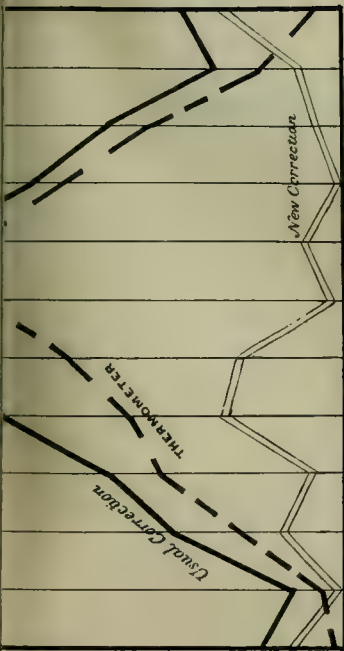
21. The connexion of lunar phase and horizontal intensity was first noticed by me in July 1845. I am not aware of any investigations on the relation of the horizontal intensity to the lunar month, excepting a paper by M. HANSTEEN, of which I have lately merely seen the title, which refers to the connexion of the horizontal intensity with the moon's ascending node.

22. Having mentioned some time ago to Professor FORBES, that I was engaged in examining the relation of the lunar periods to the variations of the earth's magnetism, I learned from him that M. KREILL of Prague had stated, in his volume of observations for 1842, that the horizontal intensity was greater at the moon's passage of the inferior meridian, than at its passage of the superior meridian. I know not whether M. KREILL has verified his statement, or to what extent his observations prove it.† I have now discussed the observations for 1844, with reference to this period, and have verified my results by a similar discussion of the observations for 1845. I shall, at present, merely state the leading facts, and leave the details to another communication.

23. The observation at the hours on which the moon was on the meridian were termed 0 hours, the observation the hour after one hour, and so up to 24; as the moon takes about 25 hours to return to the meridian again. On some occasions there were only 24 observations between the two passages; in these cases (few in number) the hour of passage was reckoned as 24 hours, and also as 0 hours of the next day. The summations for the hours were made for each month; I shall only speak of the means for the whole year in this communication; these means have been projected. The large disturbances have been eliminated from the summations for 1844 and 1845. (See Curves, No. 9, Plate IV.) Any observation in 1844 which shewed a difference from the monthly mean, for the hour at which

* It is evident that the variations of horizontal intensity may be due either to changes of the total intensity, or of its direction; any reasoning, therefore, on these facts must be necessarily incomplete, until we are certain of the actual effect.

† I have, since this was written, been favoured by Professor FORBES with a copy of M. KREILL's table for the horizontal force during the moon's hour angle. It indicates a minimum of intensity about two hours before the meridian passage, and maximum peaks at 12^h and 15^h, giving the interpolated period of maximum about 1½ hours after the inferior meridian passage; the latter period agrees completely with my own conclusion, the former differs about three hours from my result.



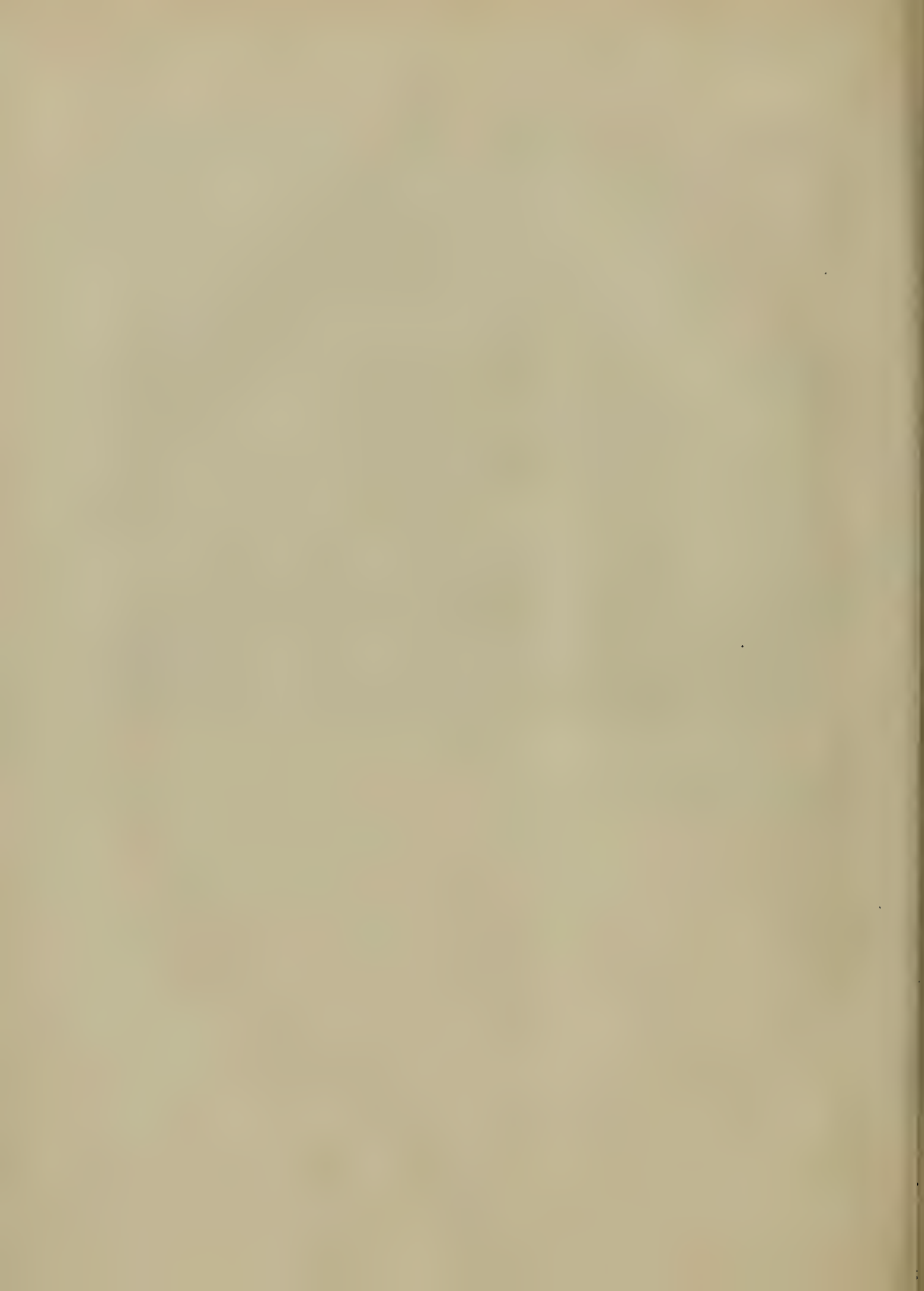
CURVES
to illustrate changes of
HORIZONTAL MAGNETIC FORCE
at
MAKERSTOWN.



it was made, of above twenty scale division (twelve times the resulting range of horizontal force in the lunar hour angle curve), was rejected, and an interpolated quantity substituted; this elimination, however, was not found to affect the periods of maxima and minima; it reduced the range, and rendered the curve somewhat more regular. In the observations for 1845, as a farther check on these eliminations, a different test number was employed, namely, forty scale divisions, or nearly twenty-five times the resulting range of the lunar hour angle curve.

24. In both years, *the* minimum occurs at 20^h, or 5^h before the meridian passage; *the* maximum at 14^h, or about 1½^h after the inferior passage: in both years, *a* minimum occurs at 8^h; in 1844, *a* maximum occurs before 4^h; and in 1845, before 3^h. The maximum at 3^h, for 1845, differs little from the maximum at 14^h; but the maximum at 4^h, in 1844, is considerably less than the maximum at 14^h. The coincidences of these results may be considered extraordinary, when it is known that the range in 1844 is only 0·000211, and in 1845 only 0·000213, or less than the effect of one degree of Fahrenheit on the magnetism of the bifilar bar.

25. Several questions spring from this result of the connexion of the intensity with the moon's hour angle. Does the range of the lunar hour angle curve vary with the moon's declination? If so, then we do not eliminate the lunar effect from the solar day curve by a monthly or any other summation. Do the periods of maxima and minima vary throughout the lunar month in the lunar hour angle curve, as they do through the year in the solar day curve? These questions I shall endeavour to examine at another opportunity.



XIII.—*On the Decomposition and Dispersion of Light within Solid and Fluid Bodies.* With a Plate. By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edin.

(Read 2d February 1846.)

HAUY*, and other mineralogists, observed the two colours which are visible in several varieties of fluor-spar. He regarded the two tints as complementary, and explained them, as he did every other analogous phenomenon, by a reference to the colours of thin plates. In describing a species of dichroism noticed by Dr PROUT† in the purpurates of ammonia and potash, Sir JOHN HERSCHEL ascribes the green reflected light‡ “to some peculiar conformation of the green surfaces producing what may be best termed a *superficial colour*, or one analogous to the colour of thin plates, and striated or dotted surfaces.” And he adds—“A remarkable example of such superficial colour, differing from the transmitted tints, is met with in the green fluor of Alston Moor, which, on its surfaces, whether natural or artificial, exhibits, in certain lights, a *deep blue* tint, not to be removed by any polishing.”

Having, many years ago, found the same property in the Derbyshire fluor-spars, I was led to study it with particular attention; and, in 1838, I communicated the results of my observations to the British Association at Newcastle.§ In every specimen in which the colour in question exists, I found it to arise from *internal*, and not from *superficial* reflexion. In an extensive series of experiments on the absorption of light by the aqueous and alcoholic solutions of the colouring matter of plants, I found this property of internal dispersion in thirty or forty of these solutions. The most remarkable of these was the alcoholic solution of the colouring matter of the leaves of the common laurel. At first its colour is a bright green, afterwards changing into a fine olive colour; but in all its stages it disperses light of a *brilliant blood red colour*, which forms a striking contrast with the transmitted tint. After a long exposure to light, the transmitted tint almost wholly disappears, while the dispersed light retains its red colour.|| Another

* *Traité de Mineralogie*, tom. i., p. 512, 521.

† *Philosophical Transactions*, 1818, p. 424.

‡ *Treatise on Light*, art. 1076.

§ See Report of the Eighth Meeting, and Trans. of Sections, p. 10-12.

|| I shewed this experiment in 1836, at Lacock Abbey, to Mr FOX TALBOT, and several members of the British Association. At the meeting of the British Association at Manchester, in 1842, a friend handed to me, in the sectional meeting, a “solution of stramonium in ether,” which

very remarkable example of internal dispersion, pointed out to me by Mr SCHUNCK, is exhibited in an alkaline, or in an alcoholic solution of a resinous powder produced from *orcine* by contact with the oxygen of the air. Its colour by transmitted light is reddish brown, and the light which it disperses is of an exceedingly rich *green* colour.

Since these experiments were made, my attention has been called to two interesting papers by Sir JOHN HERSCHEL, in the last part of the Philosophical Transactions; the one *on a case of superficial colour presented by a homogeneous liquid internally colourless*, and the other *on the epipolic (or superficial) dispersion of light*; and as these papers contain results incompatible with those which I had previously published, I found it necessary to resume the investigation of the subject.

The two papers now referred to are chiefly occupied with a description of the phenomena of coloured dispersion, as exhibited in a diluted solution of *sulphate of quinine* in weak sulphuric acid. Owing to the solution being nearly colourless by transmitted light, the general phenomenon is very beautiful. The line of bright blue light dispersed by the stratum of fluid immediately beneath the surface of incidence, and about the 50th of an inch thick, *appears* to be confined to that stratum, and it is in this respect only that the phenomenon differs from that which is exhibited by fluor-spar and the vegetable solutions which I have mentioned.

1. *On the Internal Dispersion of Fluor-Spar.*

There are many varieties of fluor-spar in which no dispersion of the intromitted light takes place. It does not exist in the *yellow*, *red*, and bright *blue* varieties which I have examined. It occurs chiefly in the *green* fluor from Alston Moor, and in several *pink*, and *bluish-yellow* varieties from Derbyshire. In order to observe the phenomena of dispersion most distinctly, I transmit a condensed beam of the sun's light through the specimen, when partially covered with black wax or black velvet. In some specimens, the intromitted beam is partially dispersed in a fine blue tint from every part of the solid which it traverses; but in other specimens, which are composed of strata of different colours, parallel to the faces of the cube, a very different and a very instructive phenomenon is displayed. The intromitted beam A B C, Fig. 1, Plate V., is crossed with bands of dispersed light of different colours, and of different intensities. In one case, a *pink* light was dispersed from the stratum close to the surface of incidence; from the next stra-

dispersed a *bright green* light. I described the phenomenon to the meeting, and it is noticed in the Transactions of the Sections, p. 14. Upon making the solution myself, I cannot obtain the same tints, either from the stalk or the dried leaves of the plant. The solution of the leaves disperses a brilliant red tint, like that mentioned in the text. The solution put into my hands must, therefore, have been one of the seeds of stramonium, or of some other substance possessing internal dispersion in a high degree.

tum there was *no dispersion* at all; this was followed by a narrow stratum, which dispersed a *bright whitish light*; then succeeded a stratum of non-dispersing fluor, and alternately dispersing and non-dispersing strata, scattering the fine blue light which has already been mentioned.

These results, which I have shewn to different persons, are incompatible with those obtained by Sir JOHN HERSCHEL with the very same variety of *fluor-spar*. He regards the blue dispersed light as *strictly* an *epipolic* or *superficial* tint,—so superficial, indeed, “that it might be referred to a peculiar texture of the surface, the result of crystallization, were it not that it appears equally on a surface artificially cut and polished.”* Were I to hazard a conjecture respecting the cause of this difference in our results, I would ascribe it to the different degrees of light in which the observations were made. While I used a condensed beam of the sun’s light, Sir JOHN HERSCHEL seems to have employed chiefly the ordinary light of day. In studying the phenomena in the solution of quinine, he “exposed it to strong day-light or sunshine;” and in another experiment, which pre-eminently required a powerful illumination, he “directed a sunbeam downwards on the surface, by total reflection from the base of a prism,” which was in reality inferior to the ordinary sun’s light. In the case of *fluor-spar*, however, he states that the epipolic colour is seen in perfection when “exposed to daylight at a window.” In such a feeble light I could not have seen the phenomena I have described, and it is owing chiefly to the intensity of the light which I employed, that I have been enabled to place it beyond a doubt that the blue light dispersed by *fluor-spar* is reflected from every part of the interior of the crystal, and is not produced by any action either strictly or partially superficial, or solely by any stratum near the surface.

Sir JOHN HERSCHEL mentions, that the green *fluor-spar* of Alston Moor is the only solid in which he has observed an epipolic tint. It is the only mineral in which I have found an internal dispersion, excepting, of course, the minerals which exhibit the analogous phenomena of opalescence and chatoyance; but I have found several glasses which possess it, one in particular of a *yellow* colour, which disperses a *brilliant green* light, and another of a *bright pink* colour, which also disperses a *green* light, and a third of an *orange* colour, which disperses rays of a *whitish green* colour. In these cases, the glass has a decided colour of its own; but I have found many specimens, both of colourless plate and colourless flint glass, which disperse a beautiful green light.

2. On the Internal Dispersion of the Solution of Sulphate of Quinine.

Sir JOHN HERSCHEL describes the epipolic dispersion of this solution as “occupying a very narrow parallelogram, having a breadth of about a 50th of

* Philosophical Transactions, 1845, p. 143.

an inch, of a vivid and nearly uniform blue colour over its whole breadth;"* but upon "directing a sunbeam downwards on the surface, by total reflection from the base of a prism, a feeble blue gleam was observed to extend downwards below this vivid line to nearly half an inch from the surface, thus leaving it doubtful whether some small amount of dispersion may not be effected in the interior of the medium at appreciable depths." By using condensed solar light, this doubt is immediately removed, and the phenomenon ranks itself as one of internal dispersion, differing only in the law of its intensity from those which I have already described. In the one the dispersible rays are thrown *gradually*, in the other *quickly*, from the intromitted beam,—a phenomenon to a great extent identical with what takes place in the analogous phenomena of absorption.

If the dispersing action of the solution were rigorously confined to a stratum the fiftieth of an inch thick, it would have followed, of necessity, that "*an epipolized beam of light* (meaning thereby, a beam which has been once transmitted through a quiniferous solution, and undergone its dispersing action) *is incapable of further undergoing epipolic dispersion*;" but as the dispersing action is not thus limited, that conclusion must be incorrect. Sir JOHN HERSCHEL, indeed, has deduced this result from direct experiment with a plate of glass immersed vertically in a quiniferous solution. In this case he could perceive no trace of colour either at the ingress or egress of the epipolized beam which was incident upon the plate. Sir JOHN does not mention the distance of the plate from the epipolising stratum. If the distance was small, we are confident, from direct experiment, that the blue tint would have been seen; but if the distance was considerable, then the beam, incident upon the glass, must have been previously shorn of all its dispersible rays.

In examining the blue rays themselves, Sir JOHN found that they consisted of a "small per-centage of rays, extending over a great range of refrangibility." They formed, however, a continuous spectrum deprived of the less refrangible red, nearly of the whole orange, and all the yellow; a rich and broad band of fine green light, slightly fringed with red, passed into a copious indigo and violet without the intermediate blue.

The comparatively feeble light of the dispersed blue rays renders it difficult to ascertain their susceptibility of being a second time dispersed. Sir JOHN HERSCHEL could not obtain any indication of this susceptibility; but we have no doubt that with condensed light their second dispersion will be discovered: and we are led to this opinion by the fact, that Sir JOHN believed that the epipolic dispersion takes place in all directions, and therefore expected to discover a second dispersion under circumstances in which, according to my experiments, it could not be found.

* The best method of seeing this experiment, is to take the solution into the open air, where the whole light of a blue sky can fall upon its surface. I have in this way seen the blue line perfectly luminous at that stage of a December twilight when there was not light enough to read by. I consider, therefore, the light of the sky as peculiarly susceptible of this species of dispersion.



Fig. 1.

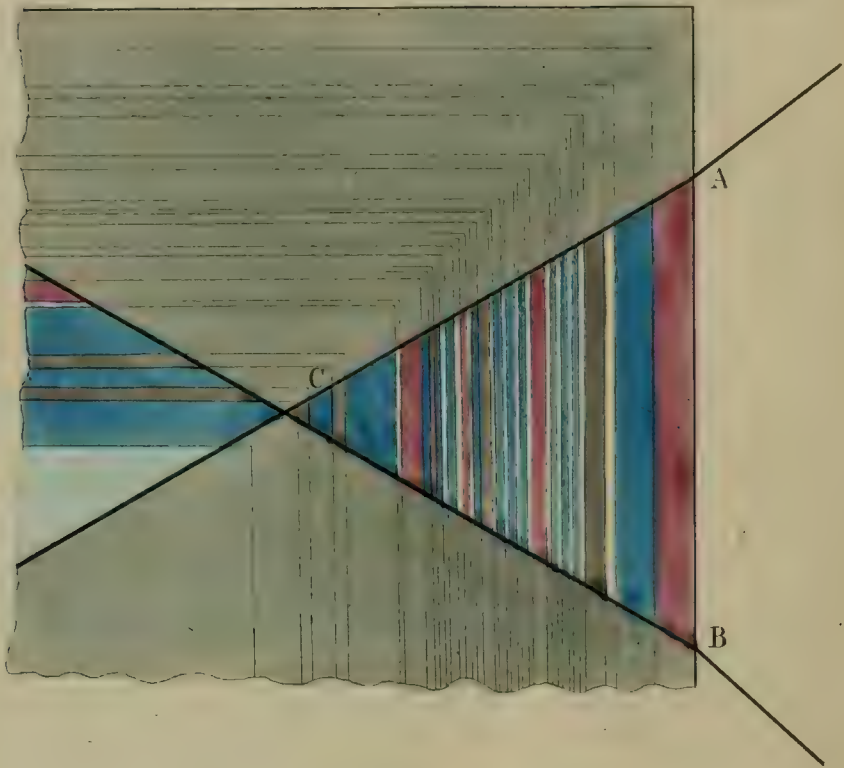
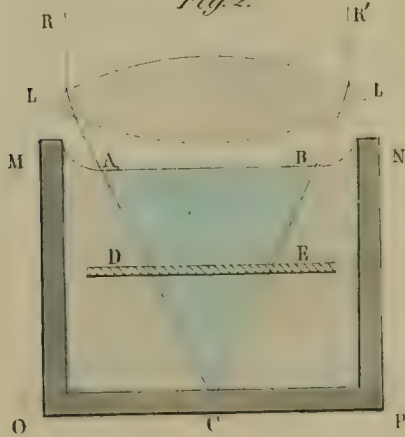


Fig. 2.



Sir JOHN has clearly shewn, that the light is dispersed outwards as well as laterally; but as he was conversant only with the phenomena of a narrow blue line, and had not seen the blue cone of rays dispersed from the cone of condensed light, he could not be aware of the changes which take place in its colour while the eye passes from the azimuth of 90° to that of 100° .

These changes are very decided, and will be understood from fig. 2 (Plate V.), in which M N O P is a horizontal section of the vessel containing the solution; R R' a beam of solar light, incident upon an achromatic lens L L, and condensed into the luminous cone A C B. Now, the blue colour produced by the first stratum, next to the side A B, is exceedingly strong, and that which occupies the rest of the cone A C B comparatively faint. When we view the bright blue stratum in the direction N M, or in the azimuth of 90° , the tint is very brilliant, because the eye receives all the blue rays dispersed by the whole length A B of the stratum; whereas, when we view it in the direction R' C, in the azimuth of 0° , we only see the tint corresponding to the thickness of the stratum. The tint, however, is, in reality, a maximum in the azimuth of 0° , and gradually diminishes till it ceases in the azimuth of 180° , or in the direction C R'.

If we now immerse in the fluid a plate of colourless glass, whose section is D E, so as to receive the beam A B E D, we shall find that there is no peculiar dispersion, as Sir JOHN HERSCHEL observed, either at its surface of incidence or emergence. Hence he concluded that the epipolised beam A B E D "is incapable of undergoing farther epipolic dispersion;" and that having thus "lost a property which it originally possessed, it could not, therefore, be considered *qualitatively* as the same light."

Now, in using a condensed beam of light, as we have done, we find that the whole cone A B C, even when *two inches* long, and with a December sun, disperses the blue light, and the stratum behind the glass plate D E nearly as much as the stratum before it. In fluor-spar, and in the other fluids I have mentioned, this is still more strikingly the case,* and hence neither of the conclusions drawn by Sir JOHN HERSCHEL are admissible.

The following appear to me to be the deductions which the experiments actually authorize:—

1. A beam of light which has suffered dispersion by the action of a solid or fluid body, (that is an *epipolised* beam) is capable of further undergoing epipolic

* In one of these experiments a piece of green fluor, from Alston Moor, when immersed in the quiniferous solution, dispersed a fine *violet blue* light, at the distance of *three-fourths* of an inch from its surface. In another experiment, a beam of light that had been dispersed in the solution of quinine, again suffered dispersion at *two inches* distance from the surface of a piece of Derbyshire fluor.

A beam of light that has passed through the Esculine solution disperses blue light, but not copiously, when transmitted through the quinine solution; but the beam that has passed through quinine is copiously dispersed when transmitted through Esculine.

dispersion, provided the thickness of the medium is not so great as to have dispersed all the dispersible rays.

2. When such a medium is thus rendered incapable of dispersing more light, it is not because it has lost a property which it originally possessed, but because it is deprived of all the dispersible rays which it contained.

It is no doubt an interesting fact, that a small number of differently coloured rays, constituting blue light by their mixture, should possess this property of being dispersed, while other rays of the same refrangibility are either less dispersible, or apparently indispersible, by the same medium; but the fact will appear less surprising and anomalous when we advert to certain phenomena of absorption in which the same property is displayed.

The difference between the *absorption* and the *internal dispersion* of light is simply this. In the one case the portion of light withdrawn from the intromitted beam is *extinguished* and *invisible*, and in the other *dispersed* and *visible*; and we may compare the two classes of phenomena by *supposing* that the light extinguished by absorption is rendered visible as if by dispersion. Now it is a remarkable fact, that almost the whole of the blue light absorbed by the mineral called *native orpiment* is extinguished during the passage of the light through the first stratum, whose thickness is less than the fiftieth of an inch; and hence it is that the thinnest slice of this substance has nearly as deep a yellow colour as the thickest. Were the absorbed blue rays to become visible by dispersion, we should actually see a more striking example of epipolism, or dispersion confined to the first stratum, than in the quiniiferous solution. Even the condensation of the beam would not in this case give us a blue cone of light.

The analysis of the blue line indeed would indicate a difference between the two phenomena. It would shew that the blue light was derived chiefly from the *violet*, *indigo*, and *blue* spaces, and but partially from the *green*, *yellow*, *orange*, and *red*, having appropriated the whole of the more refrangible rays, and but a very small portion of the less refrangible ones; whereas the blue light from the quiniiferous solution is derived almost in equal proportions from all the coloured spaces excepting the least refrangible, red. The limitation of the rays capable of absorption, like the limitation of the dispersible rays in the quiniiferous solution, is shewn in the action of various bodies on the spectrum. Such bodies change the colour of certain spaces in the spectrum, without continuing to absorb the residual rays; that is, when the absorbable rays are removed by a certain thickness of the body, an additional thickness operates very feebly, as in the quiniiferous solution, in altering the colour of the residual beam.

I have pointed out these analogies between the phenomena of absorption and dispersion to meet the case of the bright blue line in the quiniiferous solutions. The dispersion of fluor-spar, and of the glasses and vegetable solutions already described, is of a different character. In fluor-spar the dispersion effected

by the first stratum is by no means very abundant, and the intromitted beam, even after passing through one or more undispersing strata, is dispersed nearly as copiously as before. In the glasses and in the vegetable solutions there are no peculiarities which require explanation, excepting those which arise from the absorption of the dispersed beam in passing through the coloured medium.

When the phenomena of internal dispersion are exhibited in coloured fluids and solids, the influence of absorption upon the dispersed light is very interesting. Previous to its dispersion the light has the same colour as the transmitted light, were it to emerge at that point of its path, and when viewed at an azimuth above 90° , a portion of the dispersed light has that colour. The quantity of light possessing this colour increases between the azimuth of 90° and 180° . In order to see this effect disembarassed from another influence, we must make the intromitted beam parallel to the surface of the fluid or solid, so as just to graze it. In this way the dispersed light is not changed in its passage to the eye after dispersion. When the beam passes through the coloured medium without this precaution, it again suffers absorption proportional to the thickness of the coloured substance through which it has passed, and sometimes disappears altogether. This effect is finely seen in the darker solutions, which disperse a brilliant *red*, or a brilliant *green* light; the colour of the former becoming *yellowish green* and *whitish*, while that of the latter becomes *whitish yellow*.

3. On the Polarisation of Dispersed Light.

As the dispersed light is turned from its path by reflection, and is reflected at angles proper for polarising it, its partial polarisation at least might have been anticipated. Sir JOHN HERSCHEL viewed it through a tourmaline, and states that no signs of polarisation were perceived in it; but his method of obtaining the blue line from light diverging from a large area of the sky, and therefore reflected at various angles far above and far below the polarising angle, rendered it impracticable to detect its state of polarisation. The method which I adopted, of using a narrow cylindrical beam of strong light, affording a bright dispersed beam more than an inch in length, enabled me to discover its polarisation, and to investigate its peculiarities.

Upon examining the blue beam in the quiniferous solution with an analysing rhomb of calcareous spar, I found that a considerable part of it, consisting chiefly of the less refrangible portion of its rays, was polarised in the plane of reflection, while the more refrangible of its rays, constituting an intensely blue beam, had a different state of polarisation.

This insulation of the bluer rays greatly increased the beauty of the phenomenon, and promised to throw some light upon its cause. I was therefore anxious to ascertain their state of polarisation, which was not indicated by the analysing rhomb. With this view I transmitted through the solution a strong beam of polarised

light, and was surprised to find that the blue beam which it yielded by dispersion, retained the same intensity in every position of the analysing prism, and therefore possesses a *quaquaversus* polarisation, such as that which light receives when transmitted through a congeries of minute doubly refracting crystals having their axes in all possible directions.

In making the same experiment with other dispersing fluids and solids, I found some in which the whole beam was completely polarised in the plane of reflection, and others in which it exhibited solely a *quaquaversus* polarisation; but as these experiments indicate new processes in the decomposition and polarisation of light, which require a more extended analysis, I shall resume the subject in a separate communication, contenting myself at present with a general account of the more important facts, and the results to which they lead.

Having transmitted a condensed beam of light through an alcoholic solution of the leaves of the Common Laurel, or of Tea, either green or black, I found that the *bright red beam* which it dispersed, possessed, like the *blue* one in the quiniferous solution, a *quaquaversus* polarisation, a small portion of the light being polarised in the plane of reflection. The *green* beam dispersed by the preparation of *orcine*, has the same properties, the white portion of it disappearing and reappearing during the revolution of the analysing rhomb. In the aqueous solution of *esculine*,* the dispersed pencil consists of two finely-contrasted pencils, the one *whitish* and polarised in the plane of reflection, and the other a *very deep blue*, having *quaquaversus* polarisation. The *white* pencil is more intense than the *blue* one, which is the very reverse of what takes place in the solution of quinine. The alcoholic solution of the seeds of the *Colchicum autumnale* gives a bright and copious *green* beam of dispersed light, which consists of two pencils, one whitish and polarised in the plane of reflection, and the other bright green, with a *quaquaversus* polarisation. The same property is possessed by a solution of *guiacum* in alcohol, which disperses, by the stratum chiefly near its surface, a beautiful *violet* light; and also by an alcoholic solution of *sulphate* of *strychnine*, which disperses a green light, *after it has stood for some days*. The same property is possessed by almost all the oils, in some of which the dispersed light is exceedingly beautiful, varying from a pale green to a blue tint.

The polarisation of the dispersed beam in one plane, namely, in the plane of reflection, is exhibited in several fluids and solids. It is very marked in the bile of the ox, which disperses an olive-green light; in a solution of gum-myrrh in alcohol, diluted with water, which disperses a bright white beam; and in an orange-coloured glass, which disperses a pale greenish beam.

In many fluid solutions, the beam with a *quaquaversus* polarisation is very intense, when compared with the faint pencil which is polarised in the plane of

* In the alcoholic solution of Esculine, the *faint-blue* approaches to *violet*. The polarisation is like that in quinine.

reflection; but in a specimen of *yellow Bohemian glass*, which gives a copious and brilliant *green* beam by dispersion, the whole of the beam possesses a *quaquaver-sus* polarisation.

When we view the dispersed beam in different azimuths, some very interesting phenomena present themselves to our notice. In general, the colour of the dispersed light suffers a considerable change, passing, between the azimuths of 90° and 180° , from the colour of the dispersed beam to the colour of the transmitted beam. This effect is finely seen in the alcoholic solution of tea, where the brilliant *red* light passes into an *olive* tint; but it is still more remarkable in a mixture of *Prussian blue* and water. The dispersed beam is polarised in the plane of reflection. It is *bluish* in the azimuth of 90° ; *pinkish* about the azimuth of 100° ; *greenish* in that of 120° ; *bluish* in azimuth 150° ; and again *pinkish* in azimuth 170° . These three last tints may be all seen at the same time.

Such are the general phenomena of internal dispersion, a subject which promises to throw some light on the constitution of those solid and fluid bodies by which it is produced. The *apparently superficial dispersion* in the quinine solution to which Sir JOHN HERSCHEL has given the name of *epipolism*, is obviously a single case of the general phenomenon in which the ordinates of the curve of dispersion diminish rapidly after the light has entered the stratum nearest the surface; while the *real epipolism* which he ascribes to fluor-spar, so far from being an action of the surface, is much less so than that of the quiniferous solution, and entirely similar in its character to that which is produced by the fluids and solids which I have examined.

The phenomenon of internal dispersion, when considered merely as a case of reflection and polarisation, possesses much novelty and interest. If the exciting beam, as we may call it, is cylindrical, we have before us an experiment, in which the phenomena of *cylindrical reflection*, and *cylindrical polarisation*, are at once exhibited to us. The innumerable reflecting surfaces, receiving the introritted beam at all possible angles, reflect the incident light in all possible directions, so that the eye, wherever it is placed, sees the beam as if it were self-luminous; and while the eye is made to revolve in a circle round the cylindrical beam, it receives a pencil of polarised light—polarised in a plane passing through the eye and the axis of the cylinder; or, what is the same thing, a thousand spectators viewing this beam in the same azimuth, but in directions differently inclined to the horizon, would all see exactly the same phenomena of reflection and polarisation!

4. *On the Causes of the Internal Decomposition and Dispersion of Light.*

In imperfectly crystallized minerals, such as particular specimens of *adularia*, *chrysoberyl*, *opal*, and *sapphire*, the white and coloured opalescence, and the astitial radiations, have been shewn to arise from minute vacuities, or from open spaces with crystallized sides, or from narrow pipes, or linear spaces parallel to

the edges of the primitive or secondary forms of the mineral. In tabasheer, where the vacuities contain air, which we can expel and send back at pleasure, a fine blue light is dispersed, depending, no doubt, on the size of the vacuities. In a very remarkable specimen of calcareous spar, crowded with hemitrope veins, I have observed a copious internal dispersion produced by the reflection of light at the different surfaces, which, though in optical contact, have different degrees of extraordinary refraction.

All these phenomena, however, are essentially different from those which form the subject of this paper, with the exception of the phenomena of fluor-spar, in so far, at least, as they are the result of imperfect crystallization. The *epipolism* which Sir JOHN HERSCHEL ascribes to this mineral, or its *internal dispersion*, according to my experiments, does not belong to the species, but only to particular varieties, and not even to the variety, but merely to particular parts of it. It is therefore the result of inequal or imperfect crystallization. The nucleus is perfect, a coating supervenes, having a different tint by transmitted light, and dispersing a fine blue light, and so on through a succession of strata, dispersing differently coloured lights, and separated by non-dispersing spaces. An extraneous element, therefore, depending on the state of the solution, has been successively introduced into the crystal, and if it had the same refractive and dispersive power as the fluor-spar, it could not reflect any portion of the intromitted beam: But if there is any difference in the mean refraction, or in the dispersive power, or if the difference consists merely in the unequal length of certain portions of the two spectra, then, in all these cases, light will be dispersed by the extraneous element. If, for example, we place a film of oil of cassia between two prisms of flint glass, the light reflected from the film will be *blue*. The index of refraction for certain of the *red* rays is the same in the glass and in the oil, and consequently none of these rays enter into the reflected pencil, which must therefore be *blue*, whatever be the inclination of the incident rays. If we now suppose this film of oil to be solidified, and disseminated in infinitely small atoms through flint glass, or a fluid that has the same action as the glass upon light, we should have the phenomenon of a blue dispersion.*

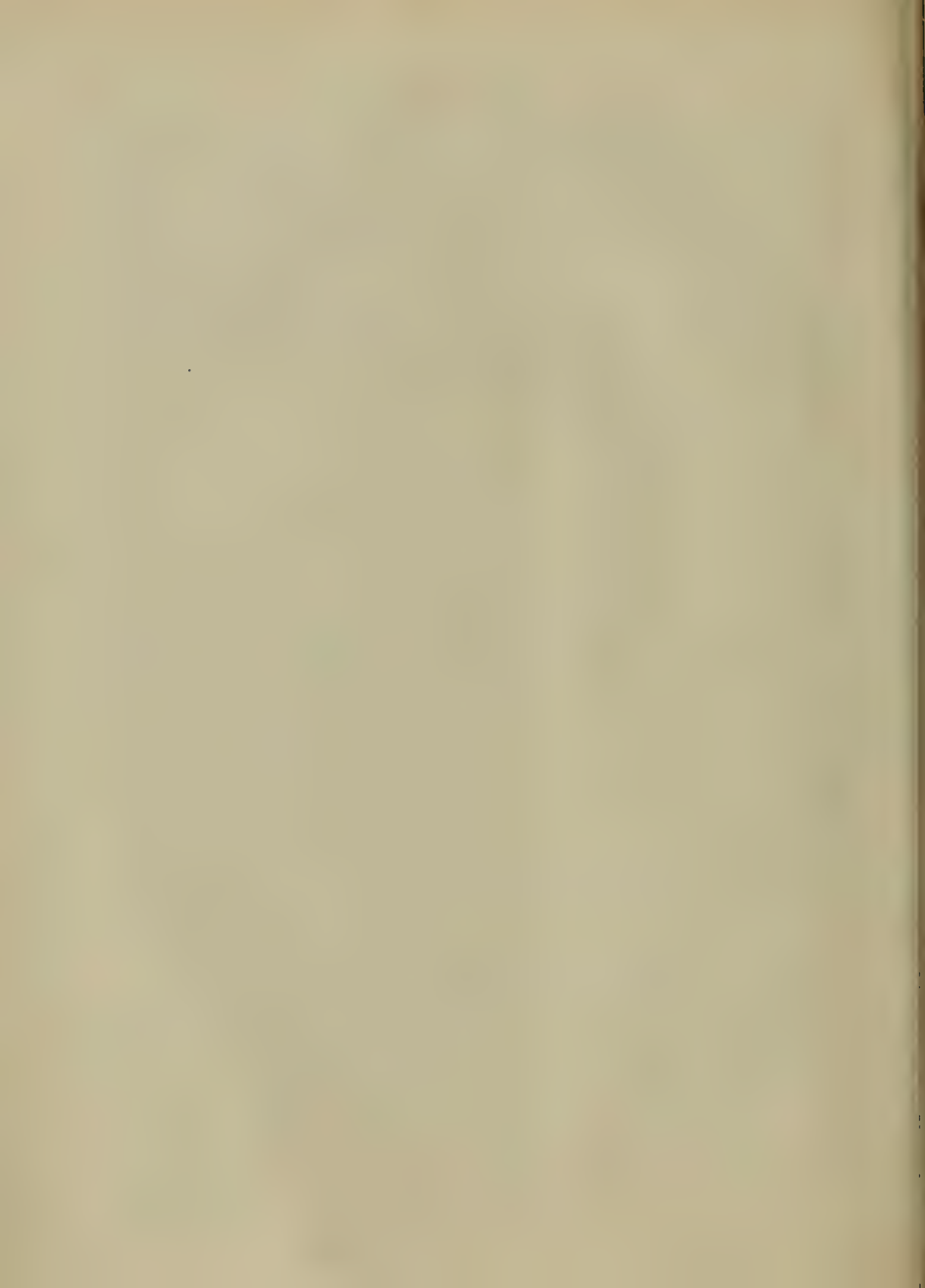
A beam of blue light thus produced should be polarised at the polarising angle, and partially polarised at other angles; and if this is not its character, we must look for some cause by which it has been counteracted. We have already seen that, in the Bohemian yellow glass, none of the light is polarised by reflection, and that in the quinquiferous solution only a part of it is so polarised, the whole pencil in the one case, and the residual pencil in the other, having a *quaquaversus* polarisation. This effect cannot be the result of an opposite polarisation

* In the experiment with *Prussian blue*, which is a very splendid one, the particles are mechanically suspended in the water; so that we have here an ocular demonstration that the particles are the cause of the dispersion and the *quaquaversus* polarisation.

by the refraction of the dispersed light at the surfaces of the reflecting particles, because such an action would only reduce the amount of polarisation by reflection ; and I have found by direct experiment, namely, by making the blue light pass through different thicknesses of the fluid, that such an effect is not produced. Unless, therefore, we suppose that this *quaquaversus* polarisation is a new property of light, produced by a peculiar action of certain solid and fluid bodies, we are driven to the conclusion, no less remarkable, that it is produced by an infinite number of doubly refracting crystals, having their axes of double refraction lying in every possible direction, and therefore reflecting from their posterior surfaces a pencil of light with *quaquaversus* polarisation.

ST LEONARD'S COLLEGE, ST ANDREWS,

January 30. 1846.



XIV.—*On the Constitution and Properties of Picoline, a new Organic Base from Coal-Tar.* By THOMAS ANDERSON, M.D.

(Read, 20th April 1846.)

THE careful study of the products of destructive distillation has enriched organic chemistry with an extensive series of results of unexpected interest and importance. These results have affected, in no inconsiderable degree, the recent progress of the science; and their influence has been of a twofold character, both general and particular, exerted in the former case in the development of some of the more remarkable general doctrines of organic chemistry; in the latter, in the important light thrown by their investigation on the constitution of the substances from which they are derived, and the facilities they have afforded of following out connections, which the examination of the original substance either does not at all present to our view, or, at least, indicates only in an imperfect or dubious manner. Added to this, we have the remarkable fact of the appearance among these products of substances in some cases identical with those occurring in organised beings; and in others, presenting analogies of the very closest character with the actual products of vital affinity, which, taken together, afford abundant reason for pursuing the investigation of substances which have already afforded results of so remarkable a character.

Setting aside altogether those substances, the occurrence of which is so frequent, that they may be called the general products of destructive distillation, such as carbonic acid, light carburetted hydrogen, olefiant gas, acetic acid, &c., it may be laid down as a general rule, that each individual compound produced during such a process, is formed by the destruction of a limited number of substances only, which bear to each other, and to the product, a more or less intimate connection in constitution or chemical relations. In those instances in which we have been enabled to submit to destructive distillation substances of a definite and simple constitution, in a state of chemical purity, and where an uniform temperature has been preserved, the results have been, for the most part, of an exceedingly simple and intelligible character; but in proportion as the atom becomes more complex, so also do the products of its decomposition, and the explanation of the results is found to be proportionately difficult and uncertain. These difficulties and uncertainties are increased in a still higher degree, in the case of a substance such as coal, where we have to deal not merely with one complex atom but with a congeries of several such, and where the process is performed on the large scale, and under a variety of perturbing influences. The distillation of coal is, in fact,

attended by the formation of about twenty different substances, the constitution and properties of which have been examined with different degrees of accuracy, and which present among them instances of almost every species of chemical compound. The discovery of six of these substances is due to RUNGE,* who published, about fourteen years ago, a very interesting memoir, containing an account of their general properties. Of these substances, three are possessed of acid properties, and three are bases, to the latter of which he gave the names of Kyanol, Leukol, and Pyrrol, from the peculiar colours developed by the action of certain reagents on their salts. The two former of these substances were afterwards submitted to a detailed examination and analysis by HOFFMAN,† who arrived at the interesting result, that both are identical with substances which had been independently obtained by the decomposition of certain well known bodies; Kyanol possessing the constitution and properties of the Aniline of FRITSCHÉ, and the Benzidam of ZININ; while Leukol is identical with the substance described by GERHARDT under the name of Chinoline, and which was obtained by him as a product of the distillation of quinine, cinchonine, and strychnia, with caustic potass. HOFFMAN failed, however, entirely in obtaining any evidence of the presence of pyrrol in the substance which he examined, and leaves in doubt the existence of such a compound.

Having lately had occasion to examine a quantity of the mixed bases contained in coal-tar, obtained by a method similar to that of RUNGE, but which, owing to a modification of the process, contained all the more volatile bases formed during the distillation of coal, I was led to try whether or not pyrrol was to be found in it, and I obtained immediate evidence of its existence, by the characteristic red colour which it gives to fir-wood moistened with hydrochloric acid. The attempt to separate this pyrrol proved that it was present in extremely minute quantity only, but led to the discovery of a new base different from those of RUNGE, for which I propose the name of Picoline, and the examination of whose properties forms the subject of the present paper.

Preparation of Picoline.

For the crude substance employed in the preparation of picoline, I am indebted to the kindness of Mr ASTLEY, of the Bonnington Chemical Works, and it was obtained by the following modification of RUNGE's process. In the preparation of naphtha from coal-tar, the first product of distillation is agitated with sulphuric acid for the purpose of separating any naphthaline which may be present, as well as a variety of substances in extremely minute quantity, which communicate to the crude naphtha the property of becoming dark-coloured

* Poggendorf's Annalen, Band 31, u. 32.

† Annalen der Chemie und Pharmacie, vol. xlvii..

by exposure to the air; among these substances, of course, are all the basic compounds contained in the oil. The sulphuric acid which had been used for this purpose was neutralised by impure ammonia obtained by a single distillation of the watery fluid of the gas-works. On the addition of the ammonia there was no separation of any oil in quantity appreciable to the eye; but upon distillation, the bases, which had been dissolved in the fluid, passed over with the first portions of water, and collected in a separate layer in the receiver. This oil, when it came into my hands, possessed a very dark brown colour, a somewhat viscid consistence, and a peculiar pungent and disagreeable odour. It was heavier than water, a layer of which, containing a small proportion of oil in solution, floated on the surface. The examination of this oil proved it to consist, in addition to picoline, of a mixture of pyrrol, aniline, an oily base possessing the general properties of leukol, and a thick heavy oil destitute of basic properties.

In order to separate picoline, the oil, along with the water which floated on its surface, was introduced into a retort and carefully distilled. At first, water, accompanied by a little oil, passed over, and then an oil by itself, which dissolved completely in the watery fluid contained in the receiver. As the distillation proceeded, another oil made its appearance, which collected in a layer on the surface of the fluid which had previously distilled. When about three-fourths of the oil had passed over, the process was stopped, by which means the oil, destitute of basic properties, which requires a very high temperature for its distillation, was left behind in the retort. The fluid in the receiver was now supersaturated with sulphuric acid diluted with water, care being taken to obtain a powerfully acid reaction. The peculiar odour which the fluid possessed, was by this process entirely changed, but not destroyed; and, on distillation, the water which passed over, carried with it all the pyrrol contained in the solution, while the other bases were retained by the sulphuric acid. Caustic potass was then added to the residue in the retort until an alkaline reaction was manifest, and it was again distilled; the water which passed over carried with it the oily bases, partly dissolved, partly floating on the surface of the solution, exactly as in the first distillation. A few sticks of fused potass were introduced into the product, and the whole was left in repose; as the potass dissolved, the oil, which is entirely insoluble in solutions of the fixed alkalis, rose to the surface and there collected in the form of a pale yellow layer, still containing a considerable quantity of water, which may amount to 30 or 40 per cent. of the bulk of the oil. The oil was separated from the watery fluid by means of a pipette and pieces of fused potass added so long as they continued to become moist. The dry oil was then introduced into a retort and distilled. A transparent and colourless oil passed over, which was tested at intervals by allowing a drop of it to fall into a solution of chloride of lime. So soon as the reaction of aniline made its appearance the receiver was changed. The first portion was now picoline in a state approaching to purity; that which

immediately followed, consisted of a mixture of picoline and aniline. The first portion was again digested with fused potass and rectified; that which distilled at 272° was collected apart, and constituted pure picoline.

Constitution of Picoline.

The general analogy in properties which picoline bears to aniline and the other oleaginous bases, permitted the assumption that it, like these substances, was free from oxygen; I proceeded, therefore, in its analysis, upon this hypothesis, and neglected the determination of the nitrogen. The following are the results of the analyses:—

Analysis I.	{	5.630 grains of picoline gave
		15.954 ... carbonic acid,
		3.944 ... water.
... II.	{	5.347 grains of picoline gave
		15.100 ... carbonic acid,
		3.670 ... water.

Which give the following results per cent.:—

	I.	II.
Carbon . .	77.16	77.18
Hydrogen . .	7.77	7.62
Nitrogen . .	15.07	15.20
	<hr/> 100.00	<hr/> 100.00

These results correspond closely with the formula $C_{12}H_7N$; the calculated result of which is—

	Theory.	Mean.
C_{12} . .	900.0	900.0
H_7 . .	87.5	87.5
N . .	177.0	177.0
	<hr/> 1164.5	<hr/> 1164.5
	100.00	100.00

This formula is precisely the same as that of aniline, along with which picoline occurs in coal-tar. In order to ascertain whether the atomic weight of these substances were also identical, I prepared the platinum salt of picoline, and determined the amount of platinum contained in it. The salt was obtained by adding bichloride of platinum to a solution of picoline in excess of hydrochloric acid: no immediate precipitation took place unless the solutions were very concentrated, but in the course of twenty-four hours the salt was deposited in fine orange-yellow needles. When dried at 212° , it gave the following results:—

I.	{	9.670 grains of chloride of platinum and picoline gave
		3.147 ... platinum = 32.544 per cent.
II.	{	10.814 grains of chloride of platinum and picoline gave
		3.517 ... platinum = 32.522 per cent.

From these analyses are deduced the following atomic weights:—

I.	II.
1211·1	1213·7

These agree sufficiently well with the theoretical atomic weight, which is 1164·5. They correspond also precisely with the results of the analysis of the aniline salts. The identity of these results is shewn by the following table of the analyses by FRITSCHÉ, ZININ, and HOFFMAN, of aniline from its different sources, and of picoline, as well as of the platinum salts of these substances:—

Aniline.*	Benzidam.*	Cyanol.	Picoline.	Theory.
C = 77·73	77·32	76·67	77·17	77·29
H = 7·60	7·50	7·72	7·69	7·43
N = 14·98	14·84	15·62	15·14	15·28
<hr/> 100·31	<hr/> 99·66	<hr/> 100·00	<hr/> 100·00	<hr/> 100·00

The following are the results for the platinum salts:—

	Benzidam.	Kyanol.	Picoline.	Theory.
Mean platinum, per cent.	32·501	32·886	32·533	32·94
Atomic weight	1216·1	1170·5	1212·4	1164·5

The results of all these analyses agree perfectly with one another; but the properties possessed by picoline differ from those of aniline, which, whether obtained from coal-tar, indigo, or nitrobenzid, presents a perfect identity in its chemical characters.

Properties of Picoline.

Picoline is a perfectly colourless, transparent, limpid fluid, extremely mobile, and destitute of viscosity. It possesses a powerful, penetrating, and somewhat aromatic smell, which, when very dilute, is replaced by a peculiar rancid odour, adhering pertinaciously to the hands and clothes. Its taste is acrid and burning when concentrated; but when very dilute, as, for instance, when its vapour is sucked into the mouth, it is powerfully bitter, as are also the solutions of its salts. It is not changed by exposure to a cold of 0°. Picoline is extremely volatile, and evaporates rapidly in the air. It boils at the temperature of 272°, and the thermometer remains perfectly stationary during the whole period of the ebullition; it is, therefore, much more volatile than aniline, which, according to HOFFMAN, boils at 359°. It may be preserved for a long time in a bottle containing only a small quantity of it, and which is frequently opened, without becoming manifestly coloured; whereas aniline becomes rapidly brown, and, indeed, cannot easily be obtained colourless, except by distillation in a current of hydrogen. The specific

* Not having the original papers of FRITSCHÉ and ZININ at hand, I extract these two results from BERZELIUS' *Arsberättelse*, 1844, p. 454, where they are calculated according to C=75·12, the rest are with C=75, but the difference is so small as not to affect the comparison.

gravity of picoline is less than that of water. I found it to be 0.955 at 50°, while, according to HOFFMAN, that of aniline is 1.020 at 68°.

Picoline mixes with water in all proportions, and forms a transparent and colourless solution. It is insoluble, however, in solution of potass, as well as in most alkaline salts, the addition of which causes its immediate separation from the water. It dissolves also readily in alcohol, ether, pyroxylic spirit, and the fixed and volatile oils. It is a powerful alkaline base: a rod dipped in hydrochloric acid, and held over it, is immediately surrounded by a copious white cloud of hydrochlorate of picoline. It restores the blue colour of reddened litmus, but does not affect the colouring matter of red cabbage. It does not coagulate the white of eggs as aniline does.

The reactions which it produces with other substances are also quite distinct from those presented by aniline. When brought in contact with the solution of chloride of lime, it does not produce, in the least degree, the violet colour which is so characteristic of aniline; on the contrary, the solution remains perfectly colourless, unless, indeed, the picoline has not been well separated from pyrrol; in which case a slight brown makes its appearance, but no violet. Picoline is also incapable of producing the yellow colour in fir wood and the pith of the elder, which is so readily obtained with aniline. When treated with chromic acid, even when very concentrated, and after boiling, no change takes place in the colour of the solution, and only a small quantity of a yellow powder is deposited; while Aniline gives an abundant precipitate, which has, according to the degree of concentration of the fluid, a green, blue, or black colour.

Picoline precipitates from solutions of chloride of copper a portion of the oxide of copper, while the remainder forms a pale blue solution, which, when evaporated to a small bulk, deposits a congeries of prismatic crystals, which seem to be a double salt. No blackening of the solution takes place, as is the case with aniline. When an excess of hydrochloric acid is present there is obtained, on evaporation, another double salt in large crystals, apparently derived from the rhombohedral system. Picoline produces also double compounds with the chlorides of mercury, platinum, gold, tin, and antimony. With chloride of gold it gives an exceedingly characteristic compound, in the form of a fine lemon-yellow precipitate, which is soluble in a considerable quantity of boiling water, and is deposited, on cooling, in delicate yellow needles. Aniline, under similar circumstances, gives a reddish-brown precipitate resembling the ferrocyanide of copper. It gives, with infusion of nut-galls, a copious curdy precipitate of a pale yellow colour, which dissolves in hot water, and is deposited again on cooling. It does not precipitate the solutions of nitrate of silver, chlorides of barium and strontium, or sulphate of magnesia.

The properties of picoline, as now detailed, are obviously different from those of aniline. They recalled, however, strongly to my mind those of a base called

Odorin, obtained by UNVERDORBEN* from Dippel's animal oil. According to this chemist, Dippel's oil, which is obtained by several successive distillations of the *oleum cornu cervi*, is a mixture of four different bases, to which he gives the names of Odorin, Animin, Olanin, and Ammolin. Of these, the two first constitute nineteen-twentieths of the whole oil, and the odorin, which resembles picoline in its solubility in water, is obtained by simply distilling the oil and collecting the product as long as it dissolves. These results, however, have been called in question by subsequent observers; REICHENBACH, especially, asserts that he was unable to separate any basic compounds, and considers the substances obtained by UNVERDORBEN to be mixtures of empyreumatic oil with ammonia. As, however, the properties which UNVERDORBEN has attributed to odorin, approximate in some respects to those of picoline, I thought it desirable to ascertain the existence of this substance, and whether or not it is identical with picoline. In order to prepare odorin, I rectified the *oleum cornu cervi*, and then distilled the product; but on allowing the first drops of oil to fall into water, they were not dissolved as UNVERDORBEN has asserted, but floated unchanged upon the surface. Finding this process unsuccessful, I agitated the crude oil with dilute sulphuric acid; the acid fluid immediately acquired a very deep reddish-brown colour, and when separated from the oil, and supersaturated with potass, a semisolid viscid mass separated from the fluid. This, when distilled with water, yielded a mixture of several oily bases, while a dark coloured resinous substance, probably UNVERDORBEN'S Fuscine, was left in the retort. The mixed bases which I thus obtained, formed an exceedingly small fraction of the oil employed. They were purified by several successive rectifications, and generally in a method similar to that employed for picoline, and the first portions of the product collected apart. It then constituted a colourless oil which became brown in the air, dissolved readily in water, and presented an odour similar to, though not quite the same as, that of picoline. It gave with chloride of gold a dirty yellow precipitate, which dissolved in hot water, and deposited, on cooling, in the pulverulent form, and with bichloride of platinum, a compound in red wart-like crystals. By an accident in the laboratory, the small quantity of this substance which I had prepared for analysis was destroyed, so that the evidence of their identity cannot be considered as sufficient. The characters of odorin, as given by UNVERDORBEN, are not perfectly identical, either with those of picoline, or the base which I obtained. Odorin, according to UNVERDORBEN, boils at about 212° , and its salts are oleaginous compounds which distil in the form of an oily fluid, whereas those of picoline are mostly crystallizable. I am at present engaged with the examination of these substances.

It is obvious, from the observations contained in HOFFMAN'S† paper, that

* Poggendorff's *Annalen*, vol. xi.

† Liebig's *Annalen*, vol. xlvii.

picoline must have been present along with aniline and chinoline in the substance which he examined. He mentions, especially, that his aniline, as obtained by distillation only, possessed a peculiar pungent and disagreeable odour, which was got rid of only by several successive crystallizations of its oxalate from alcohol, and that the impure aniline has a specific gravity less than that of water. He observes, also, that the quantity of the substance present must have been excessively minute, as it did not affect the results of the analysis, a phenomenon, the cause of which is sufficiently explained by the identity in constitution of the two substances. HOFFMAN did not obtain picoline in the separate state, simply because the bases employed by him were obtained from the less volatile portions of coal-tar, which necessarily contain it only in minute proportion.

Combinations of Picoline.

Picoline forms a series of compounds which are generally closely analogous to those of aniline, but present in a less marked degree the regularity and facility of crystallization which are so characteristic of the salts of the latter base. It forms, however, with the greater number of acids, salts which can be obtained in a crystalline form. These are all highly soluble in water, and some of them are even deliquescent; they are also for the most part readily soluble in alcohol even in the cold. They are most readily obtained by evaporating their aqueous solutions at 212° , and not by adding an acid to the ethereal solution of the base; as in the latter case the presence of even a minute proportion of water causes them to precipitate in the form of a semifluid mass. Picoline forms a number of acid salts, in which respect it differs from aniline. Its salts are less readily decomposed in the air than the corresponding aniline compounds, but they do eventually become brown, although without presenting any of the rose red colour which the latter salts assume.

Sulphate of Picoline.—I obtained this salt by supersaturating sulphuric acid with picoline. The solution obtained was perfectly colourless, and when evaporated in the water-bath, it evolved picoline in abundance, and formed a thick oily fluid, which, on cooling, concreted into a tough mass of transparent and colourless crystals, apparently of a tabular form. Exposed to the air, it deliquesces rapidly into a transparent and colourless oil, which, after a time, acquires a slight brownish colour. It is insoluble in ether, but readily in alcohol, both hot and cold. It is not deposited in crystals by allowing the hot alcoholic solution to cool. I analysed this salt by evaporating to dryness in the water-bath, in a weighed platinum crucible, and allowing it to cool under an exsiccator. It was then rapidly weighed, dissolved in water, and precipitated by chloride of barium :—

4.364 grains of sulphate of picoline gave

5.230 ... sulphate of baryta = 41.20 per cent. of anhydrous sulphuric acid.

This result corresponds with the formula $C_{12}H_7N + 2H_2O, SO_3$ as is shewn by the following calculation :—

			Theory.	Experiment.
2 Eq. Sulphuric acid	.	1000.0	41.84	41.20
1 ... Picoline	.	1164.5	48.74	...
2 ... Water	.	225.0	9.42	...
		<hr/>	<hr/>	<hr/>
		2389.5	100.00	

The sulphate of aniline dried at 212° has a different constitution, it gives 28.67 per cent. of sulphuric acid, which corresponds to the formula $C_{12}H_7N, H_2O, SO_3$.

Oxalate of Picoline.—This salt is obtained by mixing oxalic acid and picoline in excess, and evaporating the solution over quick-lime. When the solution is reduced to a very small bulk, it is deposited in the form of short prisms radiating from a centre; and on further evaporation, the whole concretes into a solid mass. The crystals evolve the odour of picoline in the air; they are highly soluble in water and alcohol, both absolute and hydrated. When heated to 212° it fuses and evolves abundance of picoline vapours, and on cooling it forms a thick fluid which slowly deposits crystals in the form of fine needles. These are probably an acid salt. I did not obtain the oxalate in a state of sufficient purity for analysis.

Nitrate of Picoline is obtained in a white crystalline mass, when a mixture of picoline and dilute nitric acid is evaporated to dryness at a moderate heat. At a higher temperature it sublimes in white feathery crystals.

Hydrochlorate of Picoline may be prepared by mixing picoline and hydrochloric acid, and evaporating on the water-bath. On cooling, the thick fluid which remains consolidates into a mass of prismatic crystals. When heated to a high temperature, it sublimes easily, and deposits itself on the sides of the vessel in transparent crystals, which deliquesce rapidly in the air.

Chloride of Platinum and Picoline.—This salt is easily obtained by adding picoline to a solution of bichloride of platinum, containing an excess of hydrochloric acid; it deposits itself immediately, if the solution be concentrated, but when moderately diluted, it makes its appearance only after the lapse of some time. The crystals which are deposited are rather liable to retain an excess of picoline, which renders it advisable to redissolve them in a dilute solution of chloride of platinum with a little hydrochloric acid. From this solution it is deposited pure, on cooling, in the form of fine orange-yellow needles, which can easily be obtained half an inch long even when operating on very small quantities. It is much more soluble both in water and alcohol than the aniline salt, and indeed than the platinum salts of the organic bases generally. It requires only about four times its weight of boiling water for solution.

The crystals of this salt, after washing with alcohol and ether, and drying at 212° , gave the following results of analysis:—

10.032 grains of chloride of platinum and picoline gave
 8.862 ... carbonic acid, and
 2.760 ... water.

The determination of the platinum, as formerly mentioned, gave in two different trials 32.544 and 32.522 per cent., the mean of which is 32.533. The analysis corresponds with the formula $C_{12}H_7N$, HCl , $PtCl_2$.

		Theory.	Experiment.
C_{12}	= 900.0	24.07	24.09
H_8	= 100.0	2.67	3.05
N	= 177.0	4.73	...
Cl_3	= 1330.4	35.59	...
Pt	= 1232.0	32.94	32.533
	<hr/>	<hr/>	<hr/>
	3739.4	100.00	

Chloride of Picoline and Mercury.—When picoline is added to a concentrated solution of bichloride of mercury, a white curdy precipitate immediately falls. If, however, the solution be dilute, it is not precipitated for some time, and then appears in the form of radiated silky needles. It is sparingly soluble in cold water, more readily in hot. It dissolves pretty abundantly in boiling alcohol, and the solution, on cooling, deposits it, sometimes in prismatic, sometimes in feathery crystals. It dissolves readily in dilute hydrochloric acid, with the formation of a peculiar compound which I have not particularly examined. Boiled with water it is decomposed, picoline being evolved, and a white powder being deposited.

In the analysis of this compound I interposed, between the combustion tube and the chloride of calcium apparatus, a small tube in which the mercury and water were condensed, and at the conclusion of the process, a current of dry air, heated to 212° , was drawn through the tube, by means of which the water was conveyed into the chloride of calcium apparatus. The salt was dried simply by exposure to the air, as it loses picoline when heated; when analysed it still smelt of picoline, which accounts for the excess of carbon obtained.

The following are the results of the analysis:—

10.962 grains chloride of mercury and picoline gave
 8.245 ... carbonic acid,
 2.168 ... water.

This corresponds to the formula $C_{12}H_7N + HgCl_2$, which gives the following results:—

		Theory.	Experiment.
C_{12}	= 900.0	19.63	20.51
H_7	= 87.5	1.90	2.19
N	= 177.0	3.86	...
Cl_2	= 887.0	19.35	...
Hg	= 2531.6	55.26	...
	<hr/>	<hr/>	<hr/>
	4583.1	100.00	

This salt differs in constitution from the aniline salt, which is represented by the formula $2 (C_{12} H_7 N) + 3 Hg Cl_2$; it tallies, however, perfectly with the compound of chinoline and bichloride of mercury, which is $C_{18} H_8 N + Hg Cl_2$.

I have not particularly examined the other compounds of picoline.

Products of the Decomposition of Picoline.

The small quantity of picoline at my disposal has hitherto prevented my examining particularly the products of its decomposition, a branch of the subject which presents numerous points of interest. Such results, however, as I have obtained, indicate a striking difference between the products afforded by it and aniline.

When treated with nitric acid of specific gravity 1.5, picoline is immediately dissolved, but without communicating to the fluid the fine indigo-blue colour which aniline produces under similar circumstances. On the application of heat there is produced an extremely slow evolution of nitrous fumes, which contrasts strikingly with the tumultuous action which aniline produces. After very long-continued treatment with nitric acid, the fluid was evaporated to a very small bulk, when it deposited large crystals in the form of rhomboidal tables. These crystals, on being treated with potass, evolved picoline unchanged. The potass solution was red, but it contained no carbazotic acid, at least no carbazotate of potass was deposited on evaporation.

An excess of bromine water added to picoline causes an immediate and abundant precipitate of a reddish colour, which, on standing during the night, deposited itself in the form of a transparent reddish oil. This substance is destitute of basic properties, and is readily soluble in alcohol and ether, but not in water. Aniline, when treated in the same manner, gives, as is well known, the bromaniloid of FRITSCHÉ, which is solid, and crystallises in silky needles, fusible at 232° . It seems probable that the oily fluid obtained from picoline may possess a constitution similar to that of bromaniloid, in which case it would have the formula $C_{12} (H_4 Br_3) N$, and would receive the name of bromopicoloid. I had not enough of it for analysis.

The action of chlorine on picoline is remarkably analogous to that which it produces on aniline. When passed into anhydrous picoline it is rapidly absorbed, and colourless crystals, apparently of hydrochlorate of picoline, are deposited. In a short time, however, the fluid becomes dark brown, and is finally converted into a resin. This resin was mixed with water, and a current of chlorine passed through it for some hours. The fluid was then introduced into a retort, and distilled, a crystalline substance, passed over along with the water, and after all the water had passed, another substance made its appearance, while a large quantity of carbon was left in the retort. The quantity in which I obtained these substances was far too small to admit of their particular examination, but it appeared

to me that the odour of the latter substance was different from that of chlorophenesic acid, which is produced by the action of chlorine on aniline.

The preceding investigation is sufficient to establish the identity, in constitution and difference, in properties of picoline and aniline. These substances are then isomeric, in the strict sense of the term, possessing the same composition per cent., and the same atomic weight.

Although isomerism has been recognised in a great variety of different classes of compounds, I believe the present to be the first instance in which it has been satisfactorily proved among organic bases. Two instances, indeed, have been previously described, but in neither can the evidence be considered absolutely conclusive. One of these cases is that of two bases discovered by PELLETIER and COUERBE* in the husks of the *Cocculus Indicus*, to which they have given the names of Menispermin and Paramenispermin. The characters which they have assigned to these substances are sufficiently distinct, but their analyses of both lead to the formula $C_{18}H_{12}NO_2$. This result, however, is unsupported by any determination of their atomic weights, without which the isomerism cannot be admitted as proved. The other instance is that of bebeerine, which, according to the analysis of Dr D. MACLAGAN,† is isomeric with morphia, both being represented by the formula $C_{35}H_{20}NO_6$; and as this result is supported by the analysis of the platinum compound, the probability of their isomerism is much higher than in the former case. Unfortunately, however, another source of fallacy enters into the question in the amorphous condition of bebeerine, which renders it impossible to determine with certainty its freedom from impurity; even the constitution of morphia, by far the most definite of the two substances, can scarcely be considered as fixed, GERHARDT, for instance, representing it by the formula $C_{36}H_{19}NO_6$, and not by that formerly given.

With aniline and picoline, however, these uncertainties disappear. Both substances are possessed of definite boiling-points widely different from one another, and of all the other physical characters of pure substances. The lowness of their atomic weight also precludes any possibility of doubt regarding the true formula, and enables us to speak with certainty as to the identity of their constitution. The isomerism of these substances is, moreover, of much higher interest in a theoretical point of view. Menispermin and morphia are isolated substances, entirely unconnected, in constitution or general relations, with any other substance. Aniline, on the other hand, is a member of one of the most extensive, widely distributed, and interesting groups of substances, with which the recent discoveries of organic chemistry have made us acquainted, the Indigo Salicyl and Benzoil series. The members of this larger group already present a variety of instances both of isomeric and polymeric compounds, a few of which I

* *Annales de Chimie et de Physique*, vol. liv.

† *Proceedings of the Royal Society of Edinburgh*, No. 26.

have here brought together in the form of a table, which does not pretend to any scientific arrangement, its sole object being to point out the remarkable relations of aniline and picoline to the group.

Indigogene,	$C_{16}H_6NO_2$	Indine.
Indigo,	$C_{16}H_5NO_2$...
Isatine,	$C_{16}H_5NO_4$...
Anthranilic acid,	$C_{14}H_7NO_4$...
Salicylic acid,	$C_{14}H_6O_6$?*
Nitrosalicylic acid,	$C_{14}H_5(NO_4)O_6$...
Benzoic acid,	$C_{14}H_6O_4$	Salicylous acid.
Nitrobenzoic acid,	$C_{14}H_5(NO_4)O_4$	Nitrosalicylous acid.
Chlorobenzoic acid,	$C_{14}H_5ClO_4$	Chlorosalicylous acid.
Hydruet of benzoil,	$C_{14}H_6O_2$	Benzoine.
Benzonitril,	$C_{14}H_5N$	Azotide of Benzoil.
Stilbene,	$C_{14}H_6$...
Phenol,	$C_{12}H_6O_2$...
Aniline,	$C_{12}H_7N$	Picoline.
Tribromaniline,	$C_{12}H_4Br_3N$	Tribromopicoline ?
Benzin,	$C_{12}H_6$?
Nitrobenzid,	$C_{12}H_5(NO_4)$...

The facility with which aniline can be obtained by the decomposition of different members of this group, renders it by no means impossible to anticipate the artificial production of picoline also.

As we can start from benzoic acid, and convert it into benzin, benzin into nitrobenzid, and that finally into aniline by the action of sulphuretted hydrogen, it seems by no means improbable that salicylous acid, the isomeric of benzoic acid, may be made to undergo a similar series of changes, the final result of which would be the formation either of picoline, or of some other compound isomeric with it and aniline. In order to subject this hypothesis to the test of experiment, I mixed salicylous acid with equal weights of slaked lime and caustic baryta, and distilled in the oil bath, with the view of obtaining a substance which should be isomeric with benzin. The greater part of the salicylous acid, however, passed over unchanged; but by agitating with solution of potass, there was left undissolved an excessively minute quantity of a solid crystalline substance. Finding this mode of operating unsuccessful, I passed salicylous acid over spongy platinum heated to a very low red heat in a glass-tube. A dark viscid oily fluid passed over into the recipient, of which the greater quantity dissolved in caustic potass, but left behind a larger quantity of the solid substance than was yielded by the first experiment. By distillation with water this substance passed into the receiver in the form of oily drops, which solidified on cooling, and formed a crystalline mass in which minute needles could be detected. It had a peculiar pleasant smell which resembled that of benzin; but the quantity which I obtained was much too minute to admit of its analysis, or of any attempt to convert it into picoline.

* GERHARDT has observed (*Precis de Chimie Organique*, tom. ii., p. 21), that benzoic acid, when fused with hydrate of potass, evolves hydrogen, and gives the potass salt of a new acid. This may possibly be isomeric with salicylic acid.

POSTSCRIPT.

Although the analogy existing between picoline and the other oleaginous bases is perfectly sufficient to warrant the assumption of the absence of oxygen in that substance, I have thought it advisable to append here an experimental determination of the nitrogen. As the volatile bases cannot be readily analysed by VARRENTRAP and WILL'S method, I made a combustion of the platinum salt, and determined the proportion by volume of the carbonic acid and nitrogen in four tubes, which gave the following results :—

I. 94 volumes gave 8· nitrogen.				
II.	240	...	18·	...
III.	84	...	6·5	...
IV.	421	...	35·	...
<hr/>			<hr/>	
839			67·5	

These results give the gases in the proportion of $11\frac{1}{2}$ to 1 ; in other words, they shew a slight excess over the theoretical result, according to which they should be in the proportion of 12 to 1. They confirm perfectly, however, the absence of oxygen.

XV.—*Results of the Makerstoun Observations, No. II. On the Relation of the Variations of the Vertical Component of the Earth's Magnetic Intensity to the Solar and Lunar Periods.* With a Plate. By J. ALLAN BROWN, Esq., *Director of General Sir T. M. BRISBANE'S Magnetical and Meteorological Observatory.* Communicated by General Sir T. M. BRISBANE, Bart.

(Read 20th April 1846.)

1. The following results are deduced from the observations of the balance or vertical force magnetometer, which consists of a magnetic needle, balanced horizontally, and resting, by a knife-edged axle, on agate planes. Much doubt has been entertained as to this instrument's capability of shewing changes of moderate nicety, and it has been considered altogether unavailable for changes of long period.* it has been shewn (Vol. XVI., p. 67), that there are several difficulties in the way of an accurate interpretation of the observations, independent of the instrumental capacity. If it be added, that disturbances seem to affect the daily means of the vertical component,† in a more serious way than they do those of the horizontal component, it will be seen that there are a series of difficulties, which tend to render good and consistent results from the balance magnetometer nearly unattainable. It will be judged afterwards how far these difficulties have been overcome in the present instance.

2. The changes of the vertical component are, in general, very gradual and regular; even during considerable disturbances the balance needle moves gradually to an extreme position, remaining there, with moderate fluctuations, for a considerable period, and afterwards returning slowly to its mean (or nearly mean) position; the bifilar magnet, on the contrary, moves with considerable rapidity from one extreme position to another. This difference in the mode of disturbance, I do not conceive due to a want of sensibility in the balance needle, but rather to a difference in the nature of the disturbances of the dip, and of the total intensity.

3. The changes of the vertical component, then, during disturbances, will evidently have little or no effect on the *regularity* of the diurnal curves, although the periods of maxima and minima may be affected; it will be different for the daily means, for, as the disturbances will, in general, be almost wholly negative

* Revised instructions by a Committee of the Royal Society of London, p. 37.

† I shall generally use the terms vertical or horizontal component, instead of vertical or horizontal force; the latter are not at all expressive where the changes may be altogether due to variations of dip.

or positive, there will be sudden and considerable daily decrements or increments of the vertical component. If the effect of these disturbances be equally positive and negative, it may be expected that, in the mean of a sufficient number of observations, they will destroy each other, or, if almost wholly negative, that they will be distributed equally over any period for which the law is desired. If, however, we expect consistent and regular results, from short series of observations, especially when the range of the variations for the resulting law is small, it is evidently desirable to eliminate disturbances as far as possible. To do so completely, requires a knowledge of the observations which should be classed as disturbed, a matter of difficult attainment, as disturbances have various magnitudes, and we have to distinguish between variations which are periodic, and those which are not so; the latter constituting what are distinctively termed disturbances. Without doubt, disturbances seem to obey an *irregular* periodic law, that is to say, they occur more frequently at certain hours than at others; but there are whole months in which they are not at all apparent, and in which, each day's observations represent equally, in range and periods of maxima and minima, the diurnal law; thus, in the months of June and July 1844, each day's observations of the horizontal component when projected, exhibit, with a few slight exceptions, a complete representation of the monthly curve, affected, however, to some extent by the varying hour angle of the moon.

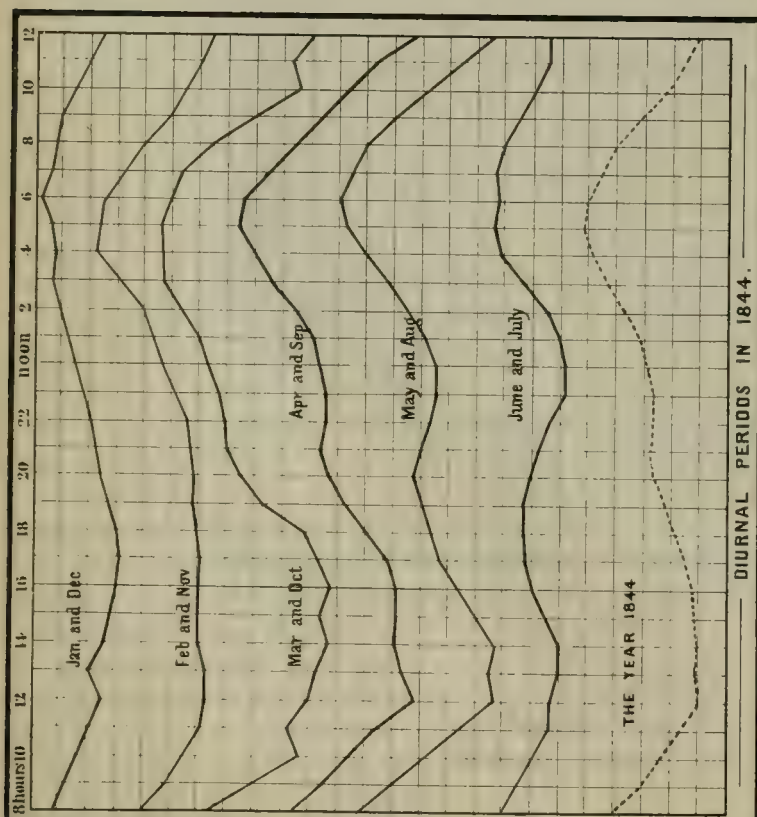
4. As it is only the larger disturbances which affect the periodic result to a marked extent, the complete elimination of disturbances is fortunately not of much moment; the methods which I have adopted for the elimination of these will be stated afterwards; it will be found, however, that I have depended as little as possible on these eliminations.

5. The results as yet obtained for the variations of the vertical component of the earth's magnetic intensity are confined, as far as I am aware, to determinations of the mean period of diurnal maxima and minima. These may be obtained from a good instrument, with moderate exactness, in spite of considerable inaccuracy in the temperature correction, as the mean diurnal variation of temperature, in well closed rooms and boxes, will not exceed a few degrees of Fahrenheit. The Toronto observations for 1841 and 1842, give consistent results, indicating

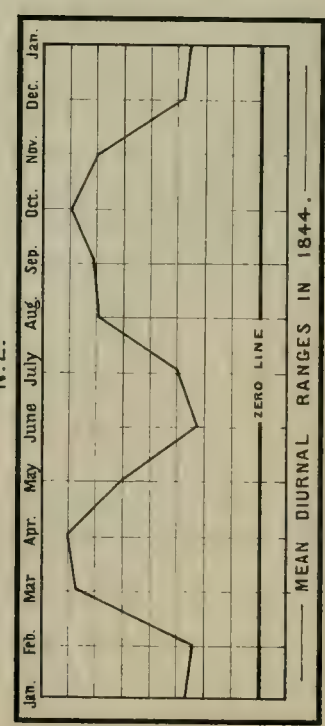
A principal maximum at 6 ^h Toronto mean time.		
A principal minimum at 14 ^h
A secondary maximum at 20 ^h
A secondary minimum at 22 ^h

There are only three months of the twenty-four indicating a single maximum and a single minimum, namely, the months of November 1842 and September of each



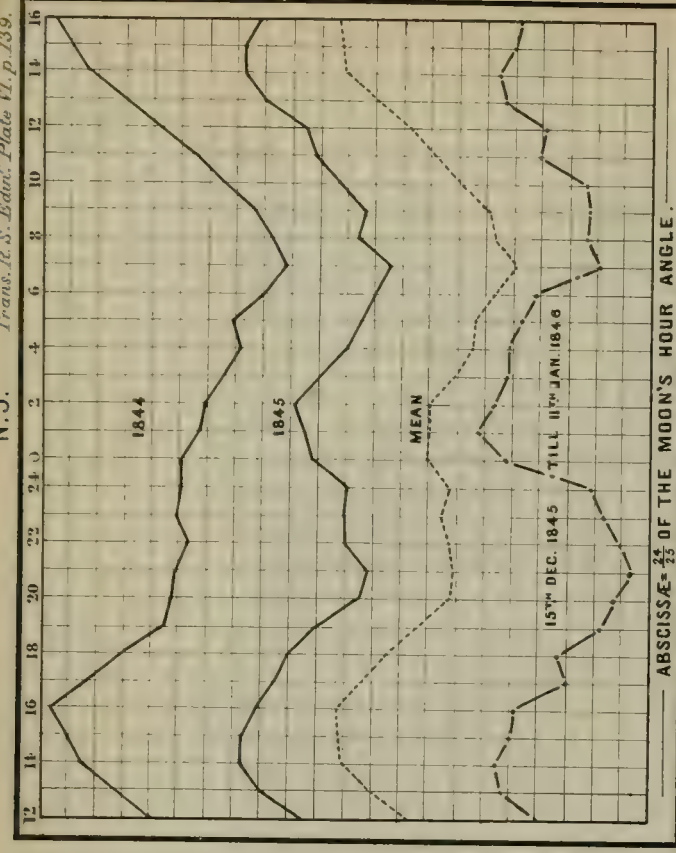


N. 2.

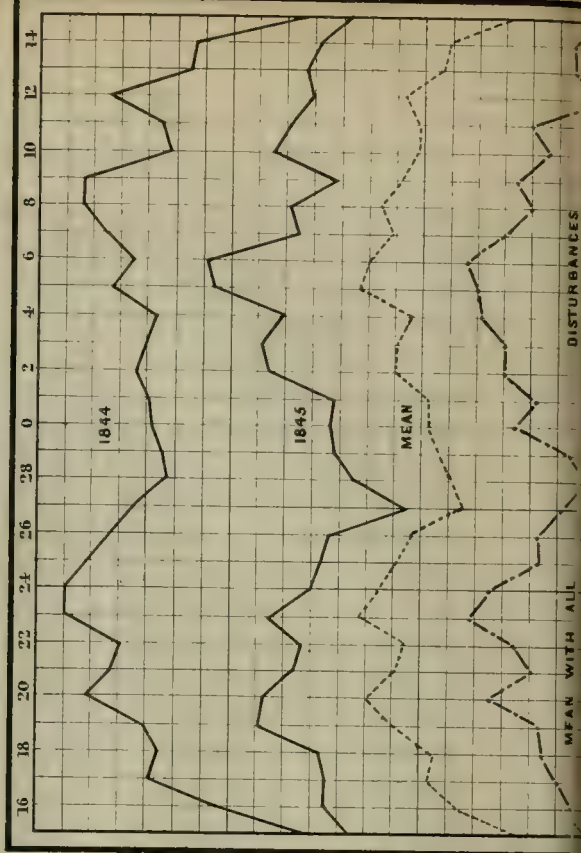


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CURVES To illustrate the Changes



N. 4.



year; the diurnal range is greatest in summer, and least in winter.* These results will be found to differ considerably from the following, excepting in the periods for the whole year.

6. The mean of the vertical component for the year 1844, at Makerstoun, was least at 14^h (Makerstoun mean time); it increased gradually, from that time, till nearly 21^h; it then diminished slightly till nearly 23^h; after which it increased rather rapidly till 5^h 30^m; it then diminished with much rapidity till midnight, increasing slightly from 12^h to 13^h, and diminishing slightly again to the minimum at 14^h; or the vertical component has

The principal minimum.....	at 14 ^h 10 ^m	Makerstoun mean time.		
A secondary maximum	at 20 ^h 50 ^m	
A secondary minimum	at 22 ^h 50 ^m	
The principal maximum	at 5 ^h 30 ^m	
Perhaps a third minimum.....	at 12 ^h 10 ^m	
And a third maximum	at 13 ^h 10 ^m	

The slight inflexion at 13^h (See Mean Curve, No. 1., Plate VI.) would not have been noted as a maximum, but simply as a slight irregularity probably due to disturbances, were it not for the evidence which, with one exception, every month in the year gave to the same effect. If, however, it be considered simply as an irregularity (it will be the only one in the whole period), 13^h 10^m must be taken as the actual period of the principal minimum, the vertical component having nearly equal values at 12^h 10^m and 14^h 10^m.

7. The form of the diurnal curve, and the periods and number of maxima and minima vary throughout the year. Having found that the diurnal curves, for each of the two months symmetrically placed with regard to the solstices, were nearly identical in periods and range, only the mean curves for each couple have been projected (Curves, No. 1, Plate VI.), namely, the means for the months of

January and December.	April and September.
February and November.	May and August.
March and October.	June and July.

8. The transitions of the periods of maxima and minima throughout these months is curious; the principal maximum occurs about 6^h at the solstices, near 4^h at the equinoxes, and about 5^h at the intermediate periods. The principal minimum occurs about 17^h at the winter solstice, about midnight at the equinoxes, and about noon at the summer solstice. In January and December there

* Toronto Observations for 1840, 1841, and 1842. Abstracts, &c., p. lx. The Toronto observations were made every two hours, and the Makerstoun observations every hour.

is no minimum visible near noon; in February and November an inflexion occurs there; in March and October there is a marked tendency to a minimum at that time: there is a well-marked minimum near noon in April and September; a strongly marked one about the same time in May and August; and the principal minimum occurs in June and July near mid-day. The same progression holds for the maximum, which occurs near 20^h, excepting that it never becomes the principal maximum, though in June it differs very little from it. The inferior maximum and minimum are best marked near the equinoxes.

The following table will exhibit the transitions of the periods with more distinctness; the negative sign (–) is placed before the hour of the principal minimum, and the positive sign (+) before the hour of the principal maximum.

TABLE of the Hours, Makerstoun mean solar time (astronomical reckoning), of the Maxima and Minima of the vertical component of Magnetic Intensity for 1844.

Months.	Minimum.		Maximum.		Minimum.		Maximum.		Minimum.		Maximum.	
	h.	m.	h.	m.	h.	m.	h.	m.	h.	m.	h.	m.
January and December,	–17	10	+6	10	12	10	13	10
February and November,	–116	40*	+5	0	12	40	14	40
March and October,	–16	10	(22)	?	(0)	?	+4	10	14	10	15	10
April and September,	15	10	21	10	23	40	+5	20	–12	10	14	20
May and August,	–14	10	20	10	23	40	+5	50	12	10	13	10
June and July,	13	30	18	50	–23	40	+6	0	11	30	12	0

It will be observed that the hours of the first minimum and maximum in this table occupy a year in completing their transitions, that the hour of the second minimum in the table is constant, and that the hours of the second maximum, and of the last minimum and maximum in the table, complete their transitions in six months.

9. Neglecting the inferior maximum and minimum, it will be remarked that the period of duplication in the forms of the curves differs completely from that for the horizontal component. For the latter, it is summer in which the diurnal curve becomes single, and winter in which it becomes double. It is the secondary or morning *maximum* of the horizontal component which becomes the principal maximum in *winter*; it is the secondary or noon *minimum* of the vertical com-

* The principal minimum in the mean for the months of February and November actually occurs at 12^h 40^m. This is due to an apparent irregularity in the month of November, that month being the only one which does not shew the inferior maximum near midnight.

ponent which becomes the principal minimum in *summer*. For the horizontal component the 22^h minimum is *the* minimum throughout the year; for the vertical component the 5^h maximum is *the* maximum throughout the year.

10. Perhaps the most curious fact in connection with the vertical component is that of the annual variation of the diurnal range. It has always been imagined, I believe, that the diurnal range of all the magnetical elements increased from winter to summer. This has been shewn to be the case for the horizontal component (No. I., 6*), and will afterwards be shewn to be true for the magnetic declination. It is not so, however, for the vertical component, as may be seen by a glance at the six diurnal curves projected (Curves, No. 1, Plate VI.). The transition in form and range is evidently worst exhibited by the mean for February and November; this, however, and other irregularities, may probably be due to disturbance. The elimination of these (which I have not at present attempted), or the observations of other years, must decide this. When the range for each month is projected (Curve No. 2, Plate VI.), it is at once evident that the diurnal range is least at the solstices, and greatest at the equinoxes. The mean of the ranges for January and December, and also the mean for June and July (the solstitial months), is about 0.00028, the whole vertical component being unity, while the mean for each of the couples of equinoctial months, namely, of March and April, and of September and October, is about 0.00068.

11. It might have been expected that this curious variation of the ranges would shew itself more or less in the ranges for the horizontal component. If we refer to No. I. of these results (6), page 101 of this volume, and to the projected curves, No. 2 of that series (Plate III.), we shall find this actually the case, although in that place it was supposed that the variations in the regular increase of the ranges might be due to disturbances. These facts seem to point to a difference in the modes and causes of increase of the diurnal range for the magnetic dip, and for the total magnetic intensity; the diurnal ranges of the latter seeming to obey a law which is equally related to the two solstices, and also to the two equinoxes, a circumstance in favour of the annual period previously announced (No. I., 10).

12. The mean values of the vertical component, at 21^h and 0^h, are nearly equal to the mean for the year, but no single hour, as for the horizontal component (No. I., 8), indicates the mean for each month.

13. Proceeding in the order of No. I., I should now consider the annual period. The results for the different years are discordant. This, it is my opinion at present, is due to an insufficiency in the temperature correction, which will be found alluded to elsewhere.† The results of the years 1842 and 1843, the latter more strongly, indicate a period similar to that found for the horizontal component, namely, maxima of

* Reference to Results of the Makerstoun Observations, No. I., p. 99 of this volume.

† Introduction to the Makerstoun Observations for 1843.

the vertical component about the solstices and minima about the equinoxes; 1844 and 1845 also indicate a maximum at the winter solstice, but the maximum at the summer solstice is imperfectly shewn, if shewn at all.

14. It is quite evident that a moderate error in the temperature correction may be sufficient to destroy all appearance of an annual period, especially when the range of the temperature may be 30° Fahrenheit, and the range of the component for the annual period may be small. It is, I am inclined to think, due to the existence of a lighted stove in the Observatory in the winters of 1842, and in the beginning of 1843 (by means of which the annual range of temperature was much diminished), that these years give a somewhat distinct indication of the annual period. For many reasons, however, it is to the observations made in the years 1844 and 1845 that I look for a consistent exhibition of the annual period; and to the result for these years I shall return when the insufficiency alluded to has been remedied.

15. The vertical component has diminished considerably since 1841, the yearly rate of diminution becoming less in each year. Something of this apparent change may be due to a loss of magnetism in the balance needle; but it is believed that this is only partially, if at all, the case. There is a curious change in the rate of diminution of the vertical component in the year 1845: in October, November, and December, it was constant or very nearly so; it was remarked of the horizontal component for 1845, that it had increased much less than in the previous year. Does this point to an approaching turning-point for the diminution of the magnetic dip?

16. Similar summations, to those indicated for the horizontal component (No. I., 25), were made for the vertical component, at the varying hour angles of the moon; the larger disturbances were also eliminated similarly, the test number for disturbances being taken more than twice as great in 1845 as in 1844. From a mean of 12 lunations in 1844 (see Curves No. 3, Plate VI.), the maximum of vertical component occurred when the moon had passed the inferior meridian about three hours, the value of the component then diminished considerably till 19^h (counting the moon on the meridian 0^h , and so up to 24^h , when it wants one hour, or, more exactly, $\frac{2}{3}$ of an hour of being on the meridian again; each of the so-called hours having only this value, 19^h corresponds to about $5^h 45^m$ before the moon's meridian passage); it diminishes slightly from 19^h till 22^h , when there is a minimum; it then increases slightly till 24^h or 0^h when there is a maximum; after this it diminishes moderately again till 7^h , when the principal minimum occurs; it then increases rapidly to the maximum at 16^h . When the disturbances are not eliminated the maximum occurs at 15^h , the component then diminishes rapidly, with some irregularities, till 2^h , which is the time of the principal minimum; a secondary maximum then occurs at 4^h , and a minimum, differing very little from the other in value, occurs at 6^h or 7^h . A more complete elimination of disturbances, it is conceived, would render the maximum about 0^h still more evident.

17. In 1845 the maximum occurs between 14^h and 15^h , or about 2^h after the moon has passed the inferior meridian, the value of the vertical component then diminishes till 21^h when there is a secondary minimum, it then increases considerably till 2^h ; a secondary maximum occurring about $1\frac{1}{4}^h$, it again diminishes till 7^h , when the principal minimum occurs, after which it increases rapidly to the principal maximum. This year's result is little affected by the elimination of disturbances; the curve is rendered somewhat more regular.

18. The results for the two years differ slightly from each other. The principal maximum occurs about an hour later in 1844 than in 1845; the principal minimum occurs at the same time in both years; but the secondary maximum and minimum are not nearly so well defined in 1844 as in 1845. The mean of the two years gives the following periods:—

The principal minimum about 5 hours before the moon's passage of the inferior meridian.	
The principal maximum about 3 hours after	
A secondary minimum about 4 hours before	superior
A secondary maximum about 1 hour after	

19. These periods are surprisingly near those indicated (No. I., 23), for the horizontal component, the principal maximum and secondary minimum of the vertical component occurring about an hour after those of the horizontal component, while the principal minimum and secondary maximum occur an hour or more before those of the horizontal component; seeming to indicate that these variations belong chiefly to the total intensity.

20. As evidence of the accuracy of the results obtained after eliminating disturbances, nothing perhaps could be more conclusive than the fact, that in those months in which no disturbances occur, the general law is found well marked: it would not be difficult to bring this kind of evidence to bear, both for the horizontal and vertical components. I shall satisfy myself at present by giving the projection of one month's results, namely, the results for the lunation included by December 15. 1845, and January 10. 1846 (Curves No. 3, Plate VI.). From this single month the same, or very nearly the same, periods of maxima and minima are obtained as in the mean for the two years. The maxima have nearly equal values, and so also have the minima; the principal maximum and minimum, however, occur at the times of the secondary maximum and minimum of the mean curve. Such a difference in the values of the maxima might be expected; for, though the moon has every declination in the course of one lunation, it is full only at one of these declinations. It would not have been difficult to have obtained a curve from one month's observations representing the mean curve better.

21. In order to obtain the variations of the vertical component with respect to the moon's age, summations were made similar to those indicated (No. I., 18) for the horizontal component. It should be mentioned (as it should have been before for the horizontal component), that in all the results for monthly periods mean

values for Sundays were used. These were obtained by interpolation; the mean of the three days preceding, and of the three days succeeding, were taken as the means for the Sundays. The want of approximate means for these days would tend to destroy the regularity and distinctness of the results, owing to the variations, due to different causes, which it is desired to eliminate, and that the blank days enter irregularly into the days of the mean monthly periods.

22. The results of these summations (Curves No. 4, Plate VI.) are, that each year indicates maxima of the vertical component near the quadratures, and minima near the syzygies. In order to render the fact more distinct, and the curves somewhat more regular, eleven days* of greatest disturbance were eliminated in each of the years 1844 and 1845, namely, those days on which the mean value of the vertical component was greater than the mean of the previous and succeeding days by more than 26 micrometer divisions (about three times the resulting range). The principal minimum in 1844 occurs at the period of full moon; in 1845 it occurs about three days before the new moon. In the mean of both years the principal minimum occurs at the time of full moon, the secondary minimum about two days before new moon; the maxima occur between these periods, and they are nearly equal. The lowest curve of No. 4 is the projection of the mean for the two years without eliminating disturbances. It differs little in regularity from the other in which the large disturbances are eliminated. The irregularities in these curves may be partially due to the cause of error already stated (17), namely, an insufficiency in the temperature correction.

23. Whether due to the cause just mentioned or not is uncertain, but the results for the relation of the variations of the vertical component to the moon's declination are neither distinct nor consistent for the two years, and the elimination of the days of large disturbance does not improve them. The mean for the two years seems to shew something like the law found for the horizontal component, namely, maxima about the periods of greatest north and south declination; but I do not place any trust at present in this result.

MAKERSTOUN, *April 13. 1846.*

* Each of these days was actually observed (observations were made) at the time as a day of disturbance.

XVI.—*On the Solubility of Fluoride of Calcium in Water, and its relation to the occurrence of Fluorine in Minerals, and in Recent and Fossil Plants and Animals.* By GEORGE WILSON, M.D.

(Read April 6. 1846.)

1. *Introductory Remarks.*

THE investigation I am about to bring before the Royal Society, was undertaken in consequence of a discussion which took place in the Zoological Society of London in 1843,* in reference to the chemical composition of the bones of the gigantic bird the *Dinornis*, discovered some time previously in New Zealand. At the meeting in question, the distinguished palæontologist Dr FALCONER drew attention to the frequent, if not constant, occurrence of fluoride of calcium in fossil bones, and, as he stated, also in those of mummies; and threw out the suggestion, that the fluoride might shew itself in these animal remains, not as an original ingredient of the bones, or as derived from the matrix in which they were found, but as a product of the transmutation of their phosphate of lime. The idea of such a conversion taking place, is as old at least as the days of KLAPROTH, who suggested the possibility of phosphoric acid becoming changed into fluoric.† It is commented upon by FOURCROY and VAUQUELIN,‡ as well as by GAY LUSSAC,§ as a thing possible but not probable, and which their ignorance of the nature of fluoric acid prevented them from discussing satisfactorily.

The revival of this suggestion by Dr FALCONER, at a period when the possibility of the chemical elements undergoing transmutation was occupying the attention of English chemists, and avowedly with a view to shew at least the possibility of such an idea proving true, excited much discussion, and led, I believe, to the researches of Mr MIDDLETON and Dr DAUBENY, which I am presently to mention, and of which my own may be considered the sequel. I have to request the forbearance of the reader, whilst, with as much brevity as possible, I refer to the labours of my predecessors in relation to the presence of fluorine in different bodies.

In 1802, MORICHINI of Rome discovered fluoride of calcium in the molars of a fossil elephant, and was led, in consequence, to search for it in the enamel of recent human teeth, where he also found it.|| His results were confirmed by GAY LUSSAC, who experimented along with him,¶ and by BERZELIUS, who found

* Literary Gazette, Dec. 2. 1843, p. 779.

† Ibid., p. 44.

|| Ibid., p. 258.

† Annales de Chimie, tome lvii. (1806), p. 43.

§ Ibid., tom. lv., p. 265.

¶ Ibid.

the fluoride in the recent bones of man and of the ox; and ascertained the proportion in which it was present in both.* On the authority of these chemists, fluorine was ranked among the constituents of animal bodies. Many excellent observers, however, soon after declared themselves unable to detect that element in recent bones. Among these are FOURCROY, VAUQUELIN,† WOLLASTON, BRANDE, Dr T. THOMSON,‡ GIRARDIN, PREISSER, and REES;§ the last of whom is not content with stating that he found no fluorine in unburied bones, but affirms that no one else can have met with it in them. More recently, Mr MIDDLETON|| and Dr DAUBENY¶ have experienced no difficulty in confirming the original results of MORICHINI and BERZELIUS. An American observer has been equally successful.** Dr GREGORY informs me that he has made many examinations for fluorine in recent bones, and has always found it present in them. My own experience of the subject is to the same effect. I shew the Society glass etched by recent human bones, male, female, and foetal, which were obtained, without special selection, from the dissecting-room; likewise glass corroded by hydrofluoric acid from the tusk of the recent elephant, and the teeth of the recent hippopotamus, walrus, leopard, and shark.

I shall return, in another section of this paper, to the consideration of the question, how the discrepance in the statements of observers concerning the presence of fluorine in recent bones is to be accounted for. It was the occurrence of that element in fossil bones which gave rise to the discussions concerning its origin, to which I shall have occasion to refer. Fluorine is not a constant ingredient of the animal remains in question, according to FOURCROY and VAUQUELIN, who examined some which contained none. But in the greater number of cases it has been found present, so that GIRARDIN and PREISSER have even proposed to consider its existence in an unknown bone as a proof of the latter not having belonged to man or to any recent organism, but to some "antediluvian animal." ††

It is acknowledged, moreover, that in buried bones, especially in those that are petrified, fluorine is frequently present in larger proportion than in recent ones. Thus LASSAIGNE found fifteen per cent. of fluoride of calcium in the bones of the Anoplotherium; †† MIDDLETON ten per cent. in those of various animals from the Sewalik Hills; §§ GIRARDIN and PREISSER nine per cent. in those of the Laman-tin. ||| Mr MIDDLETON, indeed, has endeavoured to shew that the proportion of fluoride of calcium increases according to the period of the entombment of the bone at the rate of $1\frac{1}{2}$ per cent. in a thousand years, and has proposed to estimate

* Annales de Chimie, tom. lxi. (1807), p. 256. † Ibid., 1806, t. lvii., p. 41.

‡ Chemistry of Animal Bodies, p. 236.

§ Guy's Hospital Reports, quoted in Edin. Phil. Journal, vol. xxviii., p. 93.

|| Chemical Society's Memoirs, vol. ii., p. 135.

¶ Ibid., p. 101.

** Edin. Phil. Journal, vol. xxxix., p. 235.

†† Ann. de Chim., t. ix. (1843), p. 381.

‡‡ Quarterly Journal of Geological Society, vol. i., p. 216.

§§ Ibid.

||| Ann. de Ch. et Ph., t. ix., p. 375, 1843.

the age of bones, and of the rocks containing them, by the per-centage of fluorine in the former.* This idea, however, is certainly unwarranted. In the bones of five fossil animals, including the Plesiosaurus and Ichthyosaurus, GIRARDIN and PREISSER found from one to two per cent. of fluoride of calcium;† whilst in those of the recent ox, BERZELIUS found nine per cent.‡ In the ancient bones there was thus, instead of a much higher per-centage, seven per cent. less of fluorine than in the recent bones. Many other objections might be made to Mr MIDDLETON's view.

Those who deny the existence of fluorine in recent bones, consider the whole amount of that element found in ancient buried ones, as in some way or other a product of fossilisation. According to those, on the other hand, who affirm the presence of that element in recent organisms, only a portion, at most, of the fluorine found in osseous remains has been added since they ceased to be parts of living animals. It is impossible, however, to separate the two questions. We have no data from which to determine whether or not the bones of an extinct animal contained fluorine during its life, and, if it did, how much was present. It will be sufficient, therefore, if I consider what progress has been made in answering the one question, How does fluoride of calcium come to be present in bones, either recent or fossil?

Three replies have been proposed to this query. 1st, That of Dr FALCONER, already referred to, which, taking for granted that fossil bones contain more fluorine than they possessed whilst parts of living animals, assumes, or rather suggests as possible, that phosphate of lime has been transmuted into fluoride of calcium.§ 2d, That of LIEBIG, which, going on exactly an opposite assumption, takes for granted (if I understand him aright) that bones of living antediluvian animals contained the same proportion of fluorine which we find in their fossil remains; and refers its greater abundance in these, either to its having been present in larger quantity in the food of their living possessors, than it is in that of existing animals, or to its having been appropriated to a larger amount from it.|| The third is that of Mr MIDDLETON, who supposes every bone to possess normally two per cent. of fluoride of calcium, and considers all above that which a fossil bone contains, as added to it whilst buried in the earth, by the infiltration of water containing that salt held in solution by some unknown solvent.

It is unnecessary to discuss the first and second propositions referred to. It is impossible, in the present state of our knowledge, either to prove or to disprove them. The idea of transmutation of a phosphate into a fluoride, was doubtless suggested solely because there seemed no other way of accounting for the accumulation of fluorine, and will be abandoned, if it shall appear that recognized

* Quarterly Journal of Geological Society, vol. i., p. 216.

† Annales de Chimie, 1843, pp. 370-78.

‡ Ibid., t. lxi., p. 257.

§ Literary Gazette, 1843, p. 779.

|| Chemistry of Agriculture, 3d edition, p. 123.

chemical forces can explain the phenomenon. The great German chemist, also, (whose view may be the true one,) will probably modify his opinion, when he finds that fluoride of calcium is soluble in water.

Mr MIDDLETON's supposition that all bones contain two per cent. of fluor, is certainly untenable, and so is his belief that bones invariably gain fluorine whilst undergoing fossilisation; but he brought satisfactorily to the test of experiment his view that fluoride of calcium may reach the bones both of living and dead animals through the medium of water. His experiments were not made with aqueous solutions, in which a mere trace of fluoride could at best be expected to be present, but with sedimentary deposits, of natural and artificial origin. "I was led," says he, "to institute a series of experiments on aqueous deposits of different ages, and I found, that, with one exception (a pure but incompact stalactite of carbonate of lime), fluorine exists in all, from the most recent deposit down to the old red sandstone, and that it is present in the older in larger proportion than in the newer beds. I think it is, therefore, beyond a doubt, that it is present in water, though, perhaps, in very minute quantity. *What its solvent may be I know not*; but that it is so held in solution my own experiments have demonstrated; and if they had not, the simple fact that the blood conveys it to the bones, would, I apprehend, sufficiently refute any scepticism on the subject."*

It may justly be questioned, whether the fact of a substance being soluble in a highly complex fluid like blood, would entitle us to infer that it was equally soluble in pure water. But it is singular that Mr MIDDLETON, holding such a view, and after finding fluorine in so many aqueous deposits, should not have endeavoured to dissolve the fluoride of calcium in water. He was, doubtless, prevented from making any trials on the subject by the universal statement of chemists, that the salt in question is quite insoluble in water.

2. *Of the Solubility of Fluoride of Calcium in Water.*

Many substances are spoken of by chemists as insoluble in water which are, nevertheless, known to possess a certain slight solubility in that fluid. But fluoride of calcium has been considered so well entitled to the character of total insolubility, that our most accurate analysts, as BERZELIUS and ROSE, have purposely converted fluorine into this salt in their quantitative determinations of the former, and have washed the latter freely with water, and, as they believed, without its suffering any loss. Their example has been followed by all other analysts, and the fact supplies a better proof than any quotation of individual authors could do, that fluoride of calcium has been considered quite insoluble in water. Relying implicitly on the truth of this belief, I sought for a solvent of fluor-spar which could retain it in union with water, and carry it into the tissues of plants and

* Quarterly Journal of Geology, vol. i., p. 215. Mr MIDDLETON's other papers on fluorine are in the Chemical Society's Memoirs, vol. ii., p. 134; and in the London Phil. Mag., No. 164, p. 14.

animals. The frequent association of phosphate of lime and fluoride of calcium in minerals, naturally suggested that whatever substance enabled water to become charged with the one salt, would cause it to dissolve the other. Carbonic acid is known to be one agent which confers upon water the power of taking up phosphate of lime; it seemed worth while, therefore, to try whether it would cause it to dissolve fluoride of calcium as it does so many other lime-salts. I was not aware that DANA the American mineralogist,* and Professor GRAHAM of London,† had anticipated me in this idea, or I should probably not have performed any experiments on the subject. In ignorance of their views, the following trials were made. A portion of pale green crystallised fluor-spar was reduced to fine powder and digested for some hours in warm nitromuriatic acid, so as to remove any carbonate of lime, metallic oxides, or other foreign matters, which might be present. It was then washed on a filter, dried, and suspended in pure distilled water, through which a current of carbonic acid was passed for two hours. At the end of this period the liquid was filtered through paper, and tested for lime by oxalate of ammonia. A cloudiness was soon occasioned, and speedily a white precipitate. On evaporating the liquid to dryness, a greyish-white residue was left which gave off sharp acid fumes when moistened with oil of vitriol. When this residue was warmed with Nordhausen sulphuric acid in a platina crucible covered by glass, the latter was deeply corroded in a few minutes. The process was repeated many times, and always with the same result. I shew the Society squares of glass which were etched in this way; the engraved words having been traced through wax, as in the ordinary method of testing for hydrofluoric acid. The experiments referred to were made in January last, and were supposed to justify the idea which led to their trial, namely, that carbonic acid was the agent which enabled water to dissolve fluor-spar.

If carbonic acid, however, had been essential to the retention of fluor in solution, the expulsion of that gas, by warming the liquid, should have been followed by the deposition of the fluoride. I was struck, however, by observing that the solution could be raised to the boiling-point, without any troubling or opalescence appearing, and that no precipitate shewed itself after protracted ebullition. It was manifest that water was able of itself to retain in solution the fluoride if once dissolved in it; and highly probable that it would prove equally sufficient to commence the solution of the lime-salt. The experiment was accordingly tried of suspending fluoride of calcium in cold distilled water, and shaking it occasionally in a stoppered bottle for two hours. The liquid, after filtration, shewed lime with oxalate of ammonia as readily as the carbonic acid solution had done, and left, after evaporation, a residue which gave, with oil of vitriol, acid vapours etching

* Edin. Phil. Jour., vol. xxxix., p. 255.

† Note to Mr MIDDLETON'S paper, Quart. Jour. Geol. Soc., vol. i., p. 216.

glass. Distilled water was then boiled upon powdered fluor-spar and filtered whilst hot. It precipitated oxalate of ammonia instantaneously ; and deposited, after cooling, a small quantity of a white precipitate, which answered to the tests of lime, and, when moistened with strong oil of vitriol, gave off an acid which corroded glass. The supernatant liquid likewise precipitated oxalate of ammonia, but more slowly, and yielded, on evaporation, a residue identical in characters with the deposit from the hot aqueous solution. When the deposit or residue was mixed with pounded glass and oil of vitriol, and heated in a flask, a gas was given off which deposited gelatinous silica when passed through water, and had all the characters of fluosilicic acid. It was manifest from these trials, that water can dissolve fluoride of calcium ; and that it is more soluble in boiling than in cold water.

The experiments I have mentioned are of so simple and decisive a kind, that the conclusion they warrant cannot be evaded. That no error might arise from impurity of material, many of them were made with water twice distilled, and ascertained to be quite free from foreign matter. On the other hand, specimens of fluor-spar were obtained from different cabinets ; some massive ; the greater number well crystallised. The fluor was finely powdered, and thereafter, in the greater number of cases, digested in warm aqua regia, washed and dried. The only foreign body likely to be present, which could escape removal by this treatment, is silica, a substance which would lessen rather than increase the solubility of the fluor. Lest, moreover, the agents employed to purify the fluoride of calcium should be supposed to have conferred on it a solubility which it did not originally possess, other trials were made with native crystals, which, without preliminary treatment, were reduced to powder and boiled with distilled water. In every case solutions were obtained, which, when cooled, yielded a deposit, or, when evaporated, a residue, which gave off hydrofluoric acid when moistened with oil of vitriol, and left sulphate of lime.

The pieces of etched glass which I shew the Society were corroded by hydrofluoric acid obtained from the fluoride of calcium previously in solution in water. They will be observed to be as deeply *bitten in* as if undissolved fluor-spar had been made use of. Four liquid ounces of the cold aqueous solution will be found to leave sufficient residue to etch glass permanently. The residue from the same amount of solution made at 212° Fahr. will act still more decisively.

The solution of fluoride of calcium in water at 60° is colourless, transparent, tasteless, and precipitates oxalate of ammonia. Chloride of barium and nitrate of baryta occasion a white precipitate. These reagents act more readily with the solution at 212° .

The only one of these reactions I have yet found time to examine with any attention is that of the salts of barium. The precipitate they occasion yields hydrofluoric acid abundantly, when treated with oil of vitriol. I have not ascer-

tained whether it is simply a fluoride of barium, as it is likely to be, when nitrate of baryta is employed ; or a double fluoride and chloride of barium, as it may be, when the latter is the precipitant. BERZELIUS has described such a salt. But I have frequently availed myself of the fact that barium forms a sparingly soluble compound with fluorine, in seeking for the latter in liquids. They are often most conveniently tested for that substance by precipitating them by a salt of baryta, and testing the precipitate for hydrofluoric acid. This reaction, moreover, has an important relation to qualitative chemical analysis, inasmuch as it throws an unsuspected difficulty in the way of distinguishing dissolved sulphates from fluorides. The barytic precipitate, with solution of fluoride of calcium, is soluble in excess of nitric and hydrochloric acids, but it requires a much larger addition of these to redissolve it, than the carbonate, borate, or phosphate of baryta does. A fluoride, therefore, may readily be mistaken for a sulphate, or a mixture of both for only the latter. This mistake must have been frequently made in analysing mineral waters, where fluorine is certainly more abundant than has hitherto been suspected. When fresh analyses of these bodies shall be made, I have little doubt that where fluorine is met with, as Mr MIDDLETON has already discovered it in the pipe-water of London, and I have detected it in one of the wells of Edinburgh, it is the sulphates that will be found to have been over-estimated, at the expense of whatever proportion of fluorine was also present.

The fact of the solubility of the fluoride of calcium in water introduces an insurmountable objection to the present method of estimating fluorine quantitatively. It accounts in part for the discrepance between the result obtained, when fluorine has been estimated by the loss which a substance containing it sustained when heated with sulphuric acid, as contrasted with that which has been procured when the hydrofluoric acid evolved was condensed in ammonia, and precipitated by solution of chloride of calcium. Dr DAUBENY, for example, mentions that phosphorite from Estremadura, yielded, according to the first method, fifteen per cent. of fluoride of calcium, according to the second, not nine per cent.* Part of the difference was doubtless owing to the difficulty with which fluor-spar is made to abandon all its fluorine when distilled with oil of vitriol, in consequence of the pasty condition of the sulphate of lime which is formed. But when we find Dr DAUBENY mentioning, that he subjected the precipitated fluoride of calcium to "repeated washings with water," in order to remove any accompanying sulphate of lime, we may well suspect that fluoride of calcium was also washed away.

I regret that I cannot yet announce the proportion of fluor-spar which water dissolves. Owing to the corroding action which the solution occasionally exerts on glass, I thought it unadvisable to employ vessels of that material, or of porce-

* Chemical Society's Memoirs, vol. ii., p. 98.

lain. I endeavoured to substitute for these, silver basins, but found it impossible to prevent them gaining weight from the sulphuretted hydrogen constantly present in an analytical laboratory. Through the liberality of Dr GREGORY I have recently obtained a platina basin of much larger dimensions than the resources of my own laboratory afforded, and by means of it I shall be able to announce the proportion of fluor-spar taken up by water. Meanwhile, we cannot doubt that the proportion of fluorine has hitherto been estimated too low in most of the substances ascertained to contain it. In some cases the error must have been considerable.

I had hoped that the barium salt of fluorine would prove suitable for the quantitative estimation of the latter. It certainly would be much better than the fluoride of calcium, but according to BERZELIUS it possesses a certain though slight solubility in water. Fluoride of barium must accordingly be rejected also, unless no better compound for estimating fluorine quantitatively can be discovered. I may remark, in passing, that the fact that fluoride of barium is soluble in water might have led to the discovery that the similar salt of calcium was so likewise. The salts of barium, as a class, are much more insoluble than those of calcium. If, therefore, the barium compound of a salt-radical be soluble in water, the calcium salt of the same radical should, *a fortiori*, be still more so.

The observation of the previously unsuspected solubility of fluor-spar in water, promised to throw light on some interesting problems connected with geology, mineralogy, and physiology. I was induced, in consequence, to make a series of researches in reference to these points, the results of which I shall now briefly state.

Many of the investigations were very tedious, and I take this opportunity of expressing my obligations to two of my pupils, Mr HENRY C. BRIGGS and Mr HENRY WILLIAM STANSFELD, for the cordial and untiring assiduity with which they aided me in my researches. To my assistant, Mr DAVID FORBES, I have likewise been greatly indebted for the most active co-operation throughout the inquiry.

3. *Of the presence of Fluorine in Well, River, and Sea Water.*

It was impossible to doubt, after the facts I had observed in the laboratory, that fluorine must be no infrequent constituent of well and river as well as of sea water. BERZELIUS mentions that fluoride of calcium has been found in the waters of Carlsbad.* Dr CHRISTISON has pointed out to me that HÜNEFELD detected a trace of it in the waters of Gastein in the Tyrol,† and that PLANIAVA found gr. 0.07 of the fluoride in 10,000 grains of the water of Lukatschowitz in

* Lehrbuch der Chemie, vol. ii., p. 607.

† Bulletin des Sciences Médicales, vol. xvii., 425. From Jahrbuch der Chemie und Physik, xxii. 458.

Moravia.* MIDDLETON has found it in the London pipe-water, and in three other waters, the localities of which he does not mention ; but as the experiments were made in London, they were probably English. Traces of it have been found in other waters also.

I was induced to search for it in the water used in certain of the breweries in Edinburgh, in consequence of learning that these rapidly corrode the thermometers employed to regulate the temperature of the boilers and vats. The fact was first mentioned to me by a gentleman, who, before I made any trials on the subject, inferred that the corrosion of the glass must be owing to the presence of a fluoride in the water. I discredited the statement when I first heard it, supposing that an incrustation or deposition of sulphate and carbonate of lime had been mistaken for a true corrosion. I thought it impossible, moreover, that fluoride of calcium, even if it were present, could act upon glass. But in the course of the experiments already detailed, I had once occasion to notice that a new Berlin porcelain basin, in which a considerable quantity of the aqueous solution of fluoride of calcium was boiled down, had its glaze completely removed. On observing this fact, I applied to our intelligent instrument-maker, Mr STEVENSON, through whose hands the greater number of the thermometers used by the Edinburgh brewers pass, in the course of receiving necessary repairs. He informed me that he was quite familiar with the rapid dimming of the thermometers, and that it was a true corrosion ; in proof of which he gave me two pieces of broken thermometers, which I shew the Society. They are certainly abraded, and present a surface like that of ground glass. The roughening which is so manifest was not the result of friction against the sides of the brewing vessel, or any other kind of mechanical action ; for the corroded part of the thermometer-stem was enclosed in a brass-tube, and completely protected from external violence. It is proper to mention that the workmen in some of our breweries are in the practice of scraping the stems of their thermometers, to remove the deposit of lime-salts which rapidly gathers on them, and are ready to affirm that the apparent corrosion is an abrasion occasioned by their own knives. To guard against the possibility of any deception having occurred in this way, I visited the brewery of Mr CAMPBELL, situated in the Cowgate, behind Minto House, and was shewn by his manager a thermometer which had never been scraped with any instrument, and had been in use only a few weeks, but was nevertheless so dimmed, that it required to be dipped into water in order to confer upon it a temporary transparency, before the included mercury could be distinctly seen. Mr STEVENSON informs me that he finds the protected parts of the thermometer-stems, which are enclosed in brass-tubes, as much corroded as those which are exposed.

* Bulletin des Sciences Medicales, vol. xvii. p. 425. From Zeitschrift für Physik und Mathematik.

It seemed well worth while to seek for fluorine in one of these waters. I obtained accordingly from Mr CAMPBELL's brewery, a portion of the abundant deposit, consisting chiefly of sulphate and carbonate of lime, which collects with great rapidity in the boilers. It was treated with nitric acid, the dissolved portion poured off, neutralized with ammonia, and precipitated by nitrate of baryta. The precipitate, after being washed and dried, was warmed with Nordhausen sulphuric acid, in a lead basin; a square of waxed plate-glass, with characters traced through the wax, being laid as a cover over it. In this, as in all other experiments of the kind, a wall of wax was raised on the edges of the upper side of the glass, so as to retain a portion of water sufficient to keep the plate cool, and condense the hydrofluoric acid on it. This simple, but useful device, I borrowed from Dr DAUBENY.* Three squares of glass were very distinctly, though not deeply, etched in this way.

Fluorine, then, was present in this water; and the fact has an interesting relation to the circumstance pointed out to me by Mr ROSE, that the well from which it was obtained is sunk through a bed of sandstone, containing much mica, a mineral in which ROSE,† TURNER, GREGORY,‡ and other analysts, have found between 1 and 2 per cent. of fluorine. In reference to the corrosion of the brewery thermometers, however, I wish it to be distinctly understood, that I do not seek to refer the whole abrasion of the glass to the action of a fluoride dissolved in the water in which they are immersed. The well-known experiments of LAVOISIER, made in the end of last century, proved that even distilled water, if long boiled upon glass, can corrode it. Every chemist is familiar with the rapid action of solutions of the fixed alkalis, and of phosphate of soda on flint-glass. The inferior kinds of bottle-glass, especially when containing too little silica and excess of lime, have been shewn by FARADAY|| and WARRINGTON to suffer corrosion by the action of wine, and of diluted hydrochloric, sulphuric, and tartaric acids; and it would be rash to suppose that these are the only re-agents that can act upon artificial silicates, especially upon those which contain excess of basic oxides.

On the other hand, it is impossible not to connect the fact that the thermometers are corroded, with the circumstance that the water which occasions this corrosion contains fluoride of calcium. The other constituents of the brewery water are chloride of calcium and sodium, sulphate of lime and of soda, carbonate of lime and of magnesia, silica and organic matter; no one of which is known to have any action on glass.

In connection with this fact I may mention, that Mr STEVENSON finds the thermometers used in the breweries in the valley of the Cowgate much more

* Chemical Society's Memoirs, vol. ii., p. 101.

† BREWSTER's Journal of Science.

‡ POGGENDORF's Annalen, vol. i., p. 80.

|| Chemical Society's Memoirs, vol. ii., p. 247.

rapidly corroded than those employed in the similar establishments in the Canon-gate. As the action of the water may be supposed to be the same at both places, and the attending circumstances similar, it must be some constituent of the Cow-gate wells that occasions the difference. It may be the fluoride of calcium.*

To conclude this part of the subject, I may state that Dr CHRISTISON informs me, that he has frequently had occasion to notice that considerable quantities of natural waters evaporated to dryness in glass basins, permanently destroyed the transparency of the latter. From all that has been mentioned, it will appear that fluorine is likely to prove a frequent, though not an abundant, constituent of ordinary water. If the proposal to construct the pipes of our water-works of glass be put into practice, we may have an opportunity, on the large scale, of testing the truth of this idea.

It follows as a corollary, from the truths already detailed, that fluorine must be present in sea-water. The inference that it must be there, had been drawn by Mr MIDDLETON from the fact, that fluoride of calcium occurs in the shells of marine mollusca.† SILLIMAN junior has come to the same conclusion, apparently without a knowledge of MIDDLETON's views, in consequence of invariably finding the same fluoride in calcareous corals.‡ In the teeth of the walrus and of the shark, the only marine animals I have examined, I found fluorine very distinctly, especially in the latter.

I attacked the problem, however, directly, by examining the water of the Frith of Forth. A portion of the mother-liquor or *bittern*, from the pans at Joppa, near Portobello (three miles from Edinburgh), in which sea-water is concentrated so as to yield culinary salt, was precipitated by nitrate of baryta. The precipitate, after being washed and dried, was warmed with oil of vitrol in a lead basin, covered by waxed glass, with designs on it. The latter were etched in two hours, as deeply as they could have been by fluor-spar treated in the same way, the lines being filled up with the white silica, separated from the glass. To the acknowledged constituents of sea-water, fluorine, then, must now be added.

* As the fact of the frequent presence of fluorine in water must hereafter enter as an important element into all speculations as to the cause of the corroding action of water on glass, I place here on record the result of an accurate quantitative trial on the latter subject.

I am indebted to Mr JOHN ADIE for the particulars of the following experiment, which was made with a view to discover what peculiarity in the structure of glass unfits much of it for optical purposes. A cube of glass, two and a half inches square, was inclosed in a fir box, and fixed immovably in it by pieces of wood. Holes were pierced in the sides of the wooden case, so as to permit the free passage of the water, and the whole was placed in an engine-boiler, supplied with the Edinburgh pipe-water, and left there for six months. During that period the boiler was in action twelve hours each day; the water being under a pressure of 35 pounds on the square inch, and at a temperature of about 260° Fahr. The cube weighed, when first immersed, 9157 grains, and, when taken out, had lost 457 grs., or about $\frac{1}{20}$ th part of its weight. It is right to mention, that the condensed steam was returned to the boiler, so that fresh saline matter was only furnished in the water added, from time to time, to supply the waste.

† London Phil. Mag., No. 144, p. 14.

‡ On the Chemical Composition of Calcareous Corals, by B. SILLIMAN junior.—American Journal of Science, vol. i. Second Series.

This fact, besides its interest in relation to natural history, will be welcome to chemists, as adding another link to the lengthened chain of analogies between chlorine, bromine, iodine, and fluorine. In teaching the beautiful law that bodies closely allied in chemical characters occur together in nature, I have always felt the force of the argument weakened, by the absence of fluorine from sea-water where the other members of its class are so abundant. Its detection in bittern removes the difficulty, and adds another to the many relations which are common to the well-marked natural family of simple-salt radicals to which it belongs.

4. *Of the presence of Fluorine in Minerals.*

It remains to connect the initial fact of this paper with the occurrence of fluorine in minerals, and in plants, and animals; and, first, of minerals. I exclude, in the meanwhile from notice, fossil bones, which will be best considered in relation to animal remains.

The solvent power of water over fluoride of calcium is likely to throw some light both on the appearance and disappearance of that substance from particular localities, in relation to geological changes. In connection with this branch of our subject, Mr ROSE has reminded me of a phenomenon familiar to mineralogists, namely, the frequent occurrence of quartz with deep cubical impressions on its surface, believed to be casts of crystals of fluor-spar, which had been dissolved away after the deposition of the silica. It is possible that the markings may have been occasioned by galena or iron pyrites, the latter of which we know, in contact with air, can change into sulphate of iron and sulphuric acid, and is then quite soluble in water, and might be readily carried away. Cubical iron pyrites, however, in general is quite permanent, and suffers no change even with the freest exposure to air; and galena is an exceedingly insoluble substance. Mineralogists, accordingly, have universally agreed that the square impressions on quartz are the imprints of fluor, and the very frequent association in nature of the latter with silica, seems to justify their view. It has been a problem, however, what agent has removed the fluor; and it seems not impossible that water may have been the body which dissolved it away. In confirmation of this idea, I may refer to a paper by Mr ROBERT WERE FOX,* in which he describes certain pseudomorphous octohedrons of quartz, "more than an inch in diameter," which "were broken from a copper vein in *Killas*, at the depth of about 160 fathoms from the surface." The crystals were hollow, and many of them contained, hermetically enclosed within them, water, or rather an aqueous saline solution, and numerous pieces of fluor. "Of these," Mr FOX says, "all the fragments are corroded, and indicate, by their rounded edges and indented surfaces, the action of a solvent

* Transactions of the Royal Polytechnic Society of Cornwall, quoted in *Edin. Phil. Journal*, vol. xl., p. 115.

which penetrated most readily between the planes of cleavage." The contents of the different crystals were not alike, and, unfortunately, Mr Fox, doubtless from the belief that fluor-spar is insoluble in water, made no search for that substance in the liquid procured by breaking the hollow octohedrons. In several cases, however, it precipitated nitrate of baryta and oxalate of ammonia, an action referred to the presence of sulphate of lime, but which may, in part at least, have been owing to fluoride of calcium being present in solution. In the matrix of these crystals, alternate layers of quartz and fluor-spar were found in lines like fortification-agate. When we connect these facts with the solubility both of fluoride of calcium and of silica in water, and with the observed presence of fragments of the former in the hollow crystals, it will be acknowledged that water, if not the true, would at least be a sufficient cause of the phenomena described by Mr Fox, and of the still more familiar one of square impressions on quartz, previously referred to. I have tried the experiment of placing powdered fluor-spar on a filter, and percolating distilled water through it, and have found that the latter precipitated oxalate of ammonia. Whilst, however, I think it cannot be denied that water in contact with fluor-spar must round off the edges of its crystals and dissolve it away, we have no data from which to determine what effect salts held in solution by water may have in increasing or diminishing its solvent power.

It is necessary to mention here that fluorine is already known to occur in many minerals besides fluor-spar. It has been found in hornblende, as well as in mica, in apatite, wavellite, wagnerite, urinite, phosphorite, &c., along with phosphate of lime. BERZELIUS and ROSE* found it in sulphate of baryta; so did MIDDLETON. I have found it there also; I have likewise obtained it in one case from gypsum. It has been found in other sulphates, and MIDDLETON has frequently detected it in carbonate of lime. In general, it may be expected to occur along with the insoluble salts of baryta, strontia, and lime, and probably also with those of lead, especially when these are of aqueous origin. MIDDLETON's detection of fluoride of calcium in the shells of marine mollusca, and SILLIMAN's recent elaborate analysis of corals, which resulted in shewing that in nine different species (the only ones in which it was sought for) fluoride of calcium occurred, lead directly, as the latter gentleman has indicated, to the conclusion, that shell, coral, and metamorphic limestones, may be expected to contain that salt. When to these sources of fluorine we add animal remains, especially their bones and excretions, but in truth their whole mass, it will be manifest that in all parts of our globe, water may meet with fluorine, and carry it into the tissues of plants and animals. This remark leads directly to its occurrence in the two latter; and, first, of plants.

* Griffin's Rosé's Quantitative Chemistry, p. 348.

5. Of the presence of Fluorine in Plants.

Comparatively few examinations of plants have yet been made, in reference to the occurrence of fluorine in them ; but these, on the whole, have been satisfactory. SPRENGEL appears to have been the first to suggest the likelihood of its presence in vegetables, but failed in detecting it in any of them, and referred his failure "to its existing in such a state of combination as caused it to be dissipated by the heat necessary for expelling the carbonaceous matter, so that it could not be detected in the ordinary method."*

Dr DAUBENY "ascertained that no sensible action is exerted on glass by heating, with sulphuric acid, the earthy phosphates present, in twelve pounds of barley."† I was equally unsuccessful with the ashes of Kanaster tobacco, and of peas, and with those of charcoal and of coal. I ascribe the failure, however, not to fluorine existing in a peculiar state of combination in plants, but to the presence of silica, which, when in any quantity, makes the detection of fluorine very difficult. I took no measures to separate the silica in the few experiments I made on the subject, and SPRENGEL and DAUBENY appear to have omitted the same essential preliminary. Dr WILL of Giessen, who kept this point carefully in view, states that "careful experiments, conducted under his own superintendence, by Messrs JAMES MÜLLER and BLAKE, severally, have shewn that the ashes of French barley, grown in Switzerland, contain very distinct traces of it ; both straw and grain were employed."‡

To WILL the credit of first finding fluorine in plants is entirely due. As barley, however, contains a large amount of calcareous phosphates, which the fluoride of calcium has been supposed to accompany in a peculiar state of combination, I thought it well to examine the ashes of a plant containing little phosphate of lime, and which might be considered as having derived any fluorine it contained directly from the water its roots absorbed. I chose for this purpose the crudest American potashes, which, as they are obtained in part by burning the young and succulent branches of trees, should contain portions of all that is soluble in the sap. A pound weight of ashes was supersaturated with hydrochloric acid, the liquid poured off, neutralised with ammonia, and precipitated by nitrate of baryta. The precipitate washed and dried, when treated with Nordhausen sulphuric acid in a lead basin, in the way already described, etched glass distinctly.

Whether the fluorine found in plants is essential to them, and serves some purpose in their organization, or merely circulates in their sap, as other soluble matters do, without being appropriated by the living organism, cannot be deter-

* Chemical Society's Memoirs, vol. ii., p. 103.

† Ibid.

‡ Ibid, p. 182.

mined in the present state of our knowledge. But fluorine may be expected to occur in the fluids of all the higher plants. On this subject WILL observes, "fluorine occurs in the teeth and bones of animals, having been derived by them from vegetable food; it will doubtless, therefore, exist still more abundantly in the ashes of plants."* This expectation was probably founded on the supposed insolubility of fluoride of calcium in water, and is not likely to be fulfilled. Animals which may derive fluorine both from their solid food, whether animal or vegetable, and likewise from the water they drink, are likely to excel plants in the proportion of fluorine they contain.

6. *Of the presence of Fluorine in Animals.*

As there exists, then, a twofold source of fluorine for animals, we may anticipate its occurrence in various parts of their structures. Passing by for the moment, as a disputed point, the occurrence of fluoride of calcium in recent bones, and excluding the consideration of its presence in shells and corals, it may be noticed that the urine of man is the only animal product in which fluorine has been quite certainly ascertained to occur. GAY LUSSAC appears to have been the first who suggested the probability of fluorine being found in the secretion of the kidneys,† but he did not make any experiments on the subject. With the precipitate obtained by adding lime water to human urine, BERZELIUS etched glass distinctly; and from the period of his experiments, fluorine has been ranked among the normal, or at least the occasional, ingredients of the fluid in question. The fact, however, has always been referred to with a kind of hesitation, and had it rested on the authority of any less distinguished chemist than the great Swedish one, it would have dropped, I fear, out of notice altogether.‡

REES repeated BERZELIUS' experiment, but quite unsuccessfully, and, as in the case of bones, affirms that fluorine cannot be found in urine. I made but one trial on the subject, but it was so decisively confirmatory of BERZELIUS' original result, that it seemed unnecessary to repeat it. About 50 ounces of urine (*urina potus*), were precipitated by nitrate of baryta, which, for reasons already fully detailed, is preferable to lime, as forming a less soluble salt with fluorine. The precipitate, consisting chiefly of sulphates and phosphates, was collected on a filter, and dried without washing. Warmed with Nordhausen acid, it corroded glass deeply, the lines being filled with white silica, exactly as if fluor-spar had been used. In none of my experiments, except with the dissolved fluor, and with sea-water, has the etching been so distinct as it was in this case. I recommend those who wish to succeed in this experiment to employ what the older physiologists distinguished as *urina potus*. In districts where fluorine is not abundant in the soil or waters, it may not so frequently

* Chemical Society's Memoirs, vol. ii. p. 182.

† Ann. de Ch., t. lv.

‡ SIMON'S Animal Chemistry, vol. ii.

occur in the bodies of animals, as it does in those of the inhabitants of other localities. The experiment in question was made in Edinburgh.

It could not be doubted, after the facts I have detailed, that fluorine would be found in the two great formative liquids of the animal body, blood and milk; I have found it in both. So far as I am aware, it has hitherto been overlooked in all the analyses that have been made of these liquids; probably it has not been sought for.

I employed the blood of the ox, and in two cases obtained markings on glass, which only become visible when breathed upon, but are then quite manifest. In the third the glass was distinctly, though faintly, corroded.

That others may know exactly how the experiment was made, I may mention, that in the most successful case, about 128 ounces of blood were taken, which had been freed, by stirring, from much of its fibrine, and was boiled till the liquid solidified. The broken coagulum was burned in a large crucible, the ashes boiled with dilute muriatic acid, and the liquid filtered and evaporated to dryness. The residue was then heated to redness to expel chloride of iron, and afterwards washed with a small quantity of water, sufficient only to remove the accompanying chloride of sodium. The insoluble matter, which was in brilliant metallic crystalline scales, was reduced to fine powder, and heated with Nordhausen sulphuric acid, in a basin covered by waxed plate-glass. As the experiment performed in this way gave the fluorine only or chiefly of the serum, we may expect a still more decisive result to be obtained when the whole blood is taken. But it is so difficult to burn the fibrine of blood, that I was content, in a first trial, to experiment chiefly with the serum. As already mentioned, where the entire blood was made use of, glass was marked, but not corroded. Both of the trials, however, with the entire blood, were conducted so as to involve many washings, which were avoided in the third and most successful experiment.

In examining milk, I thought to have saved myself the trouble of boiling down large quantities of that liquid by using cheese, which was burned, and the ashes digested in muriatic acid. The filtered solution was supersaturated with ammonia, and the precipitate which fell (consisting chiefly of phosphates), washed, dried, and tested with Nordhausen acid and glass. In three cases I totally failed to detect fluorine whilst operating in this way. I was not more successful with two quantities of milk treated similarly. I mention these failures to shew the necessity of avoiding methods which imply much washing upon filters, when fluoride of calcium is sought for. The large quantity of liquid made use of in the course of the process referred to, may have held in solution and carried away all the fluoride present; especially as the addition of ammonia could only separate fluoride of calcium previously dissolved by an acid. In the last and successful attempt to detect fluorine in milk, the hydrochloric solution was neutralised with ammonia and precipitated by nitrate of baryta. The precipitate, which was

slightly washed, etched quite distinctly. A still better process would be that followed with the blood, where washing was reduced to a minimum. These experiments throw no light on the condition in which fluorine exists in blood or milk; nor would it be easy to ascertain in what state of combination it occurs. In the meanwhile we may suppose it to be present as fluoride of calcium.

There is one other animal fluid, the last towards which suspicion as to its containing fluorine was likely to have been entertained, in which hydrofluoric acid has been declared to occur, namely, the gastric juice. BRUGNATELLI states that fragments of rock-crystal and agate inclosed in tubes, and introduced into the stomachs of hens and turkeys, were found in ten days to have lost 10 or 12 grains in weight. TREVIRANUS, also, is said to have found that chyme from hens' stomachs corroded porcelain capsules. These experiments are thought to prove the presence of free hydrofluoric acid in the gastric juice. TIEDEMANN and GMELIN placed the gastric juice of ducks in platina crucibles covered with plates of glass, having lines traced on them through a coating of wax; but found no action on the glass after 24 hours digestion with the aid of heat.* That hydrofluoric acid exists in the gastric juice of birds, in such quantity as to round off pebbles with the rapidity implied by BRUGNATELLI's observations, is not probable; but the force which is able to evolve hydrochloric acid at the stomach from the chlorides which enter the system, is probably adequate, in similar circumstances, to convert fluorides, along with the elements of water, into hydrofluoric acid. We may now look for fluorine in all the animal fluids.

7. *Of the presence of Fluorine in Fossil Bones, and its relation to Animal Life.*

The facts detailed in the preceding sections remove much of the difficulty that has attended previous speculations as to the source of the fluorine found in fossil bones. It will now be conceded, that water must be constantly conveying that element into the organs of animals, whilst other portions are also added in their solid food. Whether this fluorine be supposed simply to travel through the organism, dissolved in the circulating fluid as fluoride of calcium, or as some other salt, and to quit the body as it entered it, without serving any purpose therein; or be imagined to fulfil some important end in relation to the functions of life, it must be expected to shew itself as a very frequent, if not constant, ingredient in the bones. We may quite safely infer that a portion, at least, of the fluorine found in fossil osseous remains must have been present in them when they were parts of a living structure; and when we find only two per cent. of fluoride of calcium in many ancient bones, while certain recent ones contain nine per cent., it may well be doubted whether a trace of the salt has been added during fossilization. Nay, it is a question I think worthy of discussion, whether there may not

* These speculations will be found noticed in BERZELIUS' *Traité de Chimie*, or in SIMON's *Animal Chemistry*, vol. ii., art. Gastric Juice.

have been a decrease in the proportion of fluoride, water having washed it out, and dissolved it away. We naturally turn for light on this problem to the consideration of the question, What proportion of fluorine is found in the bones of existing animals? Here, however, we are at once met, as has been already stated, by the most contradictory declarations on the part of able chemists; some affirming that fluorine is found in all bones; others, that none can be detected in any.

When it is considered how extremely simple the process of testing for fluorine is, and how little room there is for difference in manipulative dexterity affecting the result, I am constrained to admit, that, in the meanwhile at least, we must refuse to fluorine the character of being a constant ingredient of bones. On the other hand, it is certainly a very common one, more frequently present than absent; and we may encourage the expectation, that future researches will explain satisfactorily the cause of failure where negative results have been obtained, and prove fluorine to be an ever-present constituent, not only of bones, but of other animal tissues.*

The suggestion, accordingly, of LIEBIG, that fossil bones contain only the fluorine which was added to them whilst parts of living structures, may be found

* Certain of the recent observers have endeavoured to reconcile the conflicting statements of their predecessors in reference to this subject, but with little success. Dr DAUBENY conceives that the failures may have arisen, in part, from the bones examined not having been deprived of their gelatine before being tested for fluorine, so that the animal matter prevented the hydrofluoric acid from acting on the glass.* But on the one hand, FOURCROY and VAUQUELIN, who were unsuccessful searchers for the element in question, pointed out long ago the necessity of burning away the gelatine as a preliminary step, and always did so before looking for fluorine.† On the other hand, Mr MIDDLETON, who found that substance abundantly in bones, simply broke the latter into small fragments, and heated them with concentrated sulphuric acid. He states, moreover, that the time occupied by each experiment was only between five and ten minutes.‡ Dr DAUBENY refers likewise to the presence of salts of volatile acids and salt radicals, such as chlorides and carbonates, from which hydrochloric and carbonic acids are evolved when sulphuric acid is poured on the bones, and which sweep away the hydrofluoric acid before it has time to corrode the glass. He has accordingly described a method of procedure which gets rid of the volatile bodies in question, but only at the risk of losing, in the liberal washings prescribed, much of the fluoride of calcium. It cannot be doubted that the acids which accompany the hydrofluoric, when sulphuric acid is poured upon burned bones, dilute and carry away the body sought for. But FOURCROY and VAUQUELIN's experiments, which were made by distilling bones with sulphuric acid in glass vessels, could have been but little affected by this source of fallacy; and BERZELIUS' successful results were obtained in the very same way.

After trying DAUBENY's process several times, I am constrained to acknowledge that I did not find it give any better results than the simpler one previously in use. If it be deemed requisite to get rid of the carbonic acid of bones before testing for fluorine, I believe it could be done most efficaciously by digesting them, after being burned and reduced to powder, in a solution of tartaric acid, and, after the whole carbonic acid had been expelled, drying up the mass.

REES, taking the opposite view from DAUBENY, has endeavoured to prove that, where fluorine has been supposed to be present, it was in reality phosphoric acid that corroded the glass.§ He acknowledges, however, that this explanation applies only to those cases where the bones were distilled with sulphuric acid, and the product of distillation evaporated to dryness in glass vessels. He regards as unexceptionable, experiments made with platina crucibles covered with waxed glass; and as MIDDLETON's, DAUBENY's, and my own trials were made in this way, and distinct corrosion or etching obtained, his suggestion must be considered as leaving the subject where it found it.

* Chemical Society's Memoirs, vol. ii., p. 100.

† Memoirs of Chemical Society, vol. ii., p. 135.

‡ Annales de Chimie, tome lvii., p. 38.

§ Edin. Phil. Journal, vol. xxviii., p. 93.

quite sufficient in many cases, especially if we add to the food, the drink of animals as a source of the ingredient in question. But when we find fossilized bones containing ten or fifteen per cent. of fluoride of calcium, whilst, at the same time, they have lost to a great extent their original structure, and have acquired a crystalline or mineralized one, it seems highly probable that Mr MIDDLETON's belief that water may have infiltrated that salt into them, will prove worthy of adoption. I cannot, however, agree with him in thinking, that it is enough to shew that water may bring fluoride of calcium to bones, to account for its accumulation in them. Water, as my own experiments prove, may carry away fluoride of calcium from osseous remains, as well as transport it to them. We require to account for its detention in bones, as well as for its conveyance to them. From an experiment made in the laboratory, as well as from their association in nature, I am inclined to think that there is a double phosphate of lime and fluoride of calcium, much less soluble than the latter salt is; and that the production of this compound fixes the fluoride, and prevents its abstraction by water. Further researches will decide this point. Till quantitative analyses of a considerable number both of recent and fossil bones are made, as to the proportion of fluoride of calcium in them, it will be impossible to decide how far individual fossils which contain that salt are to be looked upon as coming under LIEBIG's or MIDDLETON's explanation, or as requiring, as many probably will do, a reference to both. Some, as already stated, may appear to have lost, instead of gaining, fluoride of calcium, during their entombment.

Allusion was made, in the commencement of the paper, to the possibility of a conversion of fluoride of calcium into phosphate of lime having occurred. Few would more gladly see the idea of elemental transmutation realized by natural phenomenon or laboratory experiment than I should do. I can find nothing, however, to support it in the phenomena I have been discussing.

In conclusion, I would observe, that physiologists will doubtless now be tempted to speculate on the possibility of fluorine performing some essential function in living animals. Its occasional absence from their bones would not disprove that it may be necessary for the perfection of certain organs, though not for all. Quantitative analyses appear already to have indicated that the enamel of teeth contains more fluorine than any other part of the body. If that result shall be confirmed, we may suppose that, if fluorine be furnished when the development of the teeth is proceeding, it may be wanting at other periods, without injury to the animal; just as chloride of sodium must be considered as essential to the healthy life of most creatures, though they may be deprived of it for long intervals, without death ensuing.

The small quantity of fluorine found in living structures can be counted no argument against its occasional or constant importance. Quantity, is at best, but a rude measure of the value of an ingredient, in relation to the necessities

of an organism. The law of final causes, in truth, would indicate that *only* a minute proportion of fluorine should occur in any organ ; for it would be perilous to an animal to introduce into its system a large quantity of fluorides, which can so readily be changed into the deadly hydrofluoric acid. Such speculations, however, are premature. It will be time enough, when many qualitative, but especially quantitative, researches have been prosecuted, as to the presence of fluorine in animal structures, to consider of what service it is to them ; if it be of any.

EDINBURGH, *April* 4. 1846.

XVII.—*Observations on the Principle of Vital Affinity, as illustrated by recent discoveries in Organic Chemistry.* By WILLIAM PULTENEY ALISON, M.D., F.R.S.E., Professor of the Practice of Medicine in the University of Edinburgh.

(Read, 2d February 1846.)

PART I.

THE most important steps in a science are those which lead most directly to the establishment of principles or laws peculiar to that science itself, and which constitute its claim to be regarded as a distinct branch of human knowledge. It has been long acknowledged that such is the character of many of those phenomena of living bodies which depend on mechanical movements, or changes of position in their particles, and therefore that the laws of vital contractions are to be regarded as equally elementary and distinctive principles in physiology, as the laws of motion or of gravitation in natural philosophy. But a difficulty has been long felt, as to whether a similar claim to peculiarity of the principle on which they depend, can be urged for the chemical phenomena of living bodies.

In laying down the first principles of Physiology and of Pathology, I have, however, uniformly maintained the existence of a power peculiar to living bodies, and to which the term *Vital Affinity*, as recommended by several authors, may be properly applied;—a power by which “the elements of nutritious matter are thrown into the combinations necessary for forming the organic compounds, and restrained from entering into other combinations, to which they are prone as soon as life is extinct;—a power which supersedes and counteracts ordinary chemical affinities in living bodies, as completely as vital contractions counteract gravitation or the inertia of matter.”—(*Outlines of Human Physiology*, p. 22.) And in delivering lectures on physiology, I always expressed my belief that a time would come, when discoveries in the chemical department of the science,—connecting the ingesta of living bodies with the nourishment of their different textures, and with the nature of the different excretions,—would elucidate the chemical changes which are continually going on in them, and are essential to their living state, as completely as the discovery of the circulation of the blood illustrated many of the conditions of the existence of living animals. It appears to me that this anticipation has been more nearly realized by recent chemical observations, than professed physiologists have yet admitted;—that not only the existence of the principle of vital affinity has been established, but its limits and mode of

action, the cases in which it acts, and those in which it is unconcerned, are to a certain degree defined;—and that a short and general illustration of these points may be of some advantage, if not to the progress of the science, at least to the due appreciation, and proper generalization and expression of the knowledge which has been already acquired.

To shew the importance of this inquiry, I need do no more than quote a single sentence from CUVIER, with a statement which is nearly a commentary upon it by Professor WHEWELL. “It belongs to modern times to form a just classification of the vital phenomena; and upon the zeal and activity given to the task of analysing the *forces* which belong to each organic element, depends, according to my judgment, the advancement of physiology.”* “As the vital functions became better understood, it was seen more and more clearly at what precise points of the process it was necessary to assume a peculiar vital energy, and what sort of properties this energy must be conceived to possess. It was perceived when, and in what manner and degree, mechanical and chemical agencies were modified, overruled, or counteracted by agencies *which must be hyper-mechanical and hyper-chemical*.” “In attempts to obtain clear and scientific ideas of the vital forces, we have first to seek to understand the cause of change and motion in each function, so as to see at what points of the process *peculiar causes* come into play; and next, to endeavour to obtain some insight into the *peculiar character and attributes of these causes*.”†

When we say that the chemical changes which take place in living bodies are elucidated, we mean, of course, that they are referred to general laws, by which the phenomena observed in this department of Nature are found, by experience, to be regulated. And when we say that these are laws of vitality or of vital action, we mean merely, that they are laws deduced from the observation of phenomena peculiar to the state of life,—taking for granted that it is always possible to describe, and practically to distinguish, those substances which we call living, from inorganic or dead matter; and that the only correct definition of vital principles or vital powers, is, that they are the laws or the powers which regulate the phenomena that are peculiar to the state of life. They are the general expression of the results of the observation, and generalization of the facts, which are observed in this department of nature, and which are ascertained to belong to this department alone.

We are not, indeed, justified in asserting the existence of laws peculiar to the state of life, merely by the *negative* observation, that the phenomena referred to them are *inexplicable* by any known laws of inorganic or dead matter; we must have the *positive* observation that they are *inconsistent with*—that they take place

* Hist. des Sciences Naturelles depuis 1789, p. 218.

† Philosophy of the Inductive Sciences, vol. ii., pp. 39 and 47.

in despite of—the laws which regulate the changes of dead matter. It is thus that we are led to ascribe the visible movements of living bodies to vital powers; not because we do not perceive how gravitation, elasticity, or any other known causes of movement in dead matter should produce them, but because we do perceive, that, in the circumstances in which we see these motions, all those principles, deduced from the observation of dead matter, would determine either rest, or motion in a different direction from that which really takes place.

I formerly laid before this Society the grounds of an opinion, then much disputed, but now, I think, pretty generally admitted, that there are Attractions and Repulsions, as well as contractions, peculiar to the living state: chiefly, but not exclusively, observed at those parts where chemical changes are effected in living bodies, and connected with these changes; and, without reference to this general fact, I maintain that it is impossible to have a right understanding of many phenomena of essential importance in physiology and pathology.*

But the general principle is obviously equally applicable to chemical changes as to mechanical movements. It is not, indeed, so easy to ascertain, in regard to chemical changes in living bodies, that they are truly inconsistent with the chemistry of dead matter; the science must be allowed to make some progress before this can be confidently asserted in regard to any individual chemical change; but no one can doubt that, as science advances, it must become possible to say with certainty, whether the chemical changes in living bodies are consistent with those laws which regulate chemical changes elsewhere, or not; *i. e.*, whether the same chemical elements can be so brought together by the chemist, as to tend to the same combinations as are found in living bodies; or whether, in his hands, they will enter uniformly into other combinations, and form different compounds.

Farther, it appears to me that, even before any of the recent discoveries, it might be legitimately inferred from facts already known, that this last description is truly applicable, in some cases, to the chemistry of living bodies. It was known, for example, that when water, impregnated with carbonic acid and with a small proportion of ammonia, is brought into contact with vegetable substances, in a certain stage of their existence, the elements of these bodies rapidly combine so as to form starch, albumen, and oil, which are added to the substance of the vegetables,—that under no other circumstances can water, carbonic acid, and ammonia, or their elements, be made to form these compounds,—and farther, that after a time, when brought into contact, at the same temperature, with the same vegetable substance in an ulterior stage of its existence, they will form no such com-

* Professor WHEWELL, in his instructive abstract of the general principles ascertained in Physiology, regards it as established, chiefly on the authority of MÜLLER, in regard to the vital force concerned in assimilation and secretion, that “it has mechanical efficacy, producing motions, &c. But it exerts at the same point both an attraction and a repulsion, attracting matter on one side and repelling it on the other; and in this it differs entirely from mechanical forces.”—*Philosophy of Inductive Sciences*, vol. ii., p. 51. See also CARPENTER'S *Manual of Physiology*, § 597, *et seq.*

pounds, but will aid and participate in the successive changes to which vegetable matter is liable after the phenomena of its living state are over, and of which the ultimate result is, the resolution of that matter into its original constituents. And from these facts it seems quite reasonable to infer, that during the former, or what we call the living state of the vegetable, certain affinities peculiar to the living state—*i. e.*, certain vital affinities—actuate the elements of which it is composed.

In asserting the existence of vital affinities, we do not, in the first instance, give any opinion whether it is by the addition of certain chemical attractions, or by the suspension of others, during the living state, that the chemical changes peculiar to that state are effected; we assert nothing more than what is, as I think, correctly stated in the following sentence of LIEBIG:—"The chemical forces in living bodies are subject to the invisible cause by which the forms of organs are produced." "The chemical forces are subordinate to this cause of life, just as they are to electricity, heat, mechanical motion, and friction. By the influence of the latter forces, they suffer changes in their direction, an increase or diminution of their intensity, or *a complete cessation or reversal of their action.*

"Such an influence, and no other, is exercised by the vital principle over the chemical forces."

"The equilibrium in the chemical attractions of the constituents of the food is disturbed by the vital principle, as we know it may be by many other causes. The union of its elements, so as to produce new combinations and forms, indicates the presence of a *peculiar mode of attraction*, and the existence of a power distinct from all other powers of nature, *viz.*, the vital principle."—(*Organic Chemistry*, &c., pp. 355, 357).

In these passages I think that LIEBIG has expressed himself with perfect accuracy; but in other parts of his writings he uses language in regard to the nature and results of chemical changes in living bodies, which seems to me vague and speculative, and even inconsistent with what he had stated in the passages just quoted, *e. g.*, when he says that "the ultimate *causes* of the different conditions of the vital force in nutrition, reproduction, muscular motion, &c., are *chemical forces.*"—(*Organic Chemistry*, p. 10).

The following sentence by MÜLLER expresses the very same idea, although it might be thought, from the manner in which this author expresses himself against any introduction of the vital principle in this department of physiology, that he considers all the chemical changes in living structures to be referable to the same laws as in inorganic matter.

"By a small organ of a plant *a force is exercised*, exciting forces which slumbered in the carbon, oxygen, and hydrogen, or rather *modifying the forces which existed in these*, so that 12 equivalents of carbon unite with 10 of hydrogen and 10 of oxygen; and from 12 equivalents of carbonic acid (12C O_2) and 10 of water

(10 H O) starch is produced, 12 C 10 H 10 O, 24 of oxygen passing off."—(*Chemistry of Vegetable and Animal Physiology*, p. 67).*

But it is important to fix our attention, for a short time, on the instances adduced by MÜLDER, of the formation of starch, or some of its allied compounds, out of carbonic acid and water, by the combination of the carbon of the acid with the elements of water, and the expulsion of the oxygen of the acid ; because this is the grand and fundamental power, which must have been called into operation when organized structures were first created on earth, and on the continued exercise of which the existence of all such structures, vegetable and animal, is still essentially dependent ; and because the simplicity of the process makes it a fit case for considering the question, whether the power here named is strictly entitled to the epithet vital ; or whether, as some eminent physiologists in this country maintain, the idea expressed by that term is incorrect and unscientific.

The opinion of those who oppose the doctrine of vital affinity, is thus distinctly stated in the *Anatomy* of Drs QUAIN and SHARPEY :

"Although the products of chemical changes in living bodies for the most part differ from those appearing in the inorganic world, the difference is nevertheless to be ascribed, not to a peculiar or exclusively vital affinity different from ordinary chemical affinity, but to common chemical affinity, operating in circumstances or conditions which present themselves in living bodies only ; and undoubtedly the progress of chemistry is daily adding to the probability of this view."

I consider this to be a hasty and ill advised statement ; and to shew this, I request attention, *first*, to the perfect simplicity of the apparatus by which this change is effected. "In all plants," says MÜLDER, "there exists a small organ, of the most simple form, although employed by nature for the most varied purposes. It is a small filmy sac, a thin membrane, which encloses a small space, which it enables to communicate with the exterior space through invisible pores. These little sacs or cells are the chief organs of plants. A countless multitude of them, grouped together, forms the whole bulk of the plant, so that if every thing except the cells be destroyed, the shape and size of the plant are not in the least changed or diminished."

Into this simple apparatus in certain parts of plants, water, impregnated with carbonic acid, is introduced, while the plants exhibit the phenomena of life ; and

* In the foregoing and other translations from recent German writers, the word *force* is used in a sense which I think would be much better expressed by the term *power* or *property*, merely on this account, that the English word *force*, in physical discussions, has usually a precise and limited meaning assigned to it, as a cause capable of producing visible motion, and of which we have a measure, either in the velocity or in the quantity of motion which it can excite ; whereas the term *power* or *property*, applied to any material substance, has a more general meaning, as simply the cause of change of any kind, and is therefore applicable where the result of the property ascribed to any substance may be very different from visible motion.

let us next observe the intensity of the action by which the carbonic acid is there decomposed, the carbon attached to the elements of the water, and the oxygen set free. "This is done by a power," says LIEBIG, "to which the strongest chemical action cannot be compared. The best idea of it may be formed by considering, that it surpasses in power the strongest galvanic battery, by which we are not able to separate the oxygen from carbonic acid. The affinity of chlorine for hydrogen, and its power to decompose water, under the influence of light, and set its oxygen at liberty, cannot be considered as nearly equalling the power and energy with which a leaf, separated from a plant, decomposes the carbonic acid which it absorbs."—*Organic Chemistry*, p. 134.

Next let us observe the extent to which this energetic power is exercised by living plants. Perhaps the most accurate idea of it may be formed from attending to the statement of THEODORE DE SAUSSURE, that on a mean of 54 observations made in a country district, the proportion of carbonic acid in the atmosphere during the night was to its proportion in the day-time as 432 to 398, *i. e.*, the carbonic acid existing in the atmosphere was found to be diminished very nearly 10 per cent. in a few hours of every day; and for this diminution we know no cause, except that this power of the green parts of vegetables, of decomposing the carbonic acid of the atmosphere, is exercised only under the influence of light.*

Now if a power of this extraordinary energy and extensive operation, and acting in so very simple a manner, were really to be regarded as depending only on ordinary chemical affinities, exerted under peculiar conditions, it might surely be expected, that the chemist might so regulate the conditions under which he might bring together carbonic acid, air, and water, as to exhibit some traces of this power, and effect some decomposition of the carbonic acid and evolution of oxygen. But we know, not only that this cannot be done, but that when air, water, and carbonic acid, are introduced into the very same vegetable cells, within half an hour after they have exhibited this phenomenon, at the same spot, under the same light, and at the same temperature, they will not only fail to exhibit the same change, but will uniformly exhibit the very reverse, *i. e.*, the absorption of oxygen and the formation and evolution of carbonic acid.

Nay, we know that it is only in certain cells of the living vegetable, that this peculiar chemical change, under the action of light, is effected; the same fluid, introduced into cells composed of the same material in the parts of fructification, undergoes no such change; but, on the contrary, gives occasion only to the reverse process, the absorption of oxygen and evolution of carbonic acid.†

Then it is to be remembered, that this complete inversion of ordinary chemical affinities, in the case of the living plant, is only one of several cases to

* See MACAIRE'S Memoir of THEODORE DE SAUSSURE, in *Edinburgh Philosophical Journal*, vol. xl. p. 31. (Jan. 1846.)

† THEODORE DE SAUSSURE, in *Edinburgh Philosophical Journal*, vol. xl. pp. 22, 23.

be afterwards noticed, where we see chemical compounds uniformly formed in living bodies, quite distinct from any that can be formed by the chemist from the same elements, and quite distinct from those to which the same elements uniformly revert, after the phenomena of life are over.

Lastly, we must remember, when we see this apparent inversion or alteration of the ordinary chemical relations of matter, taking place in the interior of living bodies, that in that scene, by the admission of all, matter comes under the dominion of *mechanical* laws, which operate in no other department of nature; so that it is quite conformable to analogy to suppose that its chemical relations will undergo a similar modification.

When all these considerations are duly weighed, I cannot perceive what further evidence can be required in order to justify the expression which I have quoted from LIEBIG, viz., that the "*new combinations*," as well as the forms, assumed by that matter which goes to the composition of organized beings, "indicate the existence of a power distinct from all other powers of nature, viz., the vital principle;" *i. e.*, that the vital principle regulates the changes of chemical composition, as well as the changes of position which the particles of that matter undergo; which is more simply expressed by saying, that there are vital affinities as well as vital contractions and attractions.

But even if we are to regard it as doubtful whether or not ordinary chemical affinities can determine, under any conditions, this decomposition of carbonic acid and evolution of oxygen by its contact with carbon and the elements of water, I maintain that it is sound philosophy, when we see this and other rapid and extensive and important chemical changes, essentially different from those which the same elements present under other circumstances, uniformly attending the phenomena of life in vegetables,—to investigate and generalize the laws by which these changes are regulated, as laws of living action, leaving it open to future inquirers, if they can, to resolve them into other laws of more general application. For although I acknowledge the force of the aphorism, "*Frustra fit per plura quod potest fieri per pauciora*," still I apprehend, that in every case to which this aphorism is applied, the *potest fieri* must be established, not by conjecture, but by experiment; otherwise we fall into the error, so strongly condemned by BACON and others, of prematurely generalizing, and supposing the laws of nature to be fewer and more comprehensive than they really are.

Having thus, in reference to this first and simplest example, vindicated the soundness of the principle which I propose to illustrate, I think we may next shew, that the main object of inquiry in the chemical department of physiology is more simple and precise, and the extent of that inquiry, necessary to elucidate most questions in physiology, much less than might be supposed from the multiplicity of details, of which what is called the science of organic chemistry is made

up. After what LIEBIG calls the "peculiar mode of attraction" which operates in living bodies, has led to the formation of certain organic compounds, these compounds lose their connection with living bodies, become liable to an infinite number of changes and decompositions, and thus give origin to an infinite variety of substances—generally of temporary duration only, because retained in their form by attractions of no great intensity—applicable to many useful purposes, *but foreign to the inquiries of the physiologist*. He is concerned only with the chemical changes which take place *in living bodies themselves, and during the state of life*; and the results of recent inquiries seem to me sufficient to shew, that the fundamental and peculiar arrangements of chemical elements there observed are less numerous, and the laws regulating them more simple, than they have usually been thought.

In considering this subject, we are enabled, by the results of the inquiries of geologists and physiologists, to revert to the period of the introduction of living bodies into the world, and reflect on the conditions then assigned for their existence. We are justified, by reason, in allowing the imagination to fall back on the time when this Earth rolled through space an inanimate mass; and if any minds, besides that of the Great Ruler of the universe, were connected with it, they did not hold their connection through the medium of any organized structure. For I believe we are justified in laying down these propositions as established, *first*, That the simply physical arrangements of this globe were completed before any organised beings were created; *secondly*, That vegetables were created and lived chiefly on the atmosphere, fixing large quantities of carbon from it on the earth's surface, before animals were called into existence; and, *thirdly*, That at whatever time their existence began, either the first living being of every species, vegetable and animal, or the first ovum from which that being was developed, must have been formed in a manner wholly different from that in which any living bodies, at least of the higher orders, are now reproduced; *i. e.*, that they must have been formed in a manner strictly miraculous, and, of course, beyond the limits of physical science.

But although we cannot ascend higher, in prosecuting this subject, than to inquire in what manner the first plants, or the germs of the first plants, were enabled so to act on the inorganic matter around them as to extract from it the materials, first of their own growth and sustentation, and afterwards of all other organized beings,—yet in the inquiry, thus limited, important progress has been made. From the time when these nascent organized bodies sprung into existence, we must regard it as an ultimate fact, that they were endowed with the power, which all the vegetables that have succeeded them have exercised, of so modifying the attractions existing among the particles of matter, as to cause many of these particles from the air and the water immediately surrounding them, to enter into their substance, by their roots and leaves, or by the organs which soon

became their roots and leaves, and then to arrange themselves there, in those peculiar forms by which the numberless species of the vegetable world are characterized. I apprehend we must also regard it as an ultimate fact, that they were endowed with the power of so modifying the chemical relations of the elements composing those absorbed matters, as to select and retain certain of these elements, and allow others to pass away from them, to decompose the carbonic acid, fix the carbon, and invest it with those peculiar affinities for the water, the hydrogen of the water, and a few other elements, contained in the surrounding media, by which all the proximate principles, first of vegetables and then of animals, and therefore the whole substance of organized beings, are formed.

But it is important to have a precise exposition, although not an explanation, of the power thus exercised by the first plants; and it is still more important and satisfactory to be able to shew how, by the exercise of these and analogous vital powers, the atmosphere must have been gradually changed, the proportion of carbonic acid in it diminished, and the proportion of oxygen increased; how it became fitted, and is kept fitted, for the residence, first of cold-blooded and then of warm-blooded animals; how most of the other conditions of existence of these animals have been, and still are, continually prepared for them by these living actions of vegetables; how all the variety of the textures of all organized bodies, from the origin of vegetables to the death and decomposition of animals, are continually formed and maintained; and how, in both divisions of organized beings, Nature has provided, not for the permanent existence, but for the development and decay of successive generations of individuals, and thus for the perpetuation of the species. These are the subjects of investigation in the chemical department of physiology; and if it can be shewn, that, by a few simple laws, regulating what we call vital attractions and affinities, *i. e.*, modifying, in organized bodies, the attractions and affinities to which matter is everywhere liable, provision is made for all this succession and continual renewal of the phenomena of life; then, although we cannot explain the introduction of living beings into the world, any more than we can explain the dissemination of the stars throughout space,—although we must always regard the appearance of organized bodies on the earth's surface as the clearest indication which human knowledge presents of the subjection of the universe, not only to general laws, but to an arbitrary Will, superior to these laws and changing them at pleasure,—yet I think it may be said that we have nearly as clear an insight into the designs and arrangements of Providence for the maintenance of living beings upon earth, and for the eternal reproduction of them there, so long as these laws shall be in force, as we have into those by which the movements of the heavenly bodies are directed and controlled.

I. Our first business is to study the facts that have been ascertained in regard to the simplest form of chemical change to which the term vital may be applied,

which is merely the *selection*, by a portion of a living structure, of some one substance existing in a fluid, and the consequent attraction of this to a particular part of the structure, while other materials, equally presented to that living part, are excluded.

We need not here enter into the question, on which chemists and agriculturists are not yet agreed, whether the nourishment of plants, in the present condition of the earth's surface, does or does not require the pre-existence, in the soil, of organic compounds, resulting from previous living beings, which are absorbed from it. But we may justly give the name of vital attraction or affinity to that power by which certain saline matters, dissolved in the compound fluid which is absorbed, are retained in the substance of the plant, while others are returned to the soil. "The experiments of MACAIRE PRINCEP," says LEIBIG, "have shewn that plants, made to vegetate with their roots, first in a solution of acetate of lead, and then in rain-water, give back to the latter all the salt of lead which they had previously absorbed. Again, when a plant, freely exposed to the air, rain, and light, is sprinkled with a solution of nitrate of strontian, the salt is absorbed, but is again separated by the roots, and removed farther from them by every shower of rain, so that at last not a trace of it is to be found in the plant. A fir-tree, the ashes of which were analysed by a most accurate chemist, grew in Norway, on a soil to which common salt was conveyed in great quantity by rain-water. How did it happen that its ashes contained no appreciable quantity of salt, although we are certain that its roots must have absorbed it after every shower? We can explain this only by the observations above referred to, which have shewn that plants return to the soil all substances unnecessary to their own existence; and we are thus led to the conclusion that the alkaline bases, existing in the ashes of plants, must be necessary to their growth, since, if this were not the case, they would not be retained." (Ib. p. 103, 4.) Another inference is at least equally obvious, that plants have the power of fixing and retaining within them, those matters which are suited or essential to their composition; and this power we regard as the simplest form of vital affinity. It may be said, that the alkaline bases are thus fixed in plants, because they enter into combination with organic acids, and that, therefore, it is the formation of these acids, not the retention of the bases which combine with them, that is truly the vital change. But this does not apply to other saline matters contained in vegetables, which must have been taken up from the soil in the same state in which they are found in the plants, *e. g.*, the phosphate of magnesia, which is "an invariable ingredient in the seeds of grasses;" or the silica which is found in certain parts of various plants.

Were it not for this selecting and appropriating power, indicating a simple attraction of some parts of the vegetable for certain earthy or saline matters only, we should find some salts of alumina, as well as of lime or magnesia, in the ashes

of almost all vegetables,—that earth existing in large quantity in all fertile soils, whereas it is “very rarely found in the ashes of plants.”

In the animal kingdom the same power of simple selection and extraction is more fully exemplified, perhaps most strikingly in the development of many of the lower classes, of which the organization is simple, and the matters deposited from the nourishing fluid remarkably diversified, as in many of the radiata and mollusca, which have horny and earthy integuments. And in all animals, so far as any chemical change is effected in the vital actions of absorption, secretion, and even nutrition, it would appear to be chiefly of this simple kind, consisting in the selection and appropriation of compounds already existing in the fluids on which these functions are performed, not in the formation of new compounds. The chyme which is found in the intestines of an animal during digestion contains all the compounds (albuminous, fatty, and extractive matters) which are found in the chyle absorbed from it, although these are in a different state of aggregation, and associated also with other matters which are not absorbed. Since it has been ascertained that the compounds which used to be thought peculiar to the greatest secretions in the body, the bile and the urine, pre-exist in the blood, and are only evolved at the liver and kidneys,—accumulating, therefore, in the blood, when the secretive action of these organs is suspended,—it has become obvious that the main office of these organs is not *formative*, but only *attractive*, to extract from the blood compounds already existing there. And, although there is one material extensively employed in the formation of animal textures, viz., gelatin, which cannot be detected in the blood; yet, as this is the only material so employed which cannot be found there, and as a substance very closely resembling it is found there under certain circumstances, we may assert that in animals by far the greater part of the act of nutrition, numerous and diversified as the compounds forming the solid materials of animal bodies may be, is likewise of this simple kind.

We may consider, then, the selection and extraction, from a previously existing compound fluid, by the agency of a previously existing compound solid, of certain portions of that fluid already elaborated, as a chemical action, essential to all living beings, and so peculiar to them that it may be, at least with high probability, termed an exercise of a vital affinity. And, in regard to this simplest kind of such action, the following points may be considered as ascertained:—

1. It seems to be always performed, in the perfect vegetable or animal, by an agency, not of vessels, as was formerly supposed, capable of a vital contraction, and of changing the nature of their contents by the degrees of that contraction, but of *cells*, either pre-existing in the solid structure, or carried about in the nourishing fluid, and having the name of the globules or corpuscles of that fluid. Most of the textures seem to be formed by the gradual transformation, elongation, or flattening of cells, which have sprung from nuclei at-

tached to previously existing cells : and it seems to be only by the successive formation, distension, rupture, and disappearance of cells, that secretions make their way into the excreting ducts of glands, or on the surface of membranes.

The dependence of all living structures, and of all secretions, not simply on vascular action, by which nourishing fluids are circulated through them, but on *cellular action*, by which this nourishing fluid is changed, appropriated, and retained, or restored to the circulation, is the great step which has been recently gained in physiology by the use of the microscope ; and seems to me to be one of the clearest proofs of the dependence of all vital phenomena, on peculiar attractions and repulsions, actuating both solids and fluids, and causing motions in the latter,—not on any vital powers residing exclusively in solids. When it is stated, *e. g.* by Mr PAGET, that “the purpose to which the capillaries are habitually subservient, is only the passive one of conveying blood close to those parts of the body which either grow or secrete, and that it is proved that if a part be only able to imbibe the fluid portion of the blood from an adjacent vessel, it nourishes itself as completely, and after the same method, as one whose substance is traversed by numerous capillaries,”*—it becomes obvious that the movements of the fluid portion of the blood, whereby they are applied to growth and secretion, must be determined by causes quite distinct from the contractions of vessels.

2. Living and growing cells, therefore, whether acting on the nourishing fluid just taken into the system (as in the case of the intestinal villi, or the tufts of the placenta), or on the blood brought to them by the capillaries (as in the nutrition of the different textures), appear always to have two functions to perform,—to extract from the nourishing fluid the matter of which they are themselves composed, and to extract from it, likewise, the matter which is contained within them,—*i. e.*, in the organs of secretion, the secreted fluids, and in the different solid textures, that additional matter which is always found, whether lignin, oil or fat, fibrinous, cartilaginous, or bony substance, in a granular or less definite form, incrusting the walls of the cells. It does not appear possible to explain what is distinctly seen in all these cases, without supposing that the pre-existing cells exert a peculiar attraction or affinity, both for the matter by which they are themselves to be nourished, and their successors to be reproduced,—and likewise for another matter, different in the different parts of the structure, by which they are to be filled or distended. And in the case of vegetables, there seems to be this general distinction between the two,—that the former is a matter destitute of azote, and the latter one containing that element.

3. The cell, growing always by attracting to itself a compound matter, existing in the fluid state, and giving it a simple increase of aggregation, the nature of the change which takes place as this matter becomes solid, is simply

* Report in FORBES'S Medical Review, July 1843.

consolidation, not *precipitation*, just as the fibrin of the blood, differing from the albumen only in its stronger (vital) tendency to aggregation, is consolidated in its compound form from the liquor sanguinis in the act of coagulation. And thus it happens that these organic solids possess (as was particularly noticed by Dr PROUT) that peculiarity which, in the inorganic world, is observed only in fluids, that even the minutest portion of them contains the very same ingredients (whether earthy or saline, animal or vegetable matters) as is found in the whole mass.

The absence of all crystalline arrangement, and the complex nature even of the smallest particle of an organized body, are the characteristics of matter which has assumed the solid from the fluid form,—not by a chemical precipitation, or separation from matter formerly united to it, but by a vital attraction, subjecting it to “the invisible cause by which the forms of organs are produced.”

4. In the next place, we may inquire what difference exists among the cells in different parts of the same structure, to explain the great difference of the compounds which are deposited in them from the same nourishing fluid; and I apprehend, that, on this point, we must come to the same conclusion which CUVIER drew from examining, throughout the animal kingdom, the structure of the different glands, the vessels entering them, and the ducts passing out of them, viz., that *there is no difference* of structure or of composition, corresponding, in the slightest degree, to the great difference of the products which appear. All cells, in the vegetable kingdom, appear to consist of the same matter, cellulose, and in the animal kingdom of the same matter, protein; and in the first instance they are quite similar to one another. When we attend to the early stages of the existence of a living body, when the difference of textures is only beginning to appear, we find only that a fluid passing through similar capillary vessels, and effused into similar cells, in different *parts of the structure*, acquires different properties. And when we carry our inquiries farther back, and observe the first development of cells themselves out of the granular matter inclosed within the sac of the yolk, it appears obvious that the particles of this matter are attracted, not into cells already existing, but *to points where cells are about to be formed*. The facts known as to the evolution of the chick in ovo from the matter that lies in contact with the germinal membrane, sufficiently indicate that the powers which effect the separation of the different component parts of that matter, so as to form the beginning of the different textures and organs, reside, not in pre-existing cells of different composition or structure, but simply in different points of a pre-existing membrane, which, in the first instance, is homogeneous. The expression of LIEBIG, that “the chemical forces in living bodies are subject to the invisible cause by which the forms of organs are produced,” when the action of that cause is duly considered, implies, that they are subject to a cause which undoubtedly acts differently at different points of the same matter; but the differ-

ence of the action of which, at these points, is determined by no other condition, that we can see, than their *position*.

This mode of limitation of the vital affinities by which the selection and appropriation of living matter is effected, is only a statement of fact, and the most general fact that has been ascertained; and it seems highly probable, that it will be found an ultimate fact, in this department of science. It may serve to familiarize our minds with this principle to observe, *first*, that it is precisely analogous to the principle which is now well established as a first truth in the physiology of the nervous system, that portions of nervous matter, precisely similar in structure and composition, have perfectly different endowments according to the anatomical position which they occupy; and, *secondly*, that the same principle seems distinctly exemplified in various cases of diseased action. The phenomena of inflammation, and especially the easy recurrence of inflammation once excited at any one spot in a living animal, indicate that certain vital attractions and affinities existing among the particles of the blood, and between them and the surrounding textures, are peculiarly modified, not merely in a particular manner, but exclusively at a particular spot. From the spot where it commences (*e. g.*, on a serous membrane), this alteration of vital actions extends, as from a centre, to parts that are contiguous to, although having no vascular connection with, that where it commenced, as we see in tracing it from one fold of the peritoneum to another. And when we examine the results of the inflammation in the dead body, we see what clearly shews the operation of a force, producing chemical changes of the kind we are now considering, but acting only at one part, and in one direction. "The capillaries which have taken on the appearance of inflammation, are all on one side of the fine membrane, and the serum and lymph, effusions from these vessels," by which the diseased state is essentially characterized, "are all on the other."—(GOODSIR, *Anatomical and Pathological Observations*, p. 43.)

In saying that the fundamental property of chemical selection, essential to the growth of all living bodies, is strictly a vital property, we do not overlook the fact that various substances, composed of inanimate or inorganic matter, have likewise different powers of attraction for different elements or compounds brought into contact with them. It appears to be only by reference to this property, that we can explain the well-known phenomena of *endosmose* and *exosmose*, in which different fluids, brought in contact with a solid body, are attracted into its pores with very different degrees of force. It is not the nature of the process by which the selection, in the case of the living body, is effected; but the peculiarities of the selections themselves, their great force, and yet uniformly temporary existence, that entitle us to regard them as indicating a vital property.

II. But when we attend to the peculiar changes effected by living solids on the fluid matters which are brought in contact with them, we find that these are by no means confined to the *selection* and appropriation, at particular points, of

compounds pre-existing in that fluid; but that, under the influence of the living solid, *transformations* or new arrangements of the chemical elements take place, and new compounds are formed.

In regard to the precise nature, or seat, of some of these transformations, there is considerable difficulty, but we are at present concerned only with the principle; and may state in illustration of it, two cases of transformation, of which there is no doubt, the change from carbonic acid and water to starch in the cells of plants (oxygen escaping), and the change from starch to fat in the cells of animals (carbonic acid and water escaping). And that I am correct in asserting that the organ which exercises this and other chemical powers in living plants is not only of the simplest construction, but of uniform construction, while the products of its action are very various, will appear from the following statement by MÜLDER.

“Pure cellulose is easily obtained from the pith of the elder-tree, from very young roots, and from other young parts of plants. From these parts it is prepared by digesting them, after being minutely divided, with alcohol, ether, diluted potash, hydrochloric acid, and water. In this manner, the starch, gum, fats, resins, vegetable alkalis, salts, sugar,—and at the same time the peculiar woody matter are separated.”

“After the action of these solvents, and especially of the alkali, the cellulose, which was formerly solid and dense, appears in a spongy form. We may state as a fact, that the proper tissue of *all plants* which have been previously exposed to the influence of these solvents, leaves a substance which is identical in all of them, a substance which contains carbon and the elements of water.”—(*Chemistry of Vegetable and Animal Physiology*, pp. 188–195.)

MÜLDER annexes to this statement a speculation in regard to the influence of *forms* in organized bodies, as affecting their chemical powers or properties, which, so far as I can understand it, I think fitted to convey an erroneous impression.

“One of the first and chief laws visible in organic nature is, that the form has as much influence on the character of the phenomena as the substance of which that form consists. The effects of the primary forces existing in the molecules, have become, by the combination of elements into hollow globules, altogether peculiar.”

“In organic nature, besides all the peculiarities existing in the carbon, hydrogen, and oxygen, we must suppose, as a chief consequence of this, a tendency to form membranaceous, concave, spherical little bodies, in which, *because of this form*, new peculiar properties manifest themselves, which cannot be brought out by other forms. Thus, by matter and form, all that we observe in nature is, to a great extent determined.”—(*Ibid.*, p. 189.) If by this it is meant that the acquisition of the form is the physical cause of the existence of the properties which cells, or any other organized structures present in the living state, two questions

immediately present themselves, *first*, How are the cells themselves formed (*e. g.* on the germinal membrane of the ovum) out of a matter which is originally without form, otherwise than by those very properties which are here ascribed to their existence? and, *secondly*, If the properties are dependent only on forms, why do they not exist in the dead state, when the forms are, in many instances, still perfect? The enunciation of these questions seems to me sufficient to shew, that the correct expression of the state of our knowledge on this point is that already quoted from LEIBIG, that the chemical forces in living bodies are subject, not simply to an influence of forms, but to "*the invisible cause by which the forms of organs are produced,*" *i. e.*, that we must include under the head of vital properties, both the mechanical, or simply attractive power, by which cells or other organs are formed out of amorphous matter, and likewise the chemical powers with which these cells are endowed.

It is no objection to what has been stated, of the strictly vital nature of these chemical powers, to admit that their action is very often *analogous* to the principle to which the name *catalysis* is given by chemists, and which is exemplified likewise in the chemistry of inorganic compounds, where the combination of two substances is determined by the presence of a third, which nevertheless takes no part in the combination itself; or that it is analogous to that disturbance of the equilibrium of chemical compounds, by which the fermentation of an organic compound is transferred to another in contact with it, although the changes in the two go on separately, and the compounds formed are different. It is quite true, that these modes of chemical action resemble and illustrate the manner in which living solids, themselves undergoing continual changes of composition, determine new arrangements of the elements of the compound fluids which are brought in contact with them. But this analogy is far from being an explanation or resolution of the one phenomenon into the other. In the first place, the analogy is essentially defective; because although it is true that in any living being, already existing, different chemical compounds already exist in different parts of the structure, which may act in these modes on the nourishing fluid, and determine distinct transformations of these at different parts; yet this does not apply, as already observed, to the first formation of each of the textures, at its appropriate point, from a homogeneous semi-fluid matter. But farther, although we were to admit the *analogy* of all the chemical processes going on in living beings, to these forms of simply chemical action, we should not thereby be authorised to conclude that the vital processes have not that peculiarity which makes it incumbent on us to regard them as a separate class. We say that the decomposition of carbonic acid, the combination of the carbon with the elements of water to form starch, and the evolution of the oxygen, is a vital action,—not because it is a change different in kind from the decomposition of water and evolution of the hydrogen by iron and acid,—but simply because it indicates an affinity peculiar to the state of life;—because in no other circumstances, when the elements of

water are brought into contact with carbonic acid, is any such decomposition effected. So also, although it is true that the presence of spongy platinum enables oxygen and hydrogen to unite and form water, or the presence of fermenting yeast enables sugar to undergo transformation into carbonic acid and alcohol, still these facts do not interfere with those essential peculiarities on which the doctrine of vital affinity depends, viz., that the presence of living cells composed of carbon and the elements of water, determines both the addition of new matter, from a compound fluid, to those cells, and likewise the formation of other compounds within the cells, varying in different parts of the same structure,—all these compounds being different from any which the chemist can form out of the same elements, and different from those to which the same elements inevitably return, after the phenomena of life are over. The physical principle of catalysis may be said to *illustrate* the transformations in living bodies, as that of endosmose illustrates the selection and appropriation of chemical elements or compounds in living structures; but these principles, as exemplified in dead matter, include none of the peculiarities of the vital chemical actions, and therefore furnish no *explanation* of them.

The materials of which animal bodies are composed, have been now so generally found to have been prepared for them by vegetables, that it has been reasonably doubted, whether any such power of decomposing the fluids presented to them, and forming new compounds, exists in animals. There are some cases, however, in which it appears certain that an action of this kind goes on in living animals, and that it is effected, as in vegetables, by an agency of cells. Thus, there is good evidence that, in the natural state, much of the bile which is discharged into the intestines from the liver is re-absorbed in its passage along the *Primæ Viæ*; yet it never appears in the chyle, nor, in the natural state, in the blood; which seems to imply that it is decomposed, and its elements thrown into other combinations, in the course of the *cellular action* which attends the absorption of chyle.

In like manner, the formation of fatty compounds out of starch, or its kindred principles, as illustrated by the recent precise observations on the formation of wax by bees, and the formation of gelatine in the living animal, are undoubted instances of chemical transformations thus effected. The precise scene of these transformations is not yet ascertained, but we have strong reason from analogy to suppose that they are effected in the course of the circulation. And as we are certain that the greatest of all the chemical changes which are peculiar to living beings are effected within the cells of vegetables, it seems in the highest degree probable, that the corpuscles or cells (both red and white) which form so large a part of the blood of animals, are concerned in the chemical transformations which take place in blood; and therefore, that we are to regard organized and living cells as the agents or instruments employed by nature in effecting all those chemical changes which are peculiar to the state of life. And if we consider this

principle as established, it goes far to explain several facts, long regarded as obscure, in regard to the structure and position of the lymphatic and lacteal vessels. We know that the mode of origin of these vessels gives *time* and opportunity for cellular action, (*i. e.*, the development, growth, and rupture of cells,) and consequent chemical changes, at their extremities; we know that such cellular action does in fact go on there, particularly in the lacteals; and we know that the substances absorbed there, and probably elsewhere, by these vessels, are in fact altered, and so far assimilated, in the act of absorption; as in the case, already mentioned, of bile absorbed from the intestines. Thus we are led to see the importance of these vessels being placed at all points where substances are to be absorbed, which are foreign to the animal economy, or require chemical change, in order that they may be introduced with safety or good effect. Hence, also, we see the use of the lymphatic glands, at which another opportunity for cellular action, for chemical changes and assimilation, according to the observations of Mr GOODSIR, is provided.* And this also enables us to understand a general fact, which, although disputed, I believe to be both true and important in pathology,—that a substance destined for excretion, but retained in the blood by reason of disease of its excreting gland (particularly the bile or urine), is more injurious than the same matter when secreted by the gland, but re-absorbed from a mucous surface, and consequently subjected to cellular action, and thereby to chemical change.

III. Another general fact appears to be sufficiently illustrated by observations on the chemical changes in living bodies,—viz., That the vital properties by which these are effected are *transferred* from the portions of matter already possessing them, to those other portions of matter which are either taken into their substance, or deposited in their immediate neighbourhood. It is, indeed, obvious, that if we are right in saying that living matter possesses these peculiar vital properties, the act of assimilation which we know to be continually going on in living bodies, is not merely the attraction and addition of new matter, but must include this transference of vital properties to the matter which is continually added to the existing solids.

“The force with which life is kept up,” says Professor WHEWELL, “not only produces motion and chemical change, but also *vitalizes* the matter on which it acts, giving it the power of producing the same changes in other matter, and so on indefinitely. It not only circulates the particles of matter, but puts them in a stream, of which the flow is development as well as movement.”—(*Philosophy of Inductive Sciences*, vol. ii., p. 52.)

Several facts which are known in physiology and pathology, may be noticed as more special exemplifications of this principle. Thus, we know that vessels in

* See CARPENTER'S Manual of Physiology, § 493.

any part of the body communicate certain properties to the whole mass of blood which lies in contact with them, so as to modify or suspend for a long time the coagulation of such blood;—that the blood which enters the vessels of any part where inflammation has been excited, has peculiar properties impressed on it, and even changes on its composition effected, merely by coming in contact with the portions of vessels where that process is going on, and with the portions of blood previously subjected to it;—that the exudation from inflamed vessels acquires peculiar properties from the contact with the living surface on which it lies, first arranging itself as an organized structure, and then selecting and appropriating, from the neighbouring bloodvessels, those materials by which it is assimilated to the texture with which it is connected;—again, that, in the sound state, every portion of matter which is deposited from the bloodvessels, to form part of a muscle or of a nerve, immediately acquires the peculiar vital properties of the part which it nourishes; and, in the case of muscles, even that the change produced in a portion of a fibre by the application of a stimulus, is instantly communicated to the whole length of that fibre, and to many adjoining fibres. It appears to be nearly in the same manner that every portion of carbon and water which enters into the composition of any living vegetable cell, acquires the power of exerting the same vital affinities as actuated the matter which it replaces, or to which it is added.

IV. Another principle, at least equally important and characteristic, may be stated in regard to this communication of vital properties to the materials which are added to living bodies, viz., That such powers are imparted only for a brief period of time, and that long before the time of the death of the structure to which they belong, all those materials lose the vital properties which have been given to them; perhaps, as has been lately stated, as a consequence of the exercise of their peculiar vital powers, perhaps merely as a general law of vitality; but equally, whether the peculiar properties which they acquire in living bodies are of the nature of nervous actions, vital contractions or attractions, or vital affinities. But as this principle is best illustrated by reference to the phenomena of excretions, we delay doing more than merely enunciating it at present.

Having so far considered the general nature of the chemical changes which are peculiar to living bodies, and the kind of apparatus provided by nature for carrying on these changes, we may next take a more special view of the different chemical changes themselves, beginning with the greatest and most fundamental of all, the formation of the amylaceous matters by vegetables, acting on the water and carbonic acid with which they are supplied, both in the liquid form by their roots, and in the gaseous form by their leaves,—and the consequent evolution of oxygen. In regard to this grand function of living plants, the following facts seem the most important that have been ascertained.

1. We see this change effected, in the present order of things, only by the agency of one of the amylaceous principles themselves, although the quantity of that pre-existent matter, in the case of the seeds of many vegetables, is exceedingly minute. We need not enter on the question how far, besides the pre-existence of matter capable of forming cells, in the textures of the plant itself, previously existing organized matter, in the dead state, is essential as part of the nutriment of vegetables,—farther than to observe, that, as the seed of every plant contains a store of organic compounds already formed, there is certainly a strong presumption that a certain quantity of such compounds, formed by previous living processes, is highly useful, if not necessary, to the nourishment of vegetables as well as animals. This, however, appears most important in the early period of the existence of plants, when their power of decomposing the carbonic acid has not yet attained its full intensity. The evidence of the greater part of the nourishment of vegetables being from carbonic acid, water, and ammonia, applied to their leaves, or absorbed by their roots, is quite conclusive; and when we consider that vegetables preceded the appearance of animals on earth, that the first vegetables (as is well observed by LIEBIG) were of the kind which depend least on their roots and most on their leaves for subsistence, and that the kinds of animals which first inhabited the earth, were those which consume the smallest quantity of oxygen, and can live, therefore, in air highly charged with carbonic acid,—it appears in the highest degree probable, that a gradual purification of the atmosphere by the agency of vegetables abstracting carbon, was a necessary prelude to the introduction of animals, especially of warm-blooded animals, into the world: and that the greater part of the carbon now existing in the soil on the earth's surface, originally existed in the form of carbonic acid in the atmosphere, and has been gradually fixed, and enabled to become the chief support of all living beings, by this vital affinity of vegetables, and of those tribes of the lowest marine animals, which have been found to possess the same property, whereby carbon is separated from oxygen, and combined with the elements of water, to form the amylaceous matters.

2. The dependence of the exercise of this property on the presence of light, and its connection (according to the statements of Dr DRAPER), not with the heating portion of the rays, nor with those which effect other chemical changes, but simply with the luminous portion of the rays, shews distinctly that all living action on this globe is equally dependent on light as on heat, although it is, and may long be doubtful, in what manner the influence of light is exerted in producing this change; whether the theory long ago proposed by Sir H. DAVY is admissible, that light enters into the composition of oxygen gas when disengaged from any solid or liquid compound containing it; or whether the agency of light may be better expressed by saying, that it is the necessary stimulus to that kind of vital action which leads to this primary transformation of the elements of which organized beings are composed.

3. It is unnecessary to enter here on the varieties of this amylaceous matter which are formed in different vegetables or parts of the same, the *cellulose* of which the cells are formed, the *starch*, the *dextrin*, the *gum*, the *inuline*, which are deposited in different species and in different parts. All these appear to have the same simple fundamental composition, consisting almost entirely of carbon with the elements of water, and all are formed out of the same compounds and by a vital affinity essentially the same; it may be partly owing to some imperceptible difference in the relative position of the ultimate atoms, partly to differences in the minute quantities of inorganic matter, and of other organic compounds not yet mentioned, which enter into their composition, that so many varieties are found, not only in these compounds themselves, but in the qualities which they present as found in different species of plants, and even in different individuals of the same species. In the case of a graft inserted on the stem of an individual, or even of a species, different from that which furnishes the shoot, we see that the vital affinities of the particles composing the shoot are capable, not only of extracting from the nourishing fluid of the stock all the compounds required for its development, but of imparting to the living textures formed of those compounds which they extract, all those peculiar properties of form, of colour, of smell, of roughness, smoothness, &c., by which species, and even individuals of the same species, are characterized. And when we consider these facts, I apprehend we must admit that, under the influence of the vital affinities which operate in the cells of living vegetables, much more minute differences of compounds are produced, than can be detected and explained by any chemical analysis.

4. An important question here is, whether the carbonic acid of the air is decomposed in the leaves where it is chiefly taken in, the amylaceous compounds immediately formed with the help of water, and the oxygen set at liberty, or whether that acid is taken into the juices of the plant, as we now know that oxygen is into the blood at the lungs, and gradually decomposed there, letting its oxygen escape gradually, and aiding in the formation of different compounds, besides the varieties of starch. That the latter is the more probable supposition may be inferred, partly from the analogy of the action at the lungs of animals, but chiefly from the fact, that a separation of oxygen is equally required for the elaboration, which certainly takes place in vegetables, of other compounds, of the varieties of oil, and of protein, which are chiefly deposited in other parts of their structures.

5. The relations of compounds of this class to sugar demand more special notice. It seems doubtful whether this is ever the first compound formed; it appears in the sap of various plants when the fluids from the soil are ascending and dissolving the starch which had been formed and stored up by the living actions of the preceding year; it appears in almost exactly the same circumstances dur-

ing the germination of seeds, and in both these cases is useful as giving a greater degree of solubility to the starch whence it is formed. In both cases it disappears, and probably is converted into some of the varieties of starch, as the vital actions of the plant become more vigorous. Its composition, in its different varieties, as given by most analysts, $C_{12} H_{11} O_{11}$, or $C_{10} H_{10} O_{10}$, or even $C_{12} H_{14} O_{14}$, denotes that if it be formed from the starch, $C_{12} H_{10} O_{10}$, it must be either by the addition of the elements of water, or by the abstraction of carbon; and as its formation, during the germination of seeds, is attended with evolution of carbonic acid, it seems most probable that, in that case at least, it is formed in this last way, under the influence of the oxygen of the air. It appears again in the nectaries of flowers, and in the ripening of fruits, as one of the latest results of the vital action of plants, in those parts of them which are fully exposed to air and light, but at a time when we may reasonably suppose that the vital affinities are becoming comparatively ineffective, and when carbonic acid is again evolved. It may be formed by the chemist from some of the varieties of starch by a kind of fermentation, excited by diastase, as in malting; or by a catalytic action of sulphuric acid; and it is formed from starch merely by the agency of cold, as in frozen potatoes, and from inuline merely by continued boiling in water; so that its formation from starch in vegetables seems to be most probably a simple chemical change, not the effect of a vital affinity. Farther, it is a compound which takes the crystalline form, essentially different from any form assumed by those parts of organized structures which exhibit truly vital phenomena, and retains its properties when exposed to air and water better than any of the matters of which organized forms are composed. From all these facts it may be inferred, with great probability, that sugar, as it appears in the living vegetable, is generally to be regarded as a first product of decomposition of starch, by the agency of water and of the oxygen of the air, which appears to be the great agent in the resolution of those compounds, which the vital affinities have built up.

6. On the other hand, the relation of starch and cellulose to the lignin, which forms the greater part of the solid matters of dicotyledonous plants seems to be nearly the reverse of their relation to sugar. This matter is always found incrusting, or incorporated with, the cells of vegetable textures; it gives them their solidity and strength, which all decompositions by chemical agents impair; it cannot be formed from the compounds of starch by artificial means, but is formed from them in greatest quantity when the vital actions of plants are strongest; and its composition is always stated as differing from the amylaceous compounds by containing more carbon; and less oxygen, in proportion to the hydrogen, than exists in the composition of water; its formula being stated as $C_{40} H_{23} O_{18}$. This, therefore, would appear to be clearly the result of truly vital affinities, continuing to actuate the elements of starch, after the formation of the

starch from carbonic acid and water has been completed, and effecting a decomposition of part of the water, as well as of the carbonic acid, presented to the living vegetable.

In studying this first and most striking of all the changes which are to be ascribed to vital affinities, it is especially necessary to understand the parts assigned to Carbon and Oxygen; and, in taking this general view, we must regard vegetables and animals as inseparably linked together, and look to the whole series of chemical changes which intervene between the origin of vegetables and the death and decomposition of animals. We must regard the carbon, originally existing in combination with oxygen in the atmosphere, in the proportion of one equivalent to two, as the great agent employed by Nature in the formation of the whole organized creation, insomuch that all organic chemistry may be said to be the chemistry of compounds of carbon.—(*Gregory's Chemistry*, p. 241.) That it may fulfil this office it is invested with peculiar but temporary powers; it is separated at particular points and under certain conditions from the oxygen, and attaches itself to the elements of water, always present where vegetables grow, and so forms various compounds, beginning with the varieties of starch; in all which it is the principal ingredient. The compounds thus formed next attack and partially decompose the water and appropriate the hydrogen, thus causing a farther evolution of oxygen, and forming oil; and afterwards nitrogen, in small quantity, is introduced, and fresh transformations take place, by which the protein compounds are formed. All the solid structures of vegetables, and indeed of organized beings generally, are made up of these compounds of carbon, in which oxygen exists either in the proportion to hydrogen which forms water, or in a less proportion than that; and the formation of these may be confidently ascribed to vital affinities. But it is easy to conceive that other compounds of carbon with hydrogen and oxygen will exist in plants in which the oxygen will be in larger proportion than this, without supposing oxygen from the air to be added; because the vital affinities may not have been in sufficient force to separate the oxygen completely from its original union with carbon, and these, therefore, may be regarded as compounds of carbon, water, and undecomposed carbonic acid. Such are the different organic acids (the citric $12\text{C } 8\text{H } 14\text{O} = 9\text{C} + 8\text{HO} + 3\text{CO}_2$, the malic $8\text{C } 6\text{H } 10\text{O} = 6\text{C} + 6\text{HO} + 2\text{CO}_2$, the tartaric $8\text{C } 4\text{H } 10\text{O} = 5\text{C} + 4\text{HO} + 3\text{CO}_2$, the oxalic $4\text{C } 2\text{H } 8\text{O} = \text{C} + 2\text{HO} + 3\text{CO}_2$) which are found in the juices of many vegetables, particularly in the immature state.

Again, it is always to be observed, not only that all organized bodies are destined ultimately to revert to the water, carbonic acid, and ammonia, from which they were originally formed, but that, in the case of animals at least, there is a process always going on during the state of life, by which these same inorganic matters are continually evolved from the living frames. Therefore, we cannot be surprised to find that the fluids of all living animal bodies contain other

compounds, in which the characteristic predominance of carbon is not perceived ; because they are those which are formed in circumstances where the vital affinities are losing their power, and where a step has been made towards that final dissolution of organic compounds, when the oxygen is to resume its power over the carbon, and this is to revert, directly or indirectly, to the condition of carbonic acid. This general principle as to the respective offices of carbon and oxygen in living bodies,—the one the main agent in nourishing and supporting living structures, the other in maintaining the excretions by which these structures are continually restored to the inorganic world,—we shall find to be applicable, not only to the excretion of carbonic acid and water by the skin and lungs, as compared with the amylaceous compounds taken into animal bodies, but likewise to the excretions by the liver and kidneys, as compared with the two other great constituents of the food of animals, viz., the oily and the albuminous substances.

in the excretions, particularly in the great excretion by the lungs. We now know that the speculation as to the connection of the oxygen of the air with vital action, long and ably maintained by the late Mr ELLIS, viz., that its sole use is to dissolve and carry off excreted carbon, and therefore that in the bodies of animals it goes no farther than the lungs, was erroneous ; but we may assert with much confidence, that it goes no farther than the circulating blood ; and that, although its action there is essential to all the metamorphoses which are there accomplished, yet all the combinations into which it actually enters, are destined to immediate separation from the living body,—being, in fact, the media by which all living bodies, at all periods of their existence, are continually resolving themselves into the inanimate elements from which they sprung. This principle will be better illustrated, however, by a review of the leading facts lately ascertained as to the formation of the other compounds peculiar to organized bodies, and the excretions of animals.

XVIII.—*Account of some Experiments on the Temperature of the Earth at different Depths, and in different Soils, near Edinburgh.* By JAMES D. FORBES, Esq., F.R.S., Sec. R.S. Ed., &c. Corresponding Member of the Institute of France, and Professor of Natural Philosophy in the University of Edinburgh.

I. *History of the Observations.*

THE proper temperature of our globe is a question which, formerly abandoned to speculation and hypothesis, has only lately been made the subject of direct experiment. Preliminary to it, and intimately connected with it, is another inquiry of great interest, namely, What is the thermometric effect of the whole solar heat which falls in a year on the surface of the globe? How much is transmitted to the interior? How much dissipated at the surface? To what depth does the influence of the seasons extend, and in what manner is that influence modified at different depths? It is impossible to say to how many curious and important inquiries a solution of these preliminary questions may lead the way; and it is to them that our attention is at present to be confined. We shall not speak, except incidentally, of the absolute heat of the interior of the globe; we shall only discuss the modifications of the solar heating influence near its surface.

This inquiry was perhaps first agitated by the illustrious LAMBERT, a mathematical philosopher of Germany, who yields, in originality, in comprehensiveness of mind, and in the successful application of mathematics to a wide range of important physical subjects, to very few of his contemporaries or successors. His experiments were made by M. OTT, a merchant at Zurich, who had a convenient garden for introducing the thermometers.*

Having claimed for LAMBERT the first systematic inquiry on this subject (for the previous essays of MARIOTTE and HALES could not lead to any exact conclusions), I shall not trace the subsequent history of the problem, which is fully stated in M. QUETELET's papers, in the Transactions of the Brussels Academy for 1836 and 1840, and in the *Annales de l'Observatoire de Bruxelles*, tome iv. (1845). Such observations were made by HERRENSCHNEIDER, at Strasbourg; MUNCKE, at Heidelberg; by LESLIE, at Raith, near Edinburgh; by ARAGO, at Paris; by QUETELET, at Brussels; and by RUDBERG, at Upsala. As it does not appear, that, in making or discussing these observations, regard has been had to the influence of

* LAMBERT, *Pyrometrie*, 4to, 1779, page 356.

a peculiar character of the soil or rock whose temperature was observed, it occurred to me, several years ago, to make several series of observations, under circumstances as exactly similar as possible, with the exception of the nature of the soil or rock. The neighbourhood of Edinburgh, from its variety of geological character, offered peculiar facilities for this purpose; and the British Association, at my request, undertook the expense of providing and inserting thermometers in three different positions, at depths corresponding to those already employed at Brussels, namely, 3, 6, 12, and 24 French feet below the surface. The results have already been partly published in the Proceedings of the British Association, and of the Royal Society of Edinburgh. Deeming it advisable that the curves containing the details of the observations should be published at large, I requested permission from the Committee of Recommendations of the British Association, in 1845, to communicate them, for this purpose, to one of the Royal Societies; and the Council of the Royal Society of Edinburgh having agreed to be at the necessary expense of the plates, I am enabled to present the results in their present complete form, and founded upon five years' observations.

II. LESLIE'S *Observations at Abbotshall, in Fife.*

I shall here reproduce the particulars of the observations of the temperature of the ground at Abbotshall, in Fifeshire, on the property of Raith, close to the town of Kirkcaldy. The distance from Edinburgh is sufficiently small (11 miles in a right line) to render these observations comparable with ours; but I quote them more particularly, because those who have hitherto made use of them, being unaware of the original account published by Sir JOHN LESLIE,* have made almost every possible mistake as to the locality, circumstances, and depths of these observations. It will be seen that they were made by Mr FERGUSON of Raith's gardener. The following extract contains all the important details.

“ In order to throw distinct light on a subject so curious and important, ROBERT FERGUSON, Esq. of Raith, a gentleman whose elegant mind is imbued with the love of science, caused, lately, a series of large mercurial thermometers, with stems of unusual length, to be planted in his spacious garden at Abbotshall, about 50 feet above the level of the sea, and nearly a mile from the shore of Kirkcaldy, in latitude $56^{\circ} 10'$. The main part of each stem having a very narrow bore, had a piece of wider tube joined above it; and, to support the internal pressure of the column of mercury, the bulbs were formed of thick cylinders. The instruments, inclosed for protection in wooden cases, were then sunk beside each other to the depths of one, two, four, and eight feet below the surface, in a soft gravelly soil, which turns, at four feet, into quicksand, or a bed of sand and water. These thermometers were carefully observed from time to time by Mr CHARLES NORVAL,

* Supplement to the 6th edition of the *Encyclopædia Britannica*, article CLIMATE, incorporated in the 7th edition.

the very intelligent gardener at Raith; and we have now before us a register of their variations for nearly three years. It thence appears, that, in this climate, and on naked soil, the frost seldom or never penetrates one foot into the ground."

* * * * *

"These observations are quite satisfactory, and exhibit very clearly the slow progress by which the impressions of heat or cold penetrate into the ground. It will not be far from the truth to estimate the rate of this penetration at an inch every day. The thermometers hence attained their maximum at different periods, though in a tolerably regular succession. The mean temperature of the ground, however, seemed rather to increase with the depth; but this anomaly has evidently proceeded from the coldness of the two last summers, and particularly that of 1816, which occasioned such late harvests and scanty crops. Thus, the thermometer of one foot indicated the medium heat of only $43^{\circ}8$ during the whole of the year 1816. But it will be satisfactory to exhibit the leading facts in a tabular form. The following are the mean results for each month, only those for December 1817 are supplied from the corresponding month in 1815.

TABLE I. LESLIE'S OBSERVATIONS.

	1816.				1817.			
	1 Foot.	2 Feet.	4 Feet.	8 Feet.	1 Foot.	2 Feet.	4 Feet.	8 Feet.
January.....	33°0	36°3	40°7	43°0	35°6	38°7	40°5	45°1
February.....	33°7	36°0	39°0	42°0	37°0	40°0	41°6	42°7
March.....	35°0	36°7	39°6	42°3	39°4	40°2	41°7	42°5
April.....	39°7	38°4	41°4	43°8	45°0	42°4	42°6	42°6
May.....	44°0	43°3	43°4	44°0	46°8	44°7	44°6	44°2
June.....	51°6	50°0	47°1	45°8	51°1	49°4	47°6	47°8
July.....	54°0	52°5	50°4	47°7	55°2	55°0	51°4	49°6
August.....	50°0	52°5	50°6	49°4	53°4	53°9	52°0	50°0
September.....	51°6	51°3	51°8	50°0	53°0	52°7	52°0	50°7
October.....	47°0	49°3	49°7	49°6	45°7	49°4	49°4	49°8
November.....	40°8	43°8	46°3	45°6	41°0	44°7	47°0	47°6
December.....	35°7	40°0	43°0	46°0	37°9	40°8	44°9	46°4
Mean of whole Year	43°8	44°1	45°1	46°0	44°9	45°9	46°2	46°6

"If the thermometers had been sunk considerably deeper, they would, no doubt, have indicated a mean temperature of $47^{\circ}7$. Such is the permanent temperature of a copious spring which flows at a short distance, and about the same elevation, from the side of a basaltic or greenstone rock."

I had intended to have engraved the curves of the course of temperature from

1816 to 1821, which I find preserved in the Natural Philosophy Collection; but my inquiries led me to discover that most of these curves had already appeared in the 4th volume of CONSTABLE'S Edinburgh Magazine for 1819, where they have remained apparently quite unknown to scientific men, together with the *details* of the observations on which they were founded, from May 1815 to March 1819, and an explanatory article, which I understand was written by Mr GEORGE BUCHANAN, civil-engineer. These observations, particularly at the commencement, were not made with great regularity, sometimes only a single observation being made in a month, at other times as many as eight. Indeed, they have not appeared to me to be worth reprinting at length; but I have condensed into the following Table the information which they contain, shewing the mean monthly temperature, at different depths, for nearly four complete years.

TABLE II. MEAN RESULTS OF LESLIE'S OBSERVATIONS.

1815-19.				
	1 Foot.	2 Feet.	4 Feet.	8 Feet.
January	36°·0	38°·9	41°·0	43°·8
February	35·3	36·3	40·6	42·5
March	38·1	38·6	41·0	42·1
April	38·6	40·2	41·8	42·2
May	45·5	45·3	45·3	44·4
June	54·5	52·4	49·1	46·9
July	57·2	54·8	51·9	48·2
August	54·9	54·8	52·3	50·3
September	53·3	53·0	52·1	51·0
October	47·7	49·5	50·0	50·0
November	42·9	45·3	47·5	48·5
December	37·5	40·8	43·8	46·3

III. *Construction of the Instruments.*

In commencing the observations at Edinburgh, it was determined that the thermometers should consist of three sets of four each, the lengths increasing in geometrical progression, and the localities being fixed so as to embrace within a small radius, as great a variety of soil as possible. As the deepest thermometers were to be sunk to a depth of 24 French feet (25·6 English), and the portion of the tube, including a column sufficiently long to register the variations of temperature of the fluid which filled the ball (alcohol), must project a foot or two above the surface, the construction, graduation, and depositing of such unwieldy instruments, were attended with no small practical difficulty. The execution of the

whole was entrusted to Mr ADIE, to whose experience and skill I am greatly indebted for the completion of the whole without any material accident.

For the larger thermometers it is of great consequence that as little liquid as possible should be contained in the stem of the instruments, otherwise the apparent expansion of the column will depend greatly upon the variable temperature of the different parts of the stem, as well as on that of the level. This correction being difficult to apply with mathematical exactness, it was desirable to make it as small as possible (although it would be unwise to overlook the correction altogether, as most observers have done). I accordingly had twelve tubes drawn, each of about 26 feet in length; of the external thickness nearly of a common barometer tube (about half-an-inch), but whose internal diameter was nearly capillary. These were carefully examined throughout, by means of a column of mercury passed through them.

The proportional numbers, representing the calibre of the tubes, were entered in a table now before me, corresponding to every foot of their length; and the tubes were numbered, so that the degree of uniformity of any portion could at any time be ascertained. From these tubes twelve lengths were cut from the most uniform parts, amounting altogether to about 144 feet, for the construction of the three sets of thermometers.

It is to be understood that the capillary tube now spoken of was made with a view to reach the surface of the ground, above which the tube should expand into one having degrees of a convenient length. So small, indeed, was the stem compared to the bulb, that a degree of Fahrenheit in the capillary tube would have occupied, in one case, a space not less than 51 inches long. The wide tube to which the scale was attached, had a bore of about $\frac{1}{16}$ inch, and was made long enough to include the expected range of temperature at their respective depths. These ranges were, however, in some cases rather under-rated.

The bulbs were cylindrical, and varied in size from about 6 to 8 inches long, and $1\frac{1}{2}$ or $2\frac{1}{2}$ wide. They were blown at the glass-house separately from the tubes. The deepest thermometers had the largest bulbs and longest degrees, because the required range was less.

From the length and fineness of the tubes much trouble would have been experienced in filling the thermometers in the usual way. The lower end of the cylindric bulb was, therefore, drawn out into a tube, by which the liquid (freshly boiled alcohol, slightly coloured) was admitted, and it was drawn in by the action of a syringe fixed at the extremity of the long stem. Both ends were then closed in the usual way, an expansion being left at the top as in common alcohol thermometers, but most necessary in this case, in order to allow for the changes of temperature to which the instruments were exposed before sinking them in the ground.

The graduation was one of the most delicate parts of the construction. The

instruments were suspended during winter in a staircase, with their bulbs in water, and the temperature of the air surrounding the stems was carefully noted, and a correction applied for any difference between it and that of the water. The staircase was artificially heated through a few degrees, and after being left for a night, a second point was fixed. The temperatures of the water were determined by the mean of three standard thermometers, which agreed extremely closely indeed, when the error of their freezing points was corrected. The first was a standard by TROUGHTON and SIMMS, belonging to and corrected by myself.* The second was a standard constructed by Mr ADIE for the Royal Society of Edinburgh; and the third a standard having very long degrees, constructed by Mr ADIE for his own use. The first pair of observations were made by Mr ADIE *senior* alone, and scratches marked on the tubes at temperatures corresponding to $41^{\circ}73$ and $50^{\circ}77$ by the mean of the standard thermometers. To verify these results I made two additional comparisons with the assistance of Mr ALEXANDER ADIE *junior*, at temperatures $41^{\circ}97$ and $46^{\circ}42$, which agreed by interpolation remarkably closely with those of the first experiment, considering the difficulties of the observation. In only one case (the 13 feet thermometer for the Experimental Garden), was the difference at all considerable. A mean result was adopted. The length of 1° in the 24 and 12 feet thermometers being from 1 to 2 inches, and divided into 20ths, $\frac{1}{20}$ can be easily read by estimation. In the others the approximation is less.†

IV. *Localities—Sinking of Thermometers.*

Whilst the preparation of the thermometers was going forward, I had holes prepared for inserting them in the positions already fixed on with reference to the geological peculiarities of the soil. These were—

1. In the Observatory enclosure on the Calton Hill, at a height of 350 feet above the sea. The rock is a porphyritic trap, with a somewhat earthy basis, dull and tough fracture. The exact position is a few yards east of the little transit-house. There are also other buildings in the neighbourhood. The ground rises slightly to the east, and falls abruptly to the west at a distance of about 15 yards. The immediate surface is flat, partly covered with grass, partly with gravel.

* Philosophical Transactions, 1836, p. 577.

† More lately Mr ADIE has constructed two sets of thermometers resembling these, one, extending to 24 French feet for Greenwich Observatory; the other, including only the 12 feet thermometer, for Mr CALDECOTT of Trevandrum in India. Both of these sets of instruments were fortunately transported to their destinations without any accident. The graduation of the Greenwich instruments was performed by myself, and a much larger number of points fixed than above described. The result was examined, and the scale determined, by a simple method of graphical projection and interpolation, which led to the most satisfactory results; I should, therefore, recommend this method to others undertaking the same tedious and difficult operation.

2. In the Experimental Garden, adjoining the Royal Botanic Garden at Inverleith, almost exactly 1 mile NW. of the Observatory, and 280 feet lower, being about 70 feet above the sea. The soil here is a remarkably pure sand, resembling sea-sand, extending to a great depth, and including few pebbles of any size. The precise locality was the flat summit of the rising ground, immediately to the south of the large building or show-room in the Garden. The perforation of the sand was exceedingly easy, owing to the uniformity and dryness of the strata. The four thermometers were inserted in three boxes, near to one another, the two shortest thermometers being placed together in the same hole. The *surface* is garden mould, whereas, at the Observatory and Craigleith, it is covered with vegetation (in the former case interrupted by buildings and gravel walks). This circumstance may not be without some effect. Sir JOHN LESLIE has observed* that cold penetrates deeper through bare soil, or compact pavement, than through turf.

3. At Craigleith quarry, $2\frac{1}{4}$ miles west from the Observatory, in a mass of coal formation sandstone, which has for many years afforded an abundant and durable building material for the city of Edinburgh. The spot chosen was situated in a field 50 yards north of the house called Craigleith Hill, immediately to the east of the quarry, and about 75 yards distant from its north escarpment. The field was under grass during the first two or three years of the observations, afterwards under crop. The height above the sea is about 150 feet. The thermometers were inserted here, as at the Observatory, in one hole, six or seven inches in diameter at the top, and three at the bottom, which it required several weeks to form with boring-irons in the usual manner. When the hole was empty some water always flowed into it, and stood at a certain height, however often removed.

The insertion of the thermometers into the holes required the greatest precaution; the length and flexibility of the stems of the longest exposing them to great risk of casualty. The operation was managed in the following manner. A strong tripod, 12 or 15 feet high, was erected over the hole, and a ladder still longer attached it, so that a man ascending it could command completely the upper part of the instrument. The tube lay in the angle formed by two united pieces of wood, similar to a roof gutter, where it was secured by loops of string. Being raised, with this defence, into an erect position directly over the hole, the loops were successively cut, and the thermometer allowed to slip from the wooden shield, and to sink to the required depth. Dry sand was then poured in to half the depth of the hole. The second thermometer was then similarly planted, and so of the others; the aperture being well closed round the tubes with clay puddle.†

* Encyc. Brit., Article CLIMATE.

† Subsequently (May 1838), a quantity of Roman cement was employed to secure completely the opening of the holes.

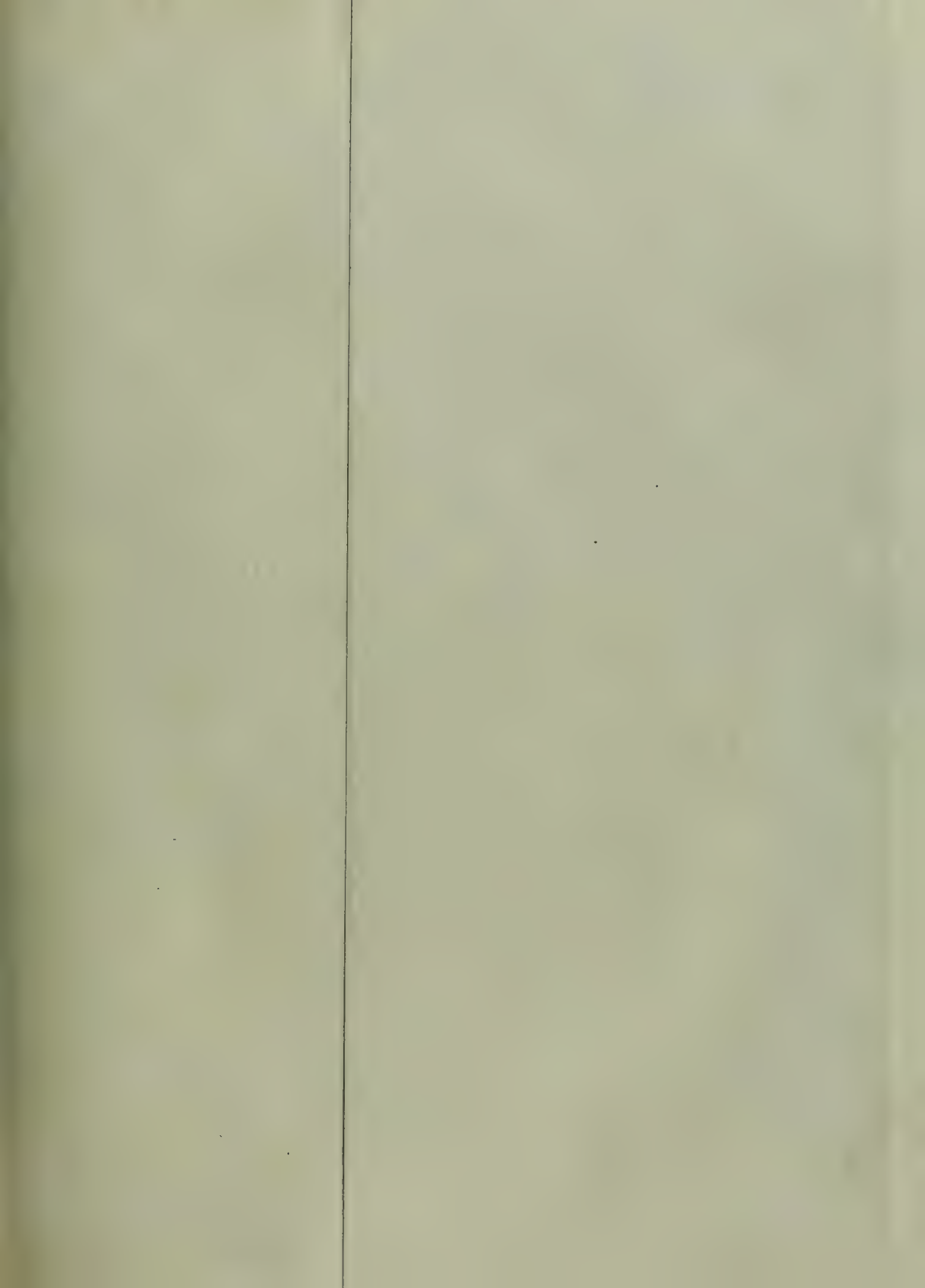
Strong wooden boxes painted green, having doors open to the north side, were then firmly fixed over the projecting stems, which boxes were afterwards pierced with holes, in order to secure a free ventilation. Small thermometers graduated to whole degrees were hung in air within the boxes; and afterwards (May 1838) other thermometers were placed with their bulbs just covered by the soil within the boxes. These last thermometers are referred to in the tables as t_5 . After the instruments were finally placed, slight metal scales were attached to the respective tubes with fine copper wire. I should add that, for the defence of the bulbs and the capillary tubes, at their inferior extremities, before mentioned, they were half inserted into tin cylindrical boxes filled up with plaster of Paris.

I had hoped to have commenced the observations with 1st January 1837; the unexpected difficulty experienced in boring the holes, and subsequently the severe weather, prevented the insertion of the thermometers before the 18th January in the Experimental Garden, the 20th on the Calton Hill, and the 21st at Craighleith Quarry; the whole was happily accomplished without the slightest accident. In all my arrangements, I was aided by the civility of the Directors of the Astronomical Institution and of the Experimental Garden, by the astronomer Mr HENDERSON, and other official persons.

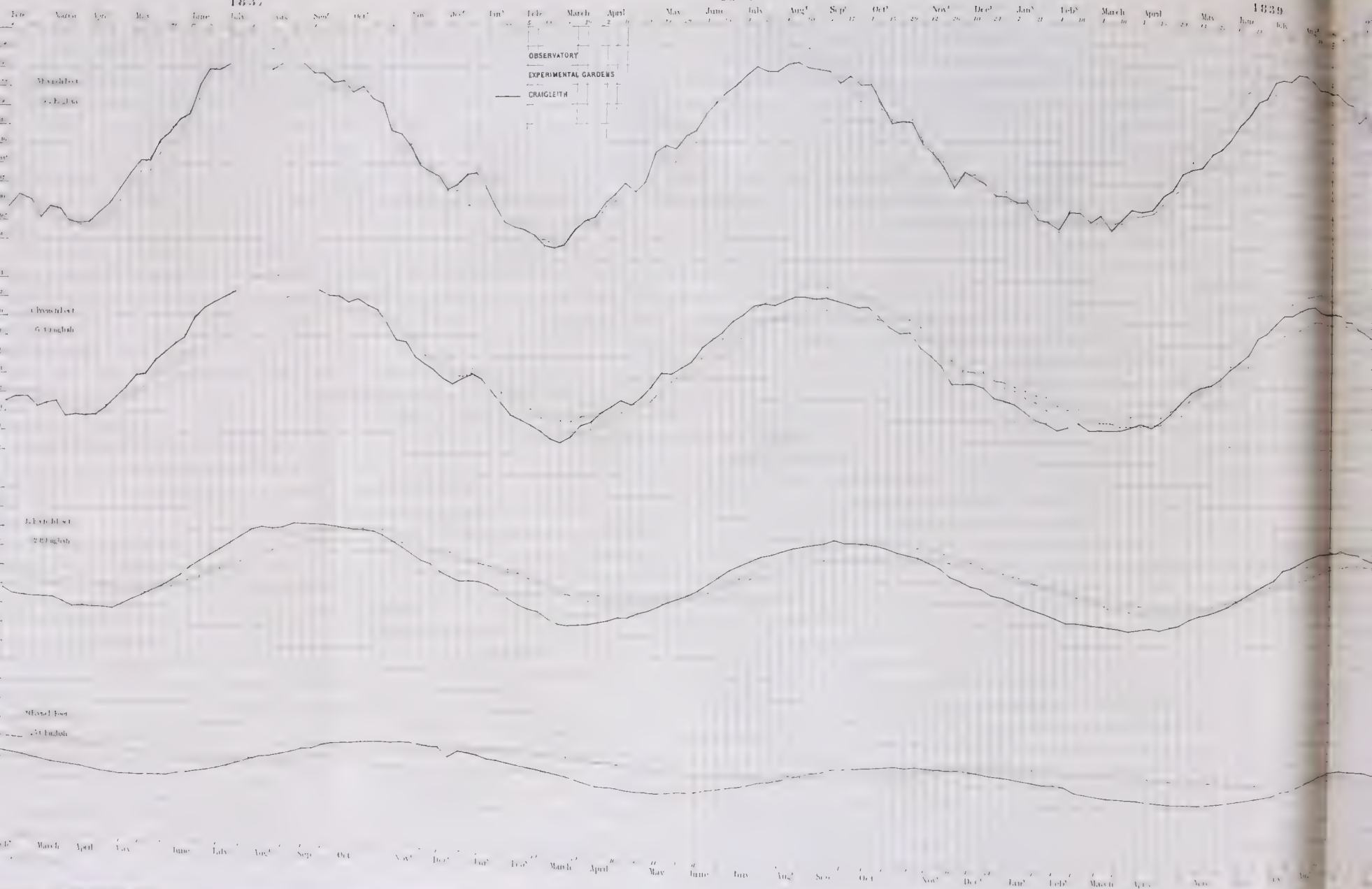
V. *Observations and Observers.*

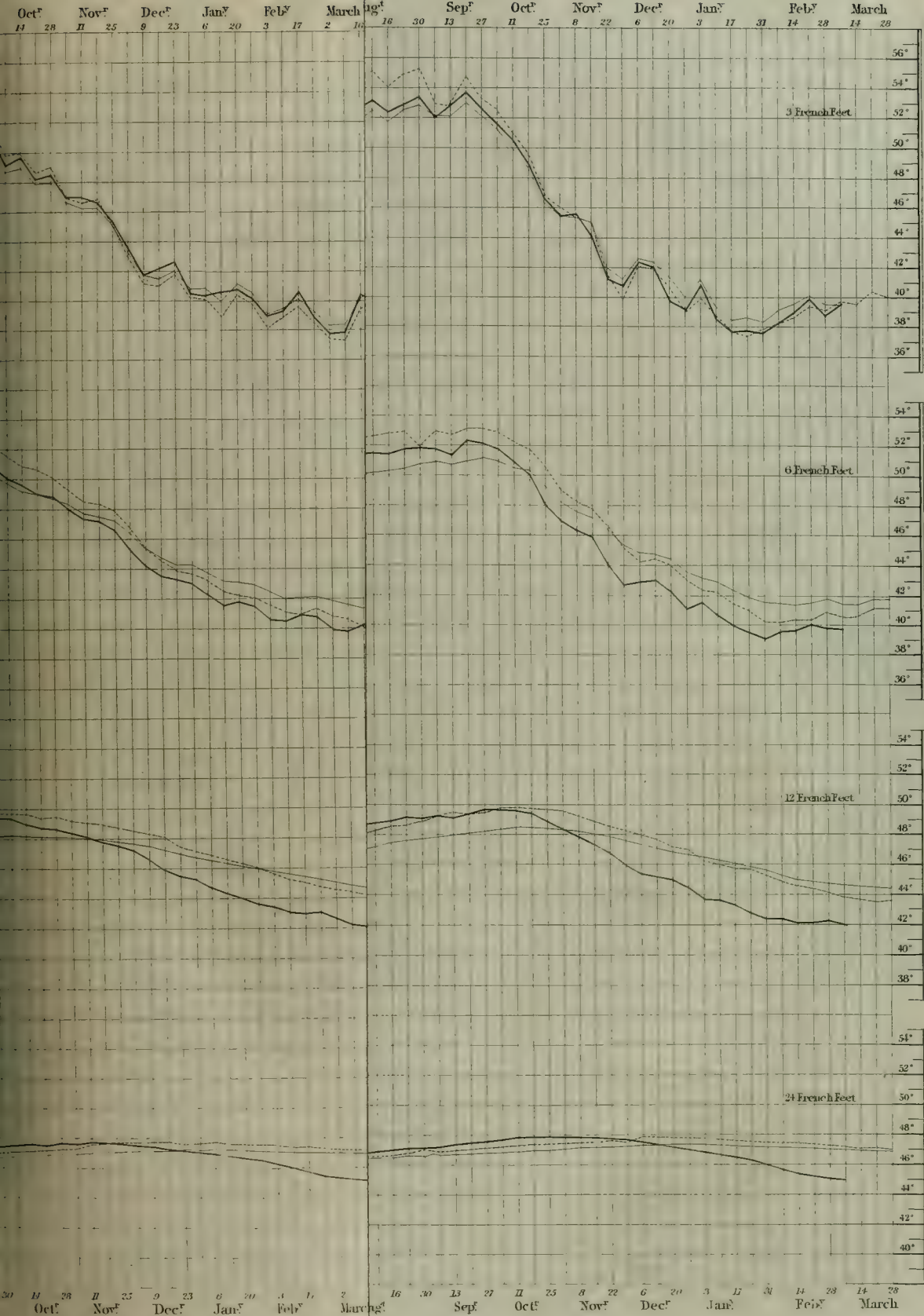
Not the least of the difficulties of carrying on such observations as the present has ever been found to be, that of getting perfectly trustworthy and zealous observers. In this matter I esteem myself particularly fortunate. Professor HENDERSON undertook, in the kindest manner, the personal superintendence of the thermometer placed on the Calton Hill in the Observatory grounds. At the Experimental Garden I received the services of Mr JAMES McNAB, the superintendent; and at Craighleith, those of Mr MACKINTOSH, whose official connection with the quarry ensures his constant residence. The observations were made weekly, and were registered in degrees and hundredths of Fahrenheit's scale (by approximation). The general superintendence which I have been able to give, assured me that all the observations were made, not only with fidelity, but without any sensible error arising from want of familiarity of two of the observers with instruments so minutely divided. And, were other proofs wanting, fortunately the ultimate projection and comparison of the three sets of observations affords the most perfect check upon any considerable inaccuracy, either in the observations or computations.

With a view to check any permanent change in the reading of the instruments, such as might arise from a permanent displacement of the freezing point, I had a spirit thermometer constructed with a bulb similar to those buried, with



1839

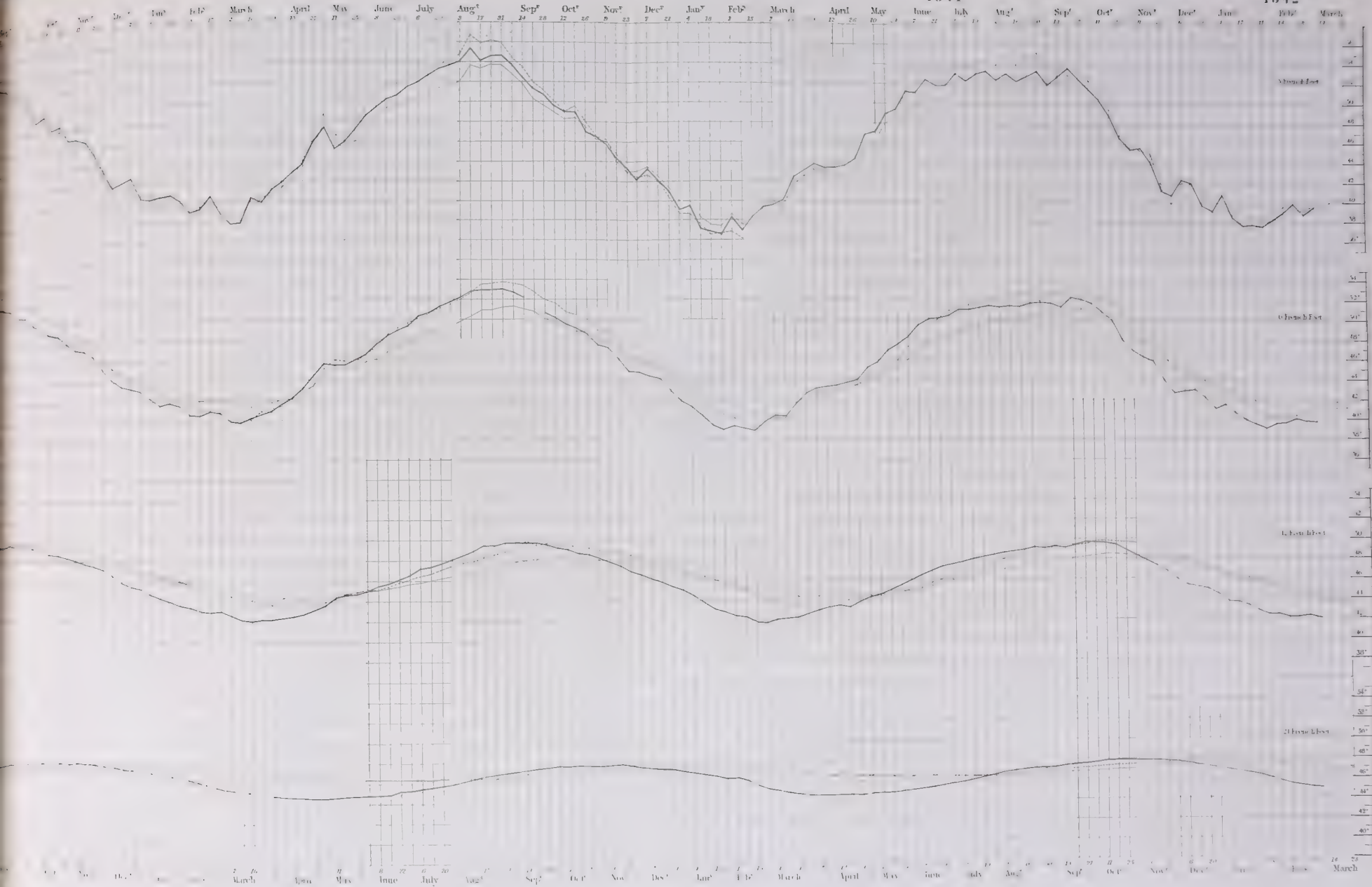




1840

1841

1842



a view to its occasional verification ; the freezing point being accurately ascertained. Nine years after (January 1846), this instrument being re-examined, shewed no appreciable change in the position of the zero point. It could not have amounted to $\frac{1}{10}$ of a degree. A common thermometer would have altered appreciably under similar circumstances. Is not this owing to the strength of the glass bulb ?

The permanent influence of the pressure of sand in the holes was suggested to me as a possible source of a change of figure of the instruments ; but the conclusive experiments lately made in America and France on the pressure of sand, convinced me that this could have no appreciable influence.

The observations were made weekly ; generally on Mondays. As it was commonly supposed that the diurnal variations of the temperature disappear at the depth of three feet, I did not take particular precautions to have all the observations made at the same hour. I find, however, that, at the Observatory, the readings were taken immediately after noon, at the Experimental Garden, about 2 o'clock, at Craighleith Quarry, regularly between 11 and 12 o'clock. The later hour of the second series may account for some irregularities observable. The observations, at all the stations, were made regularly from February 1837 down to May 1842, about which time, the thermometers at Craighleith were maliciously destroyed ; but these five years of complete observations have yielded all the results which were looked for in commencing them. The boxes covering the thermometers, in the Experimental Garden, were blown over in the winter 1844-5, crushing the thermometers. Those at the Observatory still remain in good order, and are regularly observed. The numbers, from the original registers, are exactly given in the table at end of this paper, together with the corrections applied to them, in the manner to be described in the next section. It is not to be supposed that the registers are without some errors, at least, in the case of the two less experienced observers (at Experimental Garden and Craighleith) ; but they are only oversights of the eye or hand, which can affect none of the conclusions, as the admirable coincidence of the three independent series, in Plate VII., sufficiently proves.

The following table contains the data necessary to be known, respecting the scales and dimensions of the thermometers, for correcting the temperature of the stems and exposed columns in the manner which we shall next proceed to investigate.

TABLE III. THERMOMETRIC CONSTANTS.

No.	Range of Wide Tube.	Length of 1° in inches.	Inches of Fine Tube above ground.	Inches of Wide Tube in Fine Tube.	Degrees in Fine Tube.	Length of Fine Tube in inches.	Length of 1° in inches of Fine Tube.	Degrees of Fine Tube sunk below ground.	Degree from which the column exposed to external temperature is reckoned.*
OBSERVATORY.									
1	41·6—51·0	1·79	9	24·5	13·7	316	23	13·3	41
2	41·0—51·0	1·57	9	7·0	4·5	162	36	4·2	40·5
3	36·0—54·0	0·88	9½	4·2	4·8	86	18	4·3	35
4	30·5—55·0	0·63	9½	3·6	5·7	48	8·4	4·6	28·5
EXPERIMENTAL GARDEN.									
1	39·5—52·9	1·225	8	30·0	24·5	315	12·8	23·9	38
2	38·5—53·2	1·09	6½	9·2	8·4	160	19	8·1	38
3	35·0—55·9	0·68	10	4·2	6·2	87	14	5·5	33·5
4	26·5—60·0	0·475	10	1·8	3·8	48	12·6	3·0	25
CRAIGLEITH.									
1	40·5—51·5	1·575	9	19·7	12·5	316	25·2	12·1	40
2	40·7—50·7	1·52	8½	4·9	3·2	162	51	3·0	40
3	37·0—53·6	0·98	10	6·4	6·5	87	13·4	5·7	35·5
4	31·5—55·4	0·67	10	1·4	2·1	48	22·8	1·7	31
<p>* This supposes the Atmospheric Temperature to penetrate 9 inches. The tubes at the surface would have the following readings:—</p> <p>41·2 40·7 35·5 29·4 38·9 38·2 34·3 25·7 40·1 40·5 36·2 31·1</p>									
<p>May 14. 1834. No. 4. Observatory was lowered 3°·12, by withdrawing alcohol. May 15, No. 4. Craigleith lowered by 1°·95. Corrected scales were immediately applied.</p> <p>In the above Table, No. 1 is the <i>longest</i>, No. 4 the <i>shortest</i>, Thermometer.</p>									

VI. Corrections of the Observations.

It is very evident that the readings of the thermometers cannot indicate exactly the temperature of the point corresponding to the centre of the bulb, because the stem between that point and the surface of the ground never has a uniform temperature throughout; and the portion of the column above ground is affected by the temperature of the air at the moment. Of these two corrections in *our* thermometers, the latter is by much the most important, which is fortunate, because it is also determined with the greatest accuracy.

These corrections, however obvious, have, according to M. QUETELET, been

overlooked by all observers previous to himself and M. ARAGO. It seems that M. ARAGO ascertains the expansion of the buried column of spirit by sinking, alongside of each thermometer, a stem similar to its own, having a scale above ground, but no bulb: the variations of bulk of the contained spirit being thus directly shewn are eliminable from the readings of the adjacent thermometer. As far as regards the correction due to the portion of the stem buried in the earth, this mode of correction is ingenious and satisfactory; but, when the tubes are capillary, the reduction is so small that it may readily be obtained otherwise, with sufficient accuracy. It does not, however, apply to the portion of the scale above ground, since the quantity of alcohol, so exposed, varies with the degree shewn by the thermometer. And this correction, as has been said, is, in our observations, the more important of the two. The method which I propose to employ is the following:—



The distribution of the thermometers, in geometrical progression, enables us to employ the temperatures indicated by the thermometers, Nos. 2, 3, and 4, for the correction of the reading of No. 1 (that is, to reduce the temperature of the column *ab* to the temperature of the bulb); the temperature of Nos. 3 and 4 to correct No. 2; and of No. 4 to correct No. 3. It was matter of consideration (1.), how this might be most correctly done; and (2.), to select a formula of sufficient (not superfluous) accuracy, and adapted to calculation.

The mode of doing this was partly arbitrary, and justified by application to cases where the variation of temperature in the stems was a maximum selected from the journals of observation. Thus, the depth of the successive thermometers being—

$$0, \quad a, \quad 2a, \quad 2^2 a, \quad 2^3 a, \quad \&c.$$

The intervals of depth are—

$$a \quad a \quad 2a \quad 2^2 a \quad \&c.$$

And the product of the temperature and depth must lie between two series, one of which supposes the temperature of any interval equal to the temperature of the thermometer at its superior limit, the other supposes the mean temperature equal to that at its inferior limit. It is evident that the truth must lie between these suppositions, or that denoting by *T*, the superficial temperature; and *t*₄, *t*₃, *t*₂, *t*₁, the indications of each thermometer successively in descending, we must

have the product of the temperature and depth between the values of the two series,

$$T \cdot a + t_4 a + 2 t_3 a + 2^2 t_2 a$$

$$t_4 a + t_3 a + 2 t_2 a + 2^2 t_1 a$$

and

Farther, it will be nearer the latter result than the former, since the variation of temperature diminishes as we descend.

Until after May 1838, no superficial thermometer (or one in the uppermost stratum of soil) was used. There was simply a thermometer suspended beside the scales in the box, to indicate the temperature of the part of the column exposed to the air. Fortunately, however, the correction for the temperature of the first interval α , or 3 feet, is very small indeed. The extreme excess of temperature of the air in the box above the highest thermometer, or $(T - t_1)$ during 1837, was 20° Fahr. In the most capacious of the tubes employed, supposing that the temperature of 20° had been applied through the whole depth of 3 feet, an expansion would have been produced, which would have raised the alcohol on the scale of that thermometer by 0.07 ; but the expansion could not possibly amount to half of this, seeing that the mean temperature of the 3 feet of soil would more nearly approach to that of the inferior limit of it, than to that of the air in contact with its surface. We can hardly err $.01$ (a quantity in this particular case much less than the errors of reading), by assuming that $\frac{1}{4}$ of the column of 3 feet had the temperature of the air, and the remainder that of the thermometer bulb itself.* In both the other three-foot thermometers, the error, owing to the smaller capacity of the three-foot capillary tube, would be but about half as great.

Now, by reasoning by the method of limits as above, and applying the above correction to the upper 9 inches of all the tubes, I find the following formula to be a more than sufficient approximation in every case. For the deepest thermometer, No. 1, whose temperature is t_1

$$\text{Mean temperature of stem} = \frac{\Sigma \text{Temperatures} \times \text{depths}}{\text{depth}}$$

$$\text{making } 3.2 \text{ feet} = \text{unit of depth,}$$

$$\text{mean temperature} = \frac{1 \cdot t_4 + 2 \cdot t_3 + 4 \cdot t_2 + 8 \cdot t_1}{8} \quad \dots \quad (1.)$$

And as the reduction of the temperature of the stem to that of the bulb depends on the excess of the former above the latter, we have

$$\text{Mean excess of temperature} = \frac{(t_4 - t_1) + 2(t_3 - t_1) + 4(t_2 - t_1)}{8} \quad \dots \quad (2.)$$

Farther, to adapt this to calculation, let the successive intervals of temperature of the series of thermometers be taken, and make

$$t_2 - t_1 = a$$

$$t_3 - t_2 = b$$

$$t_4 - t_3 = c$$

the above expression becomes

$$\frac{1}{8} \{ (a + b + c) + 2(a + b) + 4a \} \\ = \frac{1}{8} \{ 7a + 3b + c \} \text{ for thermometer No. 1, } \dots \quad (3.)$$

* The application of this correction becomes exceedingly easy, by considering the correction for air temperature to apply, not only to the exposed part of the tube, but also to the first 9 inches of the buried stem.

For the other thermometers, we have only to make a and b successively = 0, and substitute 4 and 2 for the depths, which give

$$\frac{1}{4}\{3b+c\} \text{ for No. 2.}$$

$$\frac{1}{2}c \text{ for No. 3.}$$

And the correction for No. 4 will be exclusively that derived from the observation of the thermometer in air T , and has for its argument

$$\frac{1}{4}\{T-t_4\} \dots \dots \dots (4.)$$

I should have observed, that, in order to ascertain that these formulæ represented the state of the instruments with sufficient accuracy, I first calculated how nearly the mean temperature of the whole column of each thermometer must be known, in order to entail no greater error than that of the reading, say of $\cdot 01$ degree. This, in the case of the deepest (26 feet) thermometer, with the widest bore, amounts to 1° of temperature, and in the three-feet thermometer to 3° .

For the second or Air Temperature correction, the quantity of alcohol to be expanded, depends on the height at which the liquid stands in the tube, and the amount of expansion on the temperature to which it is subjected.

Let us suppose, that in any thermometer the degree of temperature is known at which the surface of the column would just contract below the level of the soil. The number of degrees above this, which the thermometer at any time marks, points out the quantity to be corrected for expansion. If this correction is also to be applied to a part of the tube 9 inches lower, we have only to start from a degree of the scale corresponding to that point instead of to the surface. The number of degrees for which it is to be corrected, is the excess of the temperature of the air above that of the bulb, or $T-t_n$, t_n denoting the temperature shewn by the n^{th} . thermometer in an ascending order. Table III. in page 198, gives the point on the scale of each thermometer, corresponding to a position 9 inches below the level of the soil; let that point be l_1, l_2, l_3, l_4 , for each thermometer in succession, then the number of degrees of temperature to be corrected for, will be $T-l_1, T-l_2$, &c.

Thus, both the corrections required to reduce the observed readings amount to finding by a table, the increased (or diminished) length of a given column of alcohol (measured in degrees), for a given excess (or defect) of temperature, assigned in degrees. Such a table I have constructed, and I have thought it advisable to employ the correct value of the expansion of alcohol at atmospheric temperatures, instead of its *mean* amount between the freezing and boiling points. This latter quantity as given by DALTON, and commonly employed, is $\cdot 11$ of the volume, from 32° to 212° , or $\cdot 000611$ for 1° Fahr. Now, it appears from MUNCKE'S elaborate experiments, that alcohol, of density $\cdot 808$, expands at common atmospheric temperatures (viz. between 0° and 20° cent.), almost precisely $\cdot 001$ of its volume

at freezing for 1° cent.,* or $\cdot 000555$ for 1° Fahr., a quantity *one-tenth* greater than its mean dilatation usually adopted. I accordingly had a Table constructed on this basis, with a double entry, one for the number of degrees, or space in the tube filled with the expanded liquid, and the other for the excess of temperature to which it is exposed. The arguments used for the two corrections, with this Table, were the following :—

	1st Argument. Capacity in Degrees to be corrected.	2d Argument. Degrees of Temperature to be corrected for
1st Correction (for Temperature of Stem),	Table, p. 198.	$\frac{7a + 3b + c}{8}$
2d Correction (for Temperature of Air),	$T - t_1$	$T - t_1$

The sign of the correction is always *opposite to that of the second argument*.

* FECHNER's Repertorium, II. 430. The expansion of absolute alcohol is somewhat greater as given by MUNCKE, in a paper in the Petersburg Transactions, read 5th September 1834.

TABLE X. Corrections in Degrees for the Expansion of Alcohol in Glass Tubes = $\cdot 000555$ for 1° Fahr.

Excess of Temp.	DEGREES OF TUBE TO BE CORRECTED.																													
	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	11°	12°	13°	14°	15°	16°	17°	18°	19°	20°	21°	22°	23°	24°	25°	26°	27°	29°	30°	
1°	00	00	00	00	00	00	00	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	02	02
2°	00	00	00	00	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	03
3°	00	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
4°	00	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
5°	00	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
6°	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
7°	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
8°	00	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
9°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
10°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
11°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
12°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
13°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
14°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
15°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
16°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
17°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
18°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
19°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
20°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
21°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
22°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
23°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
24°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
25°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
26°	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	01	02	03	04
27°	02	03	05	06	07	09	11	12	14	16	17	18	19	20	22	23	24	26	27	29	30	32	33	35	36	38	39	40	42	43
28°	02	03	05	06	08	10	11	13	15	17	18	19	20	22	23	25	26	28	30	31	33	34	36	37	39	40	42	44	45	47
29°	02	03	05	06	08	10	11	13	15	16	18	19	21	23	24	26	27	29	31	32	34	35	37	38	40	42	44	45	47	48
30°	02	03	05	07	08	10	12	14	16	17	18	20	22	23	25	27	28	30	32	33	35	36	38	39	41	42	44	45	47	48
31°	02	04	05	07	09	11	12	14	16	17	19	21	22	24	26	27	29	31	33	35	36	38	40	42	44	45	47	48	50	52
32°	02	04	05	07	09	11	12	14	16	18	20	21	23	25	27	28	30	32	34	36	38	40	42	44	45	47	48	50	52	54
33°	02	04	05	08	09	11	13	15	17	19	21	22	24	26	28	29	31	33	35	37	39	41	42	44	46	48	50	52	54	55
34°	02	04	05	08	09	11	13	15	17	19	21	23	25	26	28	30	32	34	36	38	40	42	43	45	47	49	51	53	55	57
35°	02	04	06	08	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40	42	44	46	48	50	52	54	56	58	60
36°	02	04	06	08	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40	42	44	46	48	50	52	54	56	58	60
37°	02	04	06	08	11	13	15	17	19	21	23	25	27	29	31	33	35	37	39	41	43	45	47	49	51	53	55	57	59	61
38°	02	04	06	08	11	13	15	17	19	21	23	25	27	30	32	34	36	38	40	42	44	46	48	50	52	54	56	58	60	62
39°	02	04	07	09	11	13	15	18	20	22	24	26	28	31	33	35	37	39	41	43	45	47	49	51	53	55	57	59	61	63
40°	02	04	07	09	11	13	15	18	20	22	24	26	28	31	33	36	38	40	42	44	46	48	50	52	54	56	58	60	62	64

VII. *Results of the Observations.*A. *Mean Temperature at different depths.*

It has been observed at Brussels and elsewhere,* that, even at depths less than 25 feet, the mean annual temperature indicated by the lower thermometers is *greater* than that of those nearer the surface. This appears to be also clearly established by the observations at the three stations near Edinburgh, as contained in the following Table.

TABLE V. MEAN TEMPERATURE FOR FIVE YEARS.

	3 French Feet, (No. 4).			6 French Feet, (No. 3).			12 French Feet, (No. 2).			24 French Feet, (No. 1).		
	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.
1837-1838	45°94	46°54	46°26	46°23	46°70	46°50	46°65	46°90	46°30	47°22	47°28	46°46
1838-1839	44°81	45°44	45°22	45°35	45°90	45°35	46°12	46°44	45°47	46°89	47°05	45°97
1839-1840	45°48	46°14	45°83	45°75	46°43	45°73	46°18	46°67	45°70	46°68	46°98	45°87
1840-1841	45°56	46°26	45°97	45°99	46°58	46°01	46°43	46°91	46°08	46°77	47°04	46°18
1841-1842	45°66	46°29	46°13	45°99	46°51	46°01	46°44	46°89	46°07	46°79	47°08	45°87
Means	45°49	46°13	45°88	45°86	46°42	45°92	46°36	46°76	45°92	46°87	47°09	46°07
Mean of Observatory,	46°14	Elevation,	.	.	.	350 feet.
— Experimental Garden,	46°60	"	.	.	.	70 feet.
— Craigleith,	45°95	"	.	.	.	150 feet.
Mean Temperature of Air from Mr Adie's Observations,	45°28	"	.	.	.	240 feet.

Mean of 3 Feet Thermometers,	.	.	.	45°83
" 6	"	.	.	46°07
" 12	"	.	.	46°36
" 24	"	.	.	46°68

The cause of the increased mean temperature below, is by no means clear. From its irregularity, it is most probably due to several causes, of which the central heat of the earth is perhaps one; its effect at 25 feet need not be insignificant, since the average rate of increase at great depths is 1° Fahr. for from 40 to 50 English feet. In the present case, the increase is not uniform, and it is also decidedly different in the different soils, and will appear by the sequel to be intimately connected with the conducting power of the strata. The order of magnitude of the increase is this, 1. Observatory, 2. Experimental Garden, 3. Craigleith; —or, Trap, Sand, Sandstone, which is also the order of the conducting power. The following table shews that, in every instance but one (from 6 feet to 12 feet at Craigleith), the increase was apparent.

* QUETELET, *Annales de l'Observatoire Royale de Bruxelles*, iv. 150.

TABLE VI. SHEWING THE RATE OF INCREASE OF TEMPERATURE WITH DEPTH.

	Observatory.	Experimental Garden.	Craigleith.
From 3 to 6 feet,	+ 0 ^o ·37	+ 0 ^o ·29	+ 0 ^o ·04
... 3 to 12 ...	+ 0·87	+ 0·63	+ 0·04
... 3 to 24 ...	+ 0·38	+ 0·96	+ 0·19

To complete the comparison of Meteorological data, I subjoin Mr ADIE's observations on the mean temperature of the air and the quantity of rain fallen during the same period, at Canaan Cottage, near Edinburgh, 240 feet above the sea, excepting the months previous to May 1838, which were observed on the Regent Terrace, Calton Hill, at about the same elevation above the sea.

TABLE VII. MEAN TEMPERATURE AND QUANTITY OF RAIN FOR THE YEARS 1837-38-39-40-41-42, OBSERVED AT EDINBURGH BY MR ADIE.

	1837.			1838.			1839.	
	Thermometer.	Rain.		Thermometer.	Rain.		Thermometer.	Rain.
January	36·33	1·23	January	31·73	2·47	January	33·05	1·76
February	37·23	2·14	February	30·06	1·21	February	38·53	1·45
March	35·24	1·28	March	38·12	2·76	March	36·98	1·47
April	39·65	1·61	April	40·25	1·78	April	42·53	0·33
May	48·37	1·53	May	44·87	2·90	May	46·82	0·47
June	57·30	2·86	June	53·98	5·16	June	53·42	3·91
July	60·42	4·54	July	58·94	2·45	July	57·77	3·51
August	51·77	4·13	August	56·88	2·99	August	55·51	1·77
September	53·18	1·73	September	52·04	4·00	September	52·13	3·09
October	50·17	2·02	October	46·27	1·15	October	46·50	2·38
November	40·45	2·03	November	38·38	3·06	November	43·13	1·65
December	42·68	1·67	December	38·17	0·73	December	37·46	1·66
Sums	552·79	26·77		529·69	30·86		543·83	23·45
Mean	46·07			44·14			45·32	

	1840.			1841.			1842.	
	Thermometer.	Rain.		Thermometer.	Rain.		Thermometer.	Rain.
January	38·74	3·72	January	33·00	1·23	January	35·45	1·01
February	36·55	1·58	February	38·39	1·66	February	39·55	1·11
March	42·74	0·43	March	45·62	0·60	March	42·04	3·44
April	48·16	0·19	April	44·26	1·14	April	45·03	0·15
May	47·13	3·99	May	51·74	1·14	May	51·22	1·45
June	52·53	2·51	June	52·43	1·36	June	57·53	0·97
July	52·75	3·46	July	53·58	3·87	July		
August	44·51	1·99	August	53·88	3·64	August		
September	48·57	2·39	September	54·36	2·63	September		
October	44·32	2·01	October	43·48	4·53	October		
November	48·66	2·33	November	39·10	2·28	November		
December	37·31	0·68	December	39·65	1·96	December		
Sums	541·97	25·28		549·53	26·04		270·82	8·13
Mean	45·16			45·79			45·14	

B. General Observations on the Thermometric Curves.

Some of the most important results depend upon the annual ranges of temperature at different depths. But the determination of the extremes is no easy matter. I will first direct attention to the curves in Plate VII., which convey a great deal of valuable information, which can here be only slightly touched upon. They are reduced to *one-sixth* of the size of the original projections, in which one degree occupied two-thirds of an inch vertically, and one day occupied one-tenth of an inch horizontally. The *corrected* temperatures are those which have been projected.

The curves extend over five years; and are placed in the order of depths (vertically) to which they belong: the uppermost undulating curves shewing the variations at the three stations 3 French feet below the surface, the lowest set shewing the variations 24 French feet below the surface.

The most obvious results are the following:—

1. In the upper set of curves, though the irregularities are greatest, yet the three curves follow one another with singular fidelity throughout these irregularities. The curves separate a little in summer, and regularly in the same direction every summer, shewing the influence of exposure, the Experimental Garden being most heated, then Craigleith, and lastly the Observatory, which is also the order of the elevations of the stations above the sea. It may also be added, that the diurnal change may possibly have some slight influence upon the Experimental Garden, where the observations were made fully two hours later than at the other stations. (See Section V.)

2. As the local irregularities diminish at increasing depths, the range diminishes, and the times of maxima and minima are continually retarded.

3. At increasing depths, the curves, which followed one another so closely and exactly amidst the irregularities of temperature near the surface, *systematically separate from one another*, both owing to a variation in the range or degree of undulation of the curve, and owing to a greater or less degree of retardation in the maxima or minima of the different curves.

4. The effect last described is least sensible in comparing the observations at the Observatory and Experimental Garden, but *most* sensible if either of these be compared with the Craigleith observations, for which last the range diminishes more slowly, so that, at 24 French feet, it is about double that of either of the others, and the retardation of the maxima and minima is much less.

5. In the trap and loose sand, the range is diminished to *one-tenth part* in descending from 3 feet to 24 feet; but in the sandstone it is not quite diminished to a *fifth part*. The epoch of maximum temperature is retarded in the two former cases nearly five months, in the latter only three.

From these statements it is easy to see that the influence of the CONDUCTING

POWER OF THE DIFFERENT SOILS OR ROCKS FOR HEAT is very palpable. But to submit it to numerical calculation, a more elaborate analysis is necessary. Each year has been first considered by itself, and then the whole united.

C. Thermometric Ranges.

To ascertain the range for each year, the maximum and minimum points of the curves of each thermometer were ascertained graphically by the aid of an elastic wire, bent so as to represent a curve which should pass through the zig-zags of the temperature curve, and connect the observed points with tolerable accuracy. The points of greatest and least temperature in each year were thus represented with a certain degree of approximation, and the results are shewn in the following table.

TABLE VIII. SHEWING THE MAXIMUM AND MINIMUM TEMPERATURE AND RANGE FOR EACH OF FIVE YEARS.

		Observatory, Trap.			Experimental Garden, Sand.			Craigleith, Sandstone.		
		Maxi- mum.	Mini- mum.	Range, Fahren- heit.	Maxi- mum.	Mini- mum.	Range, Fahren- heit.	Maxi- mum.	Mini- mum.	Range, Fahren- heit.
3 FEET.	1837-38	56°25	37°30	18°95	57°20	37°55	19°65	55°90	37°65	17°25
	1838-39	53°40	35°70	17°70	55°45	35°12	20°33	53°90	35°38	18°52
	1839-40	53°05	38°10	15°55	56°50	37°50	19°00	54°30	37°85	16°45
	1840-41	53°87	38°95	14°92	56°35	38°10	18°25	55°10	38°95	16°15
	1841-42	52°85	38°88	13°97	54°50	37°85	17°65	53°15	38°25	14°90
6 FEET.	1837-38	52°30	40°40	11°90	54°65	39°70	14°95	53°80	39°90	13°09
	1838-39	50°90	39°70	11°20	53°20	38°63	14°57	52°35	38°10	14°25
	1839-40	50°97	40°65	10°32	53°67	39°70	13°97	52°53	39°20	13°33
	1840-41	51°35	41°10	10°25	53°75	40°52	13°23	53°15	40°05	13°10
	1841-42	51°07	40°78	10°29	52°95	39°55	13°40	51°90	38°95	12°95
12 FEET.	1837-38	49°40	43°90	5°50	50°65	43°10	7°55	51°10	41°70	9°40
	1838-39	48°65	43°60	5°05	49°95	42°85	7°10	50°05	40°75	9°30
	1839-40	48°57	43°73	4°84	50°19	43°08	7°11	49°80	41°45	8°35
	1840-41	48°80	44°30	4°50	50°30	43°60	6°70	50°45	42°12	8°33
	1841-42	49°00	44°20	4°80	50°40	43°50	6°90	50°30	41°60	8°70
24 FEET.	1837-38	47°85	46°40	1°45	48°25	46°15	2°10	48°50	44°40	4°10
	1838-39	47°45	46°20	1°25	47°88	46°00	1°88	47°88	44°05	3°83
	1839-40	47°35	45°97	1°38	47°40	45°97	1°43	47°82	43°87	3°95
	1840-41	47°38	46°15	1°23	48°00	46°10	1°90	48°12	44°40	3°72
	1841-42	47°50	46°12	1°38	48°10	46°10	2°00	48°25	44°35	3°90

Theory shews, that the annual range ought to decrease in geometrical progression, as the depths increase uniformly. In other words, the ranges may be represented by the ordinates of a logarithmic curve. And that such is the case may be seen from the curves in Plates IX. and X., where the logarithmic curves are drawn through points so as to represent, as well as is practicable, the law of decreasing range at the different stations. These diagrams were constructed without any reference to one another; and their general coincidence is highly satisfactory.

To express the results geometrically,

$$\text{Log. } \Delta = A + Bp$$

Where Δ is the thermometric range at a depth p in French feet; A and B are constants, the second of which is always negative. These constants are important, and their determination may be considered as the *primary* object of this investigation. A is manifestly equal to the logarithm of the thermometric range at the surface, or when $p=0$; B is a constant which determines the rate of diminution of the range in the interior of the earth, being smaller in proportion as the heat penetrates more readily, or as the conductivity of the soil is greater. It was shewn by FOURIER to be directly proportional to the square root of the specific heat of the soil, and inversely as the square root of the conductivity.*

These quantities A and B have reference to the thermometric scale employed, and therefore it is convenient, in order to obtain comparable results, to use the same unit as MM. POISSON and QUETELET have done in their comparison of theory with observation, that is, the centigrade scale. For this purpose, the ranges are expressed in the following table in centigrade degrees.

TABLE IX. RANGES IN CENTIGRADE DEGREES.

	3 Feet.			6 Feet.			12 Feet.			24 Feet.		
	Observatory.	Experimental Garden.	Craig-leith.	Observatory.	Experimental Garden.	Craig-leith.	Observatory.	Experimental Garden.	Craig-leith.	Observatory.	Experimental Garden.	Craig-leith.
1837	10°53	11°23	9°58	6°61	8°30	7°72	3°05	4°19	5°22	0°80	1°16	2°28
1838	9°83	11°30	10°29	6°22	8°10	7°91	2°80	3°94	5°16	0°70	1°05	2°13
1839	8°64	10°55	9°14	5°73	7°76	7°40	2°69	3°95	4°64	0°76	0°79	2°20
1840	8°29	10°14	8°98	5°70	7°35	7°28	2°50	3°72	4°63	0°89	1°06	2°07
1841	7°79	9°80	8°28	5°71	7°45	7°20	2°66	3°83	4°83	0°76	1°11	2°16
Means,	9°02	10°60	9°25	5°99	7°79	7°50	2°74	3°93	4°89	0°78	1°03	2°17

Two results are sufficient for eliminating the constants A and B at each station, and the most probable combination may be had by the method of least squares. I have preferred, however, the graphical method already referred to for finding, by means of a diagram and a pair of proportional compasses, the logarithmic curve which best represents the observations. This being done as shewn in Plates IX. and X., the values of A and B may be deduced thus. A , as already observed, is the logarithmic range at the surface. Taking a space equal to 10° Cent. (or 18° Fahr.) in the compasses, find the depth at which the curve has this quantity for an ordinate, let p_{10} be this depth. Then, since $\text{Log. } \Delta = \text{Log. } 10 = 1$, the equation above becomes

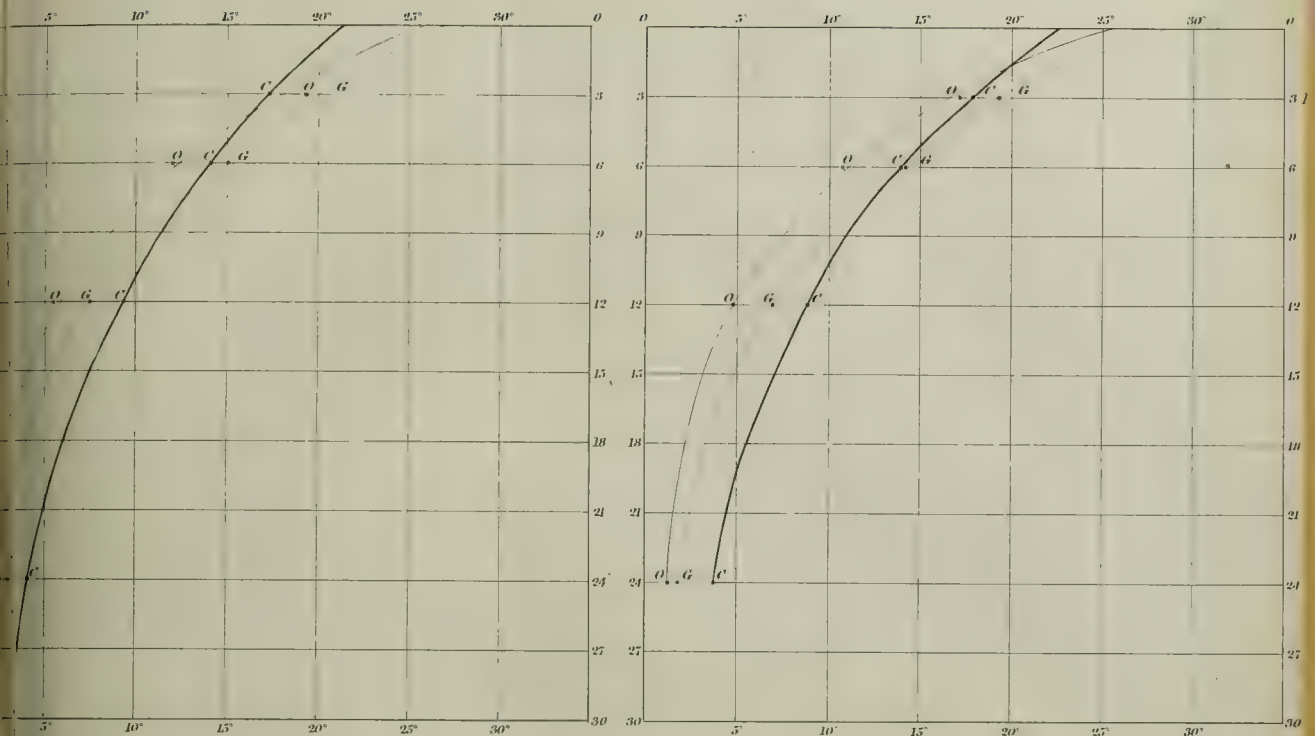
$$1 = A + Bp_{10}$$

$$\text{and } B = \frac{1 - A}{p_{10}}.$$

* For farther particulars, see the Appendix at the end of this memoir, and also Sub-Section F.

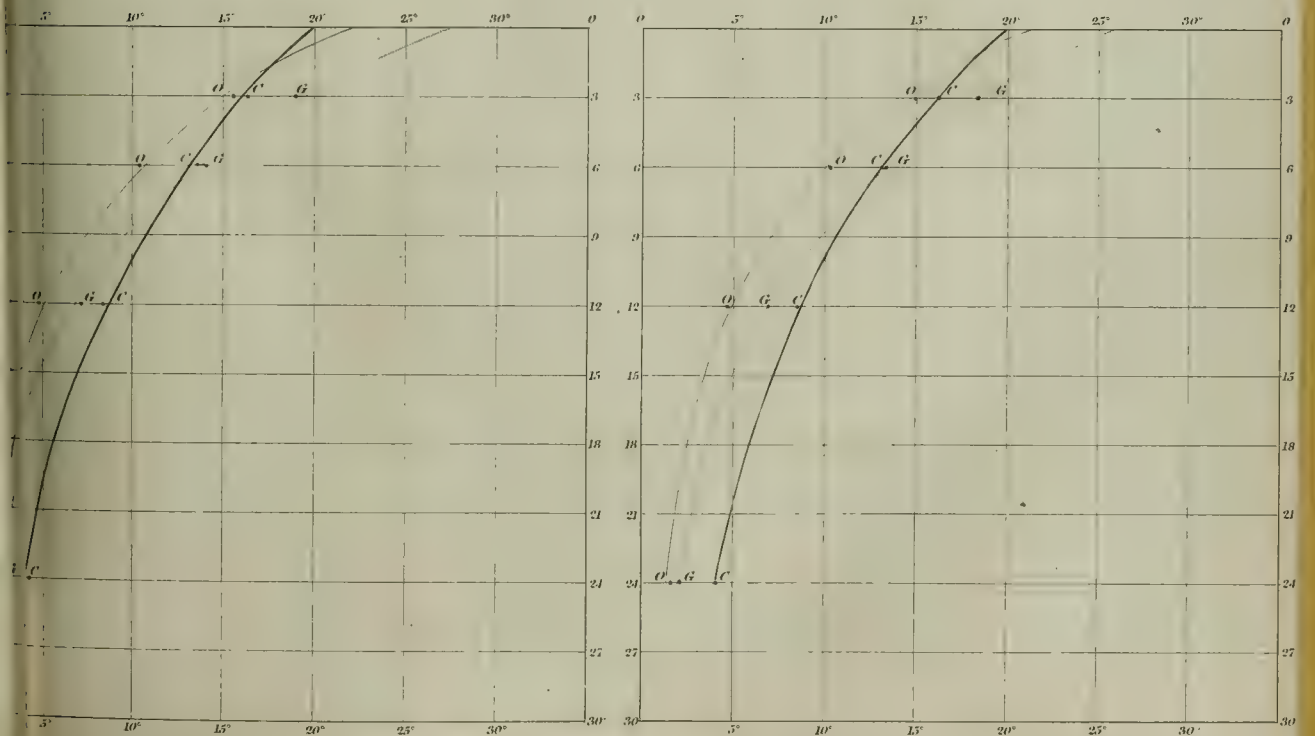
1857

1858



1859

1840



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Also p_1 , $p_{0.1}$, $p_{0.01}$ denoting the depths corresponding to a range of 1° , 0.1° , 0.01° Cent., we have

$$p_1 = -\frac{A}{B} \quad p_{0.1} = -\frac{1+A}{B} \quad p_{0.01} = -\frac{2+A}{B}.$$

Numerical Example. By the projection for 1837, Plate IX., we find

	Observatory.	Experimental Garden.	Craigleith.
The superficial range	14.6° Cent.	15.0° Cent.	11.9° Cent.
p_{10} =depth where range= 10° Cent. . .	3.0 F. ft.	4.0 F. ft.	2.4 F. ft.
Whence A	1.164	1.176	1.076
B	-0.0547	-0.0440	-0.0317
p_1	21.4 F. ft.	26.7 F. ft.	34.1 F. ft.
$p_{0.1}$	39.7 ...	49.5 ...	65.7 ...
$p_{0.01}$	58.1 ...	72.2 ...	97.3 ...

The numbers in the last line may be taken (arbitrarily) as a limit of comparison for the point at which the annual variation sensibly vanishes, and its difference in the three stations shews the marked influence of the conducting soil or rock. The following tables contain a summary of these results for five years.

TABLE X. SHEWING THE VALUES OF A AND B.*

VALUES OF A.				VALUES OF B.			
	Observatory.	Experimental Garden.	Craigleith.		Observatory.	Experimental Garden.	Craigleith.
1837	1.164	1.176	1.076	1837	-.0545	-.0440	-.0316
1838	1.173	1.217	1.114	1838	-.0641	-.0517	-.0345
1839	1.086	1.182	1.049	1839	-.0516	-.0498	-.0305
1840	1.073	1.155	1.044	1840	-.0550	-.0470	-.0308
1841	1.031	1.141	1.019	1841	-.0474	-.0460	-.0281
Means,	1.105	1.174	1.060	Means,	-.0545	-.0477	-.0311

TABLE XI. SHEWING THE DEPTHS AT WHICH THE ANNUAL RANGE IS REDUCED TO 0.01° CENT.

	Observatory.	Experimental Garden.	Craigleith.
1837	58.1	72.2	97.3
1838	49.3	61.8	91.0
1839	59.2	63.5	100.0
1840	55.9	67.1	98.8
1841	63.9	68.3	107.4
Means,	57.3	66.6	98.9

* The French foot and centigrade degree are here taken as units.

On these results it may be remarked, that A, which is the logarithm of the superficial range, is necessarily variable according to the season, and that it appears, singularly enough, to have been constantly on the decrease throughout the period of these experiments. This gives a great probability that the mean of these will be very nearly an average result for this climate. The depth at which the annual variation disappears is also evidently dependent, in part, on the quality of the season. B is the only proper constant, depending solely upon the specific heat and conductivity of the soil; and the mean results of Table X. are evidently near approximations to the truth.

These computations have been made on the supposition that the logarithmic law of the diminution of the range is correct, and that the deviations from it are due to accidental errors. These deviations appear, however, to be too systematic to admit exactly of this conclusion. The observations at Craighleith coincide most nearly with theory; those at the Observatory much less so, although there is every reason to believe that the observations there were in every respect the most unexceptionable of the three. At the Observatory, the observations at great depths indicate a less rapid contraction of the range than do those at the surface, as an inspection of the curves in Plates IX. and X., and the points through which they have been drawn, sufficiently proves.

To illustrate this difference, I had the constants A and B separately computed from all the possible combinations by pairs of the observations of 1837-38, with the following results.

TABLE XII.

	Observatory.			Experimental Garden.			Craighleith.		
	12 Feet.	6 Feet.	3 Feet.	12 Feet.	6 Feet.	3 Feet.	12 Feet.	6 Feet.	3 Feet.
Taken in combination with	24 Feet. {	A = 1.087 B = —.0484	A = 1.126 B = —.0510	A = 1.182 B = —.0533	A = 1.180 B = —.0465	A = 1.204 B = —.0475	A = 1.191 B = —.0459	A = 1.077 B = —.0300	A = 1.064 B = —.0294
	12 Feet. {	A = 1.156 B = —.0560	A = 1.202 B = —.0598 {	A = 1.216 B = —.0495	A = 1.193 B = —.0476 {	A = 1.058 B = —.0283	A = 1.069 B = —.029
	6 Feet. { {	A = 1.225 B = —.0674 { {	A = 1.182 B = —.0438 { {	A = 1.077 B = —.031
	Mean Values, . {			Mean Values, . {			Mean Values, . {		
	A = 1.1594 B = —.0560			A = 1.1943 B = —.0469			A = 1.068 B = —.029		

D. Progress of Heat downwards.

The curves of Plate VII. plainly shew that the periods of maximum and minimum temperature occur later and later as we descend. The epochs of maxima and minima were obtained graphically at the same time with the greatest and least temperature, in the manner already described. The results are contained in the following Table:—

1841

MEAN 1837-41

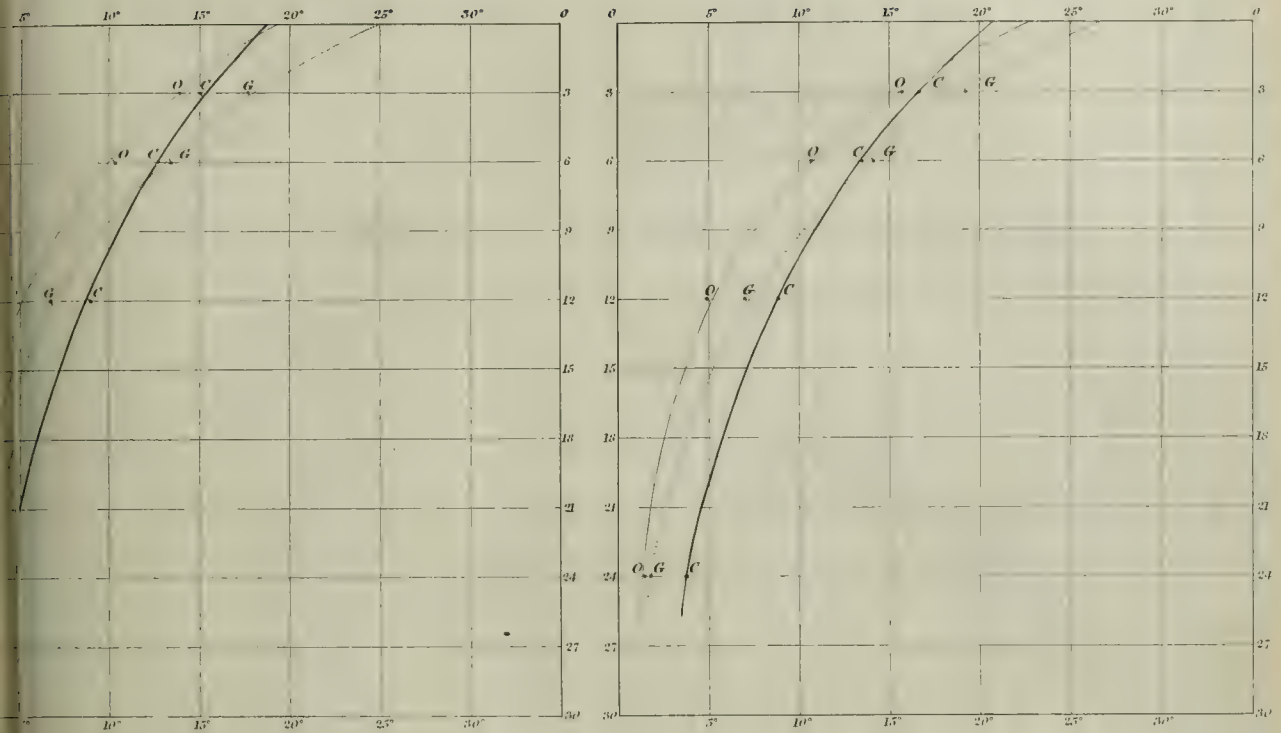
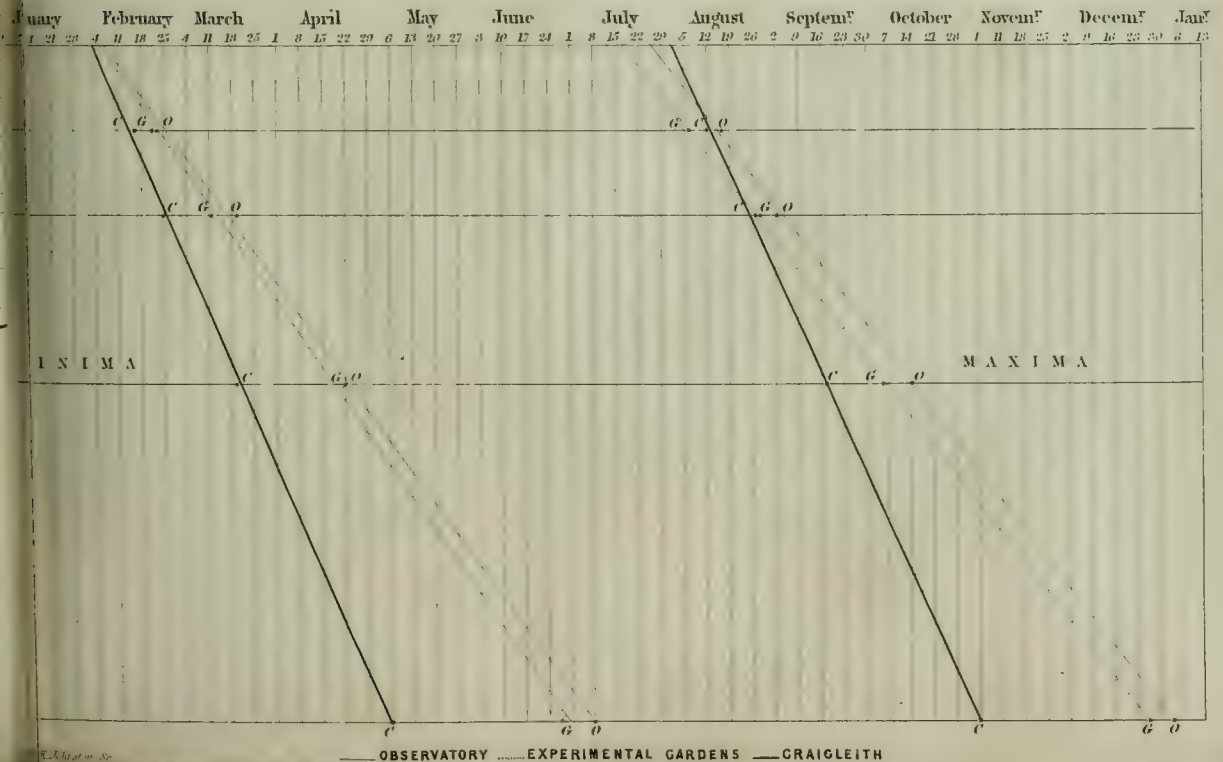


DIAGRAM SHEWING THE PROGRESS OF HEAT DOWNWARDS
BY THE MEAN OF FIVE YEARS.



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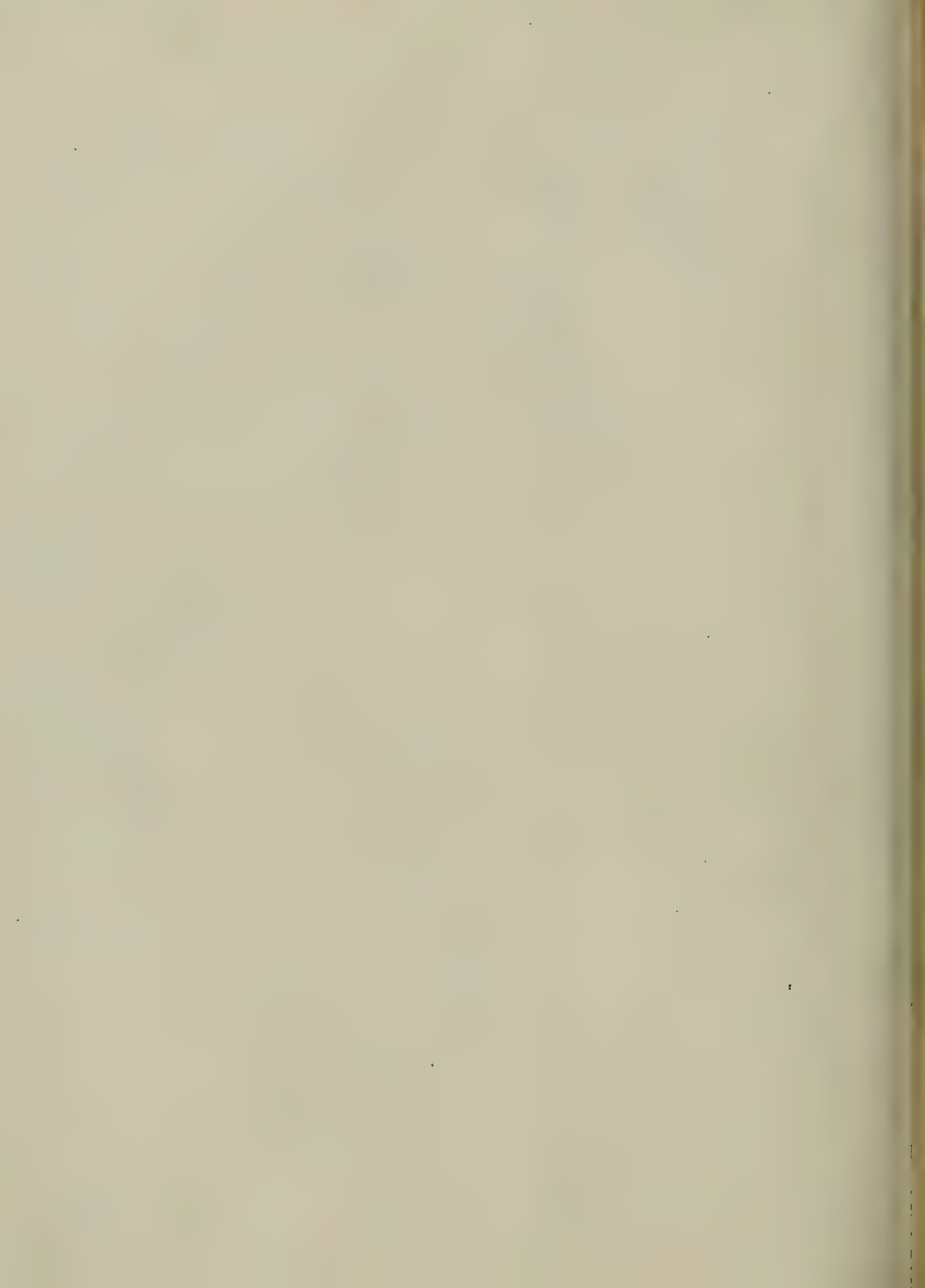


TABLE XIII. EPOCHS OF MINIMUM AND MAXIMUM TEMPERATURE (BY INTERPOLATION)
FROM 1837 to 1842.

MINIMUM.												
	3 Feet.			6 Feet.			12 Feet.			24 Feet.		
	Observa- tory.	Experi- mental Garden.	Craig- leith.	Observa- tory.	Experi- mental Garden.	Craig- leith.	Observa- tory.	Experi- mental Garden.	Craig- leith.	Observa- tory.	Experi- mental Garden.	Craig- leith.
1837							May 6	Apr. 30	Apr. 10	July 26	July 12	May 18
1838	Feb. 26	Mar. 3	Feb. 23	Mar. 14	Mar. 19	Mar. 3	Apl. 20	Apr. 22	M r. 20	July 18	July 8	May 12
1839	Mar. 14	Feb. 24	Feb. 24	Mar. 27	Mar. 25	Mar. 4	Apl. 30	Apr. 22	Apr. 1	July 12	June 24	May 12
1840	Mar. 1	Feb. 25	Mar. 1	Mar. 14	Mar. 15	Mar. 8	Apl. 19	Apr. 18	Mar. 21	July 5	June 23	Apr. 30
1841	Feb. 1	Feb. 1	Jan. 27	Feb. 17	Feb. 15	Feb. 2	Mar. 24	Mar. 20	Feb. 24	July 5	June 15	Apr. 5
1842	Jan. 25	Jan. 22	Jan. 25	Feb. 19	Feb. 15	Feb. 6						
Means	Feb. 18	Feb. 14	Feb. 13	Mar. 7	Mar. 7	Feb. 21	Apr. 20	Apr. 16	Mar. 21	July 13	June 29	May 3
MAXIMUM.												
1837	Aug. 6	July 31	Aug. 5	Sept. 2	Aug. 24	Aug. 19	Oct. 17	Oct. 6	Sept. 11	Jan. 8	Dec. 30	Nov. 11
1838	Aug. 8	Aug. 6	Aug. 16	Sept. 6	Aug. 31	Aug. 23	Oct. 19	Oct. 14	Sept. 19	Jan. 5	Jan. 4	Nov. 2
1839	Aug. 1	July 30	July 30	Aug. 26	Aug. 19	Aug. 14	Oct. 10	Oct. 3	Sept. 11	Jan. 8	Dec. 26	Nov. 4
1840	Aug. 23	Aug. 18	Aug. 18	Sept. 4	Sept. 2	Aug. 23	Oct. 6	Sept. 30	Sept. 9	Jan. 3	Dec. 18	Oct. 26
1841	Sept. 6	Aug. 23	Aug. 25	Sept. 24	Sept. 19	Sept. 15	Oct. 20	Oct. 16	Oct. 4	Dec. 29	Dec. 18	Nov. 3
Means	Aug. 15	Aug. 9	Aug. 13	Sept. 6	Sept. 1	Aug. 25	Oct. 14	Oct. 8	Sept. 17	Jan. 4	Dec. 25	Nov. 3

It will readily be understood, by the inspection of the curves, that these determinations are liable to considerable uncertainties,—in most cases amounting to several days. The curves at small depths are liable to many anomalous fluctuations, and even occasionally present an appearance of two minima; and at great depths the curves, though even, are so flat, that a considerable error may occur in detecting their highest and lowest points. It does not appear, however, that more real accuracy would be obtained by the methods of calculation which have usually been employed, instead of interpolating curves. We shall presently, however, shew how the two may be advantageously combined.

We thus see that the greatest cold of winter attains the depth of 24 French feet,—

At the Observatory (trap rock), on the 13th July;

At the Experimental Garden (loose sand), on the 29th June;

At Craigleith (sandstone), on the 3d May;

and that the greatest heat occurs on the 4th January, 25th December, 3d November respectively; shewing, in both cases, the very same order of facility in conducting heat which we had before deduced from the diminution of ranges, namely, that the Observatory ground is the worst conductor, that of the Experimental Garden but little better, and the rock at Craigleith by far the best.

Unfortunately, the measure of the retardation of epochs has, as yet, been so

imperfectly reduced to theory, that we cannot satisfactorily compare it with experiment,* but one law of great simplicity has long been known from theory to be approximately true, namely, that the retardation of epochs is *uniformly* greater as the depth increases. This is also easily verified graphically. By taking the depths in a vertical direction, and setting off the day of greatest or least temperature horizontally, a series of points is obtained through which a straight line should pass. I have not engraved these projections for *each* year, but that for the mean of the whole will be seen in the lower part of Plate X., where the interpolating lines in general pass so nearly through the dots that they cannot be distinguished. From these projections the mean rate of propagation downwards is easily determined, and affords a palpable illustration of the conducting powers of the soil.

TABLE XIV. SHewing THE NUMBER OF DAYS REQUIRED BY THE IMPRESSION OF HEAT TO PASS THROUGH ONE FOOT OF SOIL.

	MAXIMA.			MINIMA.		
	Observa- tory.	Experimen- tal Garden.	Craigleith.	Observa- tory.	Experimen- tal Garden.	Craigleith.
	Days.	Days.	Days.	Days.	Days.	Days.
1837	7.5	7.1	4.9
1838	6.8	6.8	3.6	6.5	5.8	3.6
1839	7.8	7.2	4.6	6.0	5.1	3.6
1840	6.6	5.95	3.5	6.1	5.7	3.05
1841	5.4	5.1	3.0	6.4	5.7	3.6
Means	6.82	6.43	3.92	6.25	5.58	3.46

It must be added, that in the several years the law of *uniform* progression is by no means accurate, although, in the mean of five, the accidents are nearly compensated. And here, again, we find the good conductor, the sandstone, gives by far the most regular and consistent results.

E.—On the Form of the Annual Curves.

With a view to approximate more nearly to the form of the annual curves of temperature at different depths, I have had the mean temperature for each week of the year taken by the mean of five years, which has the effect of disposing of the more irregular fluctuations, as may be seen in Plate VIII., the curves in which are taken from the following Table :

* See the Appendix.

January February March April May June July August September October November December

MEAN TEMPERATURE OF 5 YEARS (1837-42) AT DIFFERENT DEPTHS

Observatory
Exp. Garden
Craigleith

Mean Temperature

3 French Feet - 3.2 Eng.

Mean Temperature

6 French Feet - 6.4 Eng.

Mean Temperature

12 French Feet - 12.8 Eng.

21 French Feet - 25.6 Eng.

January February March April May June July August September October November December



Month.	Day.	EXPERIMENTAL GARDEN.										CATHEDRAL.			
		24 Feet.	12 Feet.	6 Feet.	3 Feet.	24 Feet.	12 Feet.	6 Feet.	3 Feet.	24 Feet.	12 Feet.	6 Feet.	3 Feet.	24 Feet.	12 Feet.
January	0.	47.47	46.95	44.07	40.99	47.98	47.21	43.46	40.39	47.09	44.92	42.47	40.54	47.09	44.92
"	7.	47.47	46.73	43.63	40.96	47.95	46.86	42.46	40.16	46.93	44.46	42.09	40.47	46.93	44.46
"	14.	47.46	46.73	43.21	40.96	47.91	46.54*	42.46	38.82	46.68	44.11	42.09	39.13	46.68	44.11
"	21.	47.43	46.26	42.69	38.95	47.90	46.45	41.84	38.19	46.51	43.66	40.55	38.28	46.51	43.66
February	23.	47.41	45.99	42.18	38.62	47.86	45.87	41.30	37.45	46.31	43.13	40.05	37.95	46.31	43.13
"	1.	47.54	45.77	41.82	38.35	47.86	45.52	40.85	37.45	46.23	42.95	39.97	38.13	46.23	42.95
"	11.	47.48	45.44	41.49	38.73	47.75	45.16	41.28*	38.05	46.00	42.51	39.98	38.53	46.00	42.51
"	18.	47.43	45.23	41.42	38.80	47.69	44.80	40.39	38.25	45.75	42.35	40.01	38.69	45.75	42.35
"	25.	47.36	45.01	41.29	38.29	47.63	44.49	40.36	37.78	45.54	42.14	39.65	37.91	45.54	42.14
March	4.	47.30	44.79	41.04	38.54	47.50	44.29	40.14	37.94	45.32	41.99	39.65	38.22	45.32	41.99
"	11.	47.22	44.60	40.96	38.46	47.45	44.02	39.98	37.73	45.13	41.87	39.64	38.34	45.13	41.87
"	18.	47.15	44.42	40.99	39.32	47.33	43.82	39.98	38.82	44.95	41.59	40.08	39.50	44.95	41.59
"	25.	47.07	44.27	41.18	39.47	47.21	43.63	40.28	39.29	44.77	41.77	40.45	39.71	44.77	41.77
April	1.	46.99	44.17	41.27	39.89	47.06	43.49	40.50	40.03	44.61	41.91	40.86	40.39	44.61	41.91
"	8.	46.91	44.07	41.40	40.01	46.93	43.38	41.07	40.13	44.49	42.04	41.09	40.64	44.49	42.04
"	15.	46.84	44.02	41.56	40.91	46.85	43.38	41.07	41.13	44.39	42.16	41.63	41.80	44.39	42.16
"	22.	46.76	43.97	41.90	41.53	46.75	43.34	41.62	41.99	44.31	42.31	42.16	42.32	44.31	42.31
"	29.	46.68	43.97	42.33	42.78	46.64	43.39	42.06	43.14	44.26	42.62	43.02	43.75	44.26	42.62
May	6.	46.61	44.02	43.03	44.54	46.51	43.51	43.08	45.38	44.23	43.10	44.18	45.13	44.23	43.10
"	13.	46.54	44.10	43.66	44.55	46.48	43.69	43.93	45.71	44.30	43.96	45.21	46.34	44.30	43.96
"	20.	46.47	44.23	44.01	45.02	46.39	43.92	44.51	46.35	44.37	44.36	45.87	47.33	44.37	44.36
June	27.	46.40	44.40	44.47	46.00	46.22	44.49	46.04	47.46	44.46	44.78	46.63	48.58	44.46	44.78
"	3.	46.35	44.56	45.09	47.26	46.22	44.49	46.04	49.22	44.57	45.31	47.61	49.43	44.57	45.31
"	10.	46.30	44.75	45.85	48.41	46.16	44.77	47.08	50.61	44.73	45.92	48.63	51.07	44.73	45.92
"	17.	46.26	44.99	46.59	49.69	46.13	45.15	48.05	51.98	44.92	46.49	49.49	52.09	44.92	46.49
"	24.	46.22	45.20	47.39	50.63	46.12	45.54	49.08	53.10	45.08	46.99	49.92	52.09	45.08	46.99
July	1.	46.21	45.55	48.05	51.04	46.13	46.02	49.90	53.30	45.51	48.06	51.11	53.09*	45.51	48.06
"	8.	46.19	45.87	48.66	51.74*	46.12	46.49	50.72	54.87	45.28	47.49	50.72	53.11	45.28	47.49
"	15.	46.20	46.19	49.38	51.96*	46.23	46.95	51.55	55.04	45.51	48.06	51.22*	53.32*	45.51	48.06
"	22.	46.23	46.53	49.79	52.00*	46.23	47.04	51.99	55.12	45.74	48.50	51.52*	53.63*	45.74	48.50
"	29.	46.25	46.85	50.09	52.32*	46.63*	48.06*	52.53*	55.19*	45.97	48.91	51.52*	53.73*	45.97	48.91
August	5.	46.35	47.13	50.37	52.96	46.29	48.28	52.78	55.45	46.20	49.25	51.79*	53.73*	46.20	49.25
"	12.	46.30	47.42	50.67	53.57	46.37	48.67	53.10	56.06	46.44	49.48	52.12*	54.15*	46.44	49.48
"	19.	46.35	47.70	50.99	52.75	46.46	49.05	53.45	55.61	46.66	49.78	52.02*	53.30*	46.66	49.78
"	26.	46.40	47.93	51.06	53.10	46.51	49.33	53.42	55.25	46.88	49.96	52.07*	53.74	46.88	49.96
September	2.	46.47	48.15	51.06	52.81	46.66	49.64	53.17	54.79	47.07	50.05	52.13*	53.53	47.07	50.05
"	9.	46.54	48.35	51.03	52.03	46.80	49.84	53.16	53.50	47.30	50.22	52.10	52.78	47.30	50.22
"	16.	46.62	48.52	50.82	51.71	46.99	50.09	52.70	53.02	47.46	50.20	51.73	52.52	47.46	50.20
"	23.	46.69	48.62	50.69	51.49	47.03	50.17	52.64	52.91	47.53	50.18	51.84	52.18	47.53	50.18
"	30.	46.77	48.71	50.52	50.91	47.15	50.17	52.29	52.08	47.71	50.08	51.23	51.50	47.71	50.08
October	7.	46.86	48.77	50.28	50.03	47.36	50.20	51.93	50.92	47.83	49.97	50.73	50.38	47.83	49.97
"	14.	46.93	48.80	49.86	49.21	47.36	50.19	51.28	50.04	47.97	49.50	49.42	48.81	47.97	49.50
"	21.	47.01	49.45	49.45	48.62	47.46	50.11	50.80	49.40	48.03	49.15	48.64	47.34	48.03	49.15
"	28.	47.08	48.75	49.01	47.46	47.58	50.02	50.18	48.13	48.03	49.15	48.64	47.34	48.03	49.15
November	4.	47.15	48.67	48.27	45.78	47.65	49.83	49.16	46.00	48.03	48.22	47.01	45.68	48.03	48.22
"	11.	47.23	48.55	47.63	45.63	47.77	49.67	48.48	45.78	48.03	48.22	47.01	45.68	48.03	48.22
"	18.	47.29	48.38	47.11	44.48	47.82	49.42	47.60	44.35	48.01	47.74	46.07	44.29	48.01	47.74
"	25.	47.34	48.19	46.38	43.05	47.88	49.06	46.56	42.93	47.58	46.62	44.16	42.36	47.58	46.62
December	2.	47.38	47.97	45.64	42.46	47.85	48.74	45.66	41.97	47.58	46.08	44.16	42.36	47.58	46.08
"	9.	47.42	47.71	45.18	42.38	47.97	48.38	44.95	41.81	47.63	46.08	44.16	42.36	47.63	46.08
"	16.	47.45	47.46	44.83	42.12	47.99	48.03	44.50	41.55	47.33	45.67	43.41	41.91	47.33	45.67
"	23.	47.47	47.19	44.41	41.70	47.98	47.39	44.22	40.94	47.29	45.18	42.89	41.18	47.29	45.18
"	30.	47.47	46.95	44.07	40.99	47.98	47.21	43.46	40.39	47.09	44.92	42.47	40.54	47.09	44.92

The numbers marked thus * are obtained from the Mean of Four Years only.

The dates in the preceding Table are the mean of the corresponding days of observation during the five years. More correctly they ought to be about half a-day earlier; thus, the temperature of February 4 belongs to February 3·5, or to midnight of the 3d, instead of the 4th at noon, and so of the others.

The practice of denoting periodic variations of temperature by a series of the form

$$y_n = A + B \sin (n + b) + C \sin (2n + c) + \&c.$$

(where y_n is the temperature corresponding to the fraction of the year denoted by n , and A, B, C, b, c , are constant quantities), has prevailed in Germany at least since the time of LAMBERT.* I have thought it worth while to compute the equations for each of the 12 curves, so as to facilitate comparison with the results of QUETELET† and others. But my method of proceeding has been somewhat different from his. I sketched very carefully interpolating curves through the curves of Plate VIII., so as to diminish their remaining irregularities, and having divided the horizontal space corresponding to a year into 12 equal parts (each of which may be represented by the space of 30° , the whole period of variation being 360°), I measured and inserted in a table the ordinates of the interpolated curve corresponding to these points; and with the aid of these ordinates, the equation to the curve was calculated by the aid of the tables given at the end of the second volume of DOVE's *Repertorium*. The results were as follows:—The first term is of course the mean temperature of the year, which has been taken from Table V.

TABLE XVI. CONTAINING THE EQUATIONS TO THE ANNUAL CURVES.

3 FEET.	
Observatory, $y_n = 45.49 - 7.39 \sin (n \cdot 30^\circ + 43^\circ) + 0.362 \sin (n \cdot 60^\circ + 29^\circ)$	
Ex. Garden, $y_n = 46.13 - 9.00 \sin (n \cdot 30^\circ + 49^\circ) + 0.737 \sin (n \cdot 60^\circ + 63^\circ)$	
Craigleith, $y_n = 45.88 - 8.16 \sin (n \cdot 30^\circ + 47^\circ) + 0.284 \sin (n \cdot 60^\circ + 34^\circ)$	
6 FEET.	
Observatory, $y_n = 45.86 - 5.06 \sin (n \cdot 30^\circ + 23^\circ) + 0.433 \sin (n \cdot 60^\circ + 7^\circ)$	
Ex. Garden, $y_n = 46.42 - 6.66 \sin (n \cdot 30^\circ + 29^\circ) + 0.501 \sin (n \cdot 60^\circ + 5^\circ)$	
Craigleith, $y_n = 45.92 - 6.16 \sin (n \cdot 30^\circ + 36^\circ) + 0.368 \sin (n \cdot 60^\circ + 340^\circ)$	
12 FEET.	
Observatory, $y_n = 46.36 - 2.44 \sin (n \cdot 30^\circ + 344^\circ) + 0.075 \sin (n \cdot 60^\circ + 330^\circ)$	
Ex. Garden, $y_n = 46.76 - 3.38 \sin (n \cdot 30^\circ + 348^\circ) + 0.230 \sin (n \cdot 60^\circ + 319^\circ)$	
Craigleith, $y_n = 45.92 - 4.22 \sin (n \cdot 30^\circ + 13^\circ)$	
24 FEET.	
Observatory, $y_n = 46.87 - 0.655 \sin (n \cdot 30^\circ + 85^\circ)$	
Ex. Garden, $y_n = 47.09 - 0.920 \sin (n \cdot 30^\circ + 275^\circ)$	
Craigleith, $y_n = 46.07 - 1.940 \sin (n \cdot 30^\circ + 327^\circ)$	

The following table contains the experimental ordinates, and those obtained from the preceding equations. The coincidence would have been somewhat closer had the mean of the 12 equidistant ordinates been taken for the mean temperature (A), instead of the mean of the entire observations.

* Pyrometrie, § 675.

† Ann. de l'Observatoire de Bruxelles, iv. 169.

TABLE XVII.

3 FEET.									
n.	Observatory.			Experimental Garden.			Craigleith.		
	Obs.	Calc.	Diff.	Obs.	Calc.	Diff.	Obs.	Calc.	Diff.
0	-4.5	-4.88	-.38	-6.0	-6.10	-.10	-5.5	-5.85	-.35
1	-6.75	-6.71	+.04	-8.25	-8.21	+.04	-7.7	-7.68	+.02
2	-7.2	-7.01	+.19	-8.45	-8.57	-.12	-7.7	-7.66	+.04
3	-5.75	-5.56	+.19	-6.5	-6.60	-.10	-5.7	-5.68	+.02
4	-2.4	-2.40	.00	-2.25	-2.38	-.13	-1.85	-2.07	-.22
5	+1.25	+1.40	+.15	+3.0	+2.93	-.07	+2.3	+2.30	.00
6	+5.5	+5.22	-.28	+7.3	+7.42	+.12	+6.15	+6.16	+.01
7	+7.2	+7.43	+.23	+9.3	+9.45	+.15	+8.0	+8.25	+.25
8	+7.3	+7.39	+.09	+8.5	+8.48	-.02	+7.65	+7.90	+.25
9	+5.2	+5.22	+.02	+5.75	+5.28	-.47	+5.55	+5.36	-.19
10	+1.8	+1.78	-.02	+1.25	+1.15	-.10	+1.2	+1.50	+.30
11	-2.3	-1.87	+.43	-3.20	-2.85	+.35	-3.0	-2.55	+.45

6 FEET.									
0	-1.75	-1.92	-.17	-3.15	-3.18	-.03	-3.5	-3.75	-.25
1	-3.5	-3.64	-.14	-5.25	-5.25	.00	-5.7	-5.39	+.31
2	-4.6	-4.68	-.08	-6.2	-6.25	-.05	-5.2	-5.77	-.57
3	-4.6	-4.71	-.11	-6.0	-5.87	+.13	-5.0	-4.86	+.14
4	-3.5	-3.45	+.05	-4.0	-3.88	+.12	-2.75	-2.74	+.01
5	-0.9	-0.96	-.06	-0.65	-0.52	+.13	+0.25	+0.28	+.03
6	+2.3	+2.03	-.27	+3.3	+3.27	-.03	+4.0	+3.50	-.50
7	+4.45	+4.44	-.01	+6.2	+6.16	-.04	+5.8	+5.87	+.07
8	+5.25	+5.37	+.12	+6.8	+7.07	+.27	+6.2	+6.50	+.30
9	+4.8	+4.60	-.20	+5.75	+5.78	+.03	+5.3	+5.11	-.19
10	+3.0	+2.65	-.35	+3.3	+2.97	-.33	+2.6	+2.27	-.33
11	0.0	+0.27	+.27	-0.7	-0.30	+.40	-1.4	-1.01	+.39

12 FEET.									
0	+0.55	+0.64	+.09	+0.35	+0.55	+.20	-1.0	-0.95	+.05
1	-0.5	-0.55	-.05	-1.2	-0.97	+.23	-2.8	-2.88	-.08
2	-1.7	-1.62	+.08	-2.45	-2.28	+.17	-3.9	-4.03	-.13
3	-2.35	-2.31	+.04	-3.35	-3.15	+.20	-4.15	-4.11	+.04
4	-2.55	-2.40	+.15	-3.35	-3.28	+.07	-3.25	-3.08	+.17
5	-1.9	-1.83	+.07	-2.7	-2.49	+.21	-1.3	-1.23	+.07
6	-0.9	-0.71	+.19	-1.0	-0.85	+.15	+1.0	+0.95	-.05
7	+0.5	+0.62	+.12	+1.0	+1.11	+.11	+3.0	+2.88	-.12
8	+1.7	+1.77	+.07	+2.7	+2.74	+.04	+4.0	+4.03	+.03
9	+2.25	+2.38	+.13	+3.25	+3.45	+.20	+4.0	+4.11	+.11
10	+2.3	+2.32	+.02	+2.9	+3.14	+.24	+3.25	+3.08	-.17
11	+1.7	+1.68	-.02	+2.0	+2.03	+.03	+0.9	+1.23	+.33

24 FEET.									
0	+0.80	+0.65	-.15	+0.8	+.92	+.12	+1.0	+1.06	+.06
1	+0.6	+0.59	-.01	+0.7	+.75	+.05	+0.2	+0.10	-.10
2	+0.5	+0.38	-.12	+0.45	+.39	-.06	-0.75	-0.88	-.13
3	+0.1	+0.06	-.04	0.0	-.08	-.08	-1.5	-1.63	-.13
4	-0.25	-0.28	-.03	-0.45	-.53	-.08	-1.85	-1.94	-.09
5	-0.35	-0.54	-.19	-0.9	-.83	+.07	-1.75	-1.73	+.02
6	-0.6	-0.65	-.05	-1.05	-.92	+.13	-1.05	-1.06	-.01
7	-0.5	-0.59	-.09	-0.75	-.75	.00	0.0	-0.10	-.10
8	-0.25	-0.38	-.13	-0.3	-.39	-.09	+1.0	+0.88	-.12
9	-0.05	-0.06	-.01	+0.05	+.08	+.03	+1.75	+1.63	-.12
10	+0.30	+0.28	-.02	+0.65	+.53	-.12	+1.9	+1.94	+.04
11	+0.7	+0.54	-.16	+0.8	+.83	+.03	+1.85	+1.73	-.12

Subtracting (5) from (4),

$$4mx = a(A - B)$$

and substituting the value of a just found

$$x = \frac{m}{2} \cdot \frac{A - B}{A + B}$$

which determines the position of the greatest ordinate, whence that ordinate may be deduced.

The results are contained in the following table.

TABLE XVIII.

	MAXIMA.				MINIMA.			
	y	x	Epoch.		y	x	Epoch.	
			Fraction of Year.	Month and Day.			Fraction of Year.	Month and Day.
3 FEET.								
Observatory . . .	53.20	224° 30'	.624	Aug. 16.7	36.39	50° 22'	.140	Feb. 21.0
Experimental Garden	55.73	214° 48'	.597	Aug. 7.0	37.46	56° 12'	.139	Feb. 20.5
Craigleith . . .	54.29	220° 50'	.613	Aug. 12.7	37.96	44° 42'	.124	Feb. 15.3
6 FEET.								
Observatory . . .	51.23	240° 19'	.668	Sept. 2.0	41.02	75° 47'	.211	Mar. 19.0
Experimental Garden	53.50	235° 23'	.654	Aug. 27.7	40.12	67° 30'	.188	Mar. 10.7
Craigleith . . .	52.45	234° 39'	.652	Aug. 27.0	40.13	53° 00'	.147	Feb. 23.7
12 FEET.								
Observatory . . .	48.85	282° 30'	.785	Oct. 14.7	43.86	108° 58'	.303	April 21.7
Experimental Garden	50.23	275° 41'	.766	Oct. 7.7	43.39	109° 15'	.303	April 21.7
Craigleith . . .	50.14	257° 00'	.714	Sept. 18.7	41.70	77° 00'	.214	Mar. 20.0
24 FEET.								
Observatory . . .	47.53	5° 00'	.014	Jan. 6.0	46.21	185° 00'	.514	July 7.7
Experimental Garden	48.01	355° 00'	.986	Dec. 27.0	46.17	175° 00'	.486	June 27.3
Craigleith . . .	48.01	303° 00'	.842	Nov. 4.3	44.13	123° 00'	.342	May 6.0

These results, obtained in a different manner, may be compared with those in Tables VIII. and XIII. The inspection of the deviations of the annual curve in Plate VII., from the average results in Plate VIII., illustrates well the remarkable variations in the character of the seasons in these five years, and renders it probable that the mean effects of ordinary atmospheric temperatures throughout the year may be most conveniently and accurately studied, and the annual curve ascertained, by observations at a moderate depth in the soil.

F. On the Influence of "Specific Heat" on the Results.

The quantity which we have, in page 208, called B (after M. QUETELET*) is equal to

$$\frac{\sqrt{\pi}}{\alpha} \log e$$

Where $\pi = 3.1416$, e is the base of natural logarithms, and α the symbol used by

* Annales, &c., vol. iv. p. 112.

POISSON to express the ratio $\sqrt{\frac{k}{c}}$ where k is the conductivity of the soil and c is specific heat. Whence, if the whole quantity B be known, and c the specific heat be deduced from direct experiment in the laboratory, k may be found. [In the present instance, it is to be recollected that the *French foot* is taken as the unit.]

M. ELIE DE BEAUMONT, who has taken much interest in the experiments described in this paper, very obligingly requested M. REGNAULT of Paris (whose skill in this matter is well known) to determine the specific heat of specimens taken from the grounds of the Observatory, Experimental Garden, and Craigleith respectively; and M. REGNAULT had the goodness promptly to submit them to experiment, and he communicated to me the following results:—

				Specific Heat.
Porphry of the Calton Hill,	.	.	.	0·20654
Another Experiment,	.	.	.	0·20587
Mean,				0·20620
Sand of the Experimental Garden,	.	.	.	0·19432
Sandstone of Craigleith Quarry,	.	.	.	0·19257
Another Experiment,	.	.	.	0·19152
				0·19205

Some correction would, no doubt, require to be made for the moisture contained in the soil, but this appears difficult to apply, and probably would be inconsiderable. The above results evidently represent specific heats referred to unit of *weight* of the body, but that referred to in the theoretical investigation, is taken with respect to unity of volume.* The above results require, therefore, to be multiplied by the specific gravities (water being the standard in each case) which I have found to be, when reduced to 60° F.

	Trap.	Sand.	Sandstone.
Specific gravity,	2·562	1·547†	2·408
Whence we have specific heat referred to unit of <i>volume</i> ,	0·5283	0·3006	0·4623

G. Final Results.

The value of POISSON's constant a , expressive of the ratio $\sqrt{\frac{k}{c}}$ being obtained from our constant B by means of the relation

$$a = \frac{\sqrt{\pi}}{B} \log e$$

* Poisson, *Théorie de la Chaleur*; *Suppl.*, p. 4.

† Mean of two experiments, 1·556 and 1·538. It is evident, that since it is required to find the specific heat of unit of volume of the mass to be heated or cooled, we must take the aggregate of sand as we find it in the soil, and not the specific gravity of the individual grains. Accordingly, the specific gravity was determined by comparing the weights of closely packed sand and of distilled water contained in a stoppered phial.

(which is equivalent to the expression in the *Theorie de la Chaleur*, p. 499, Eq. (26)), gives the following numerical result :—

Trap.	Sand.	Sandstone.
14·124	16·137	24·750

but if referred to the French METRE instead of *foot* as unity (the centigrade degree has been already employed), they become

<i>a</i>	4·588	5·242	8·040
----------	-------	-------	-------

which are comparable with POISSON'S result, 5·11655 for the Observatory of Paris. Now, the specific heat *c* having been found in the last section, we may eliminate it, and obtain the following numerical values of *k*, the conducting power of the strata, which it may be presumed has rarely been so accurately determined for any kind of matter.

<i>k</i>	Trap.	Sand.	Sandstone.
	11·120	8·260	29·884

There is another constant *b* employed by Poisson, which involves the character of the recipient surface of the ground as well as the interior conductivity, and which is determinable from the retardation of epochs by equation (27) of page 499 of the *Theorie de la Chaleur*.

$$b = \frac{\sqrt{\pi}}{a} \left\{ \cot \left[\frac{1}{2} (\theta + \theta_1) - \frac{x}{2a\sqrt{\pi}} \right] 360^\circ - 1 \right\}$$

where θ and θ_1 are the epochs of maximum and minimum temperature at any given depth, reckoned from the 21st March in fractions of a year (= 1), the metre being also the unit. Instead of taking observations at a single depth, we may take the epoch for 24 French feet from the interpolating lines in Plate X., which represent not merely the observations at that depth, but the result of their combination with all the others.

	Trap.	Sand.	Sandstone.
	Year.	Year.	Year.
Maxima at 24 F. ft.,	July 8. = ·515	July 1. = ·496	May 6. = ·234
Minima,	Jan. 4. = 1·008	Dec. 26. = ·984	Nov. 4. = ·841
Mean reckoned from 1st Jan.,	·761	·740	·591
Reckoned from 21st Mar. = $\frac{1}{2} (\theta + \theta_1)$	·545	·524	·375

Substituting the values of $x = 7·7961$ metres (24 F. ft.), and of *a* before found, we obtain

<i>b</i>	Trap.	Sand.	Sandstone.
	0·4972	0·1007	0·0772

M. POISSON finds for *b* at the Paris Observatory, the value 1·057. If we examine the circumstances which influence the value of *b*, we shall admit that its determination in this manner is liable to so great errors as to render it almost worthless.

I shall not follow farther the application of these results, of which a and b are the most immediately important. In particular, I shall not attempt to find, with POISSON, the whole climateric effect of the solar influence which he deduces from the quantities a and b found above; both on account of the uncertainty of the value of b , and because I have attempted elsewhere to shew that the physical assumptions, upon which the great French analyst has founded the determination of this quantity, are exceedingly precarious.*

I have only farther to add, that the extensive reductions and computations of which the results have been given in this paper, were performed under my immediate superintendence by different persons at different times. My thanks are due to Mr BROWN, Mr MOFFAT, Mr LINDSAY, and especially to Mr GREGG, for their attention and accuracy in conducting them.

EDINBURGH, June 1846.

APPENDIX,

Containing Remarks on the Connection of the Preceding Observations with the Theory of Fourier and Poisson.†

“ So far as the effect of SOLAR HEAT is concerned, the *à priori* solution of the problem of the temperature of any part of the earth's surface may be thus imagined:—(1.) The *whole* quantity of sunshine which falls on any part of the earth's surface in the course of a year is to be found, and also the law of its variation of force at different seasons. (2.) The part of this heat which becomes effective in heating the earth's crust is to be found by multiplying the amount by a constant depending upon the absorbent power of the surface. (3.) This quantity of heat thus reduced is propagated towards the interior, according to the laws of conduction, which again presuppose the knowledge of two constants proper to each soil, namely, the Conductivity and the Specific Heat.

“(1.) The measure of the quantity of sunshine received by any place in a year, and its distribution at different seasons, has been a favourite problem with mathematicians. In ultimate analysis, it depends of course on the astronomical elements which affect the progress of the seasons, viz., the obliquity of the ecliptic (γ), the latitude of the place (μ), the eccentricity of the earth's orbit (α), and the longitude of the sun's perigee (ω). But there are also elements quite as important as any of these; the imperfect transparency of the air and its varying thickness, owing to differences of obliquity of the transmitted rays, and the condition of opacity depending on the weather. Neither of these is insignificant, neither of them compensatory; both may be considered as functions of the hour-angle and fraction of the year, and the second is besides subjected to the most capricious changes. Yet of these ele-

* See Second Report on Meteorology, Arts. 104, &c., in the British Association Reports for 1840.

† Taken from the Second Report on Meteorology, British Association Report, 1840, Art. 88, &c.

ments theory has hitherto taken no account, and consequently the expression for the quantity of sunshine obtained, in terms of the astronomical constants, with so much labour, we must hold to be nearly useless as a physical datum. It is vain to say, with M. POISSON, "Les lois d'absorption de la chaleur solaire à travers l'atmosphère, les variations diurnes et annuelles sont également inconnues, et l'on peut seulement supposer qu'elles sont peu considérables." We know, on the contrary, that they are so considerable, that, estimating the loss of radiant heat by a *vertical* passage through the atmosphere at only twenty-five per cent., at an angle of elevation of 25° the force of the solar rays would be reduced to a half, and at 5° to *one-twentieth* part. We know, indeed, that the difference of the *direct* effect of a vertical and a horizontal sun is due to this cause alone, exaggerated, of course, immensely by the variable meteorological state of the atmosphere, which again is a function of the latitude.

"(2.) The receptive power of the surface is a datum which we find it very difficult directly to determine, and which, since the quantity of sunshine cannot (as we have seen) possibly be directly computed, must be inextricably mixed up with it. It might be a question, whether, by covering a tolerably extensive surface of soil, in which thermometers are inserted, with a composition of known superficial conductivity, this element might not become known.

"(3.) The specific heat (c) and conductivity (k) of the soil are also inextricably mixed up together in the analysis; but either becoming known, the other may be inferred from thermometric observations carried below the surface. The specific heat seems that best adapted for laboratory experiments; M. ELIE DE BEAUMONT has assigned 0.5614 for the value of c (that of an equal *bulk* of water being = 1), proper to the soil at the Observatory at Paris.

"To obtain the conductivity of the soil *à posteriori*, it is fortunately not necessary that the preceding theoretical estimation of the distribution of sunshine should be correct; but there are other estimates into which it essentially enters, and which must therefore be received with corresponding caution. To facilitate reference to M. POISSON'S work, I will shew how the simple and very satisfactory observation of maximum and minimum temperature of the earth's crust at given small depths (above the *invariable stratum*) may be made to yield a knowledge of some of the constants above referred to.

"Let the excess of the annual maximum above annual minimum temperature at a depth p be expressed by Δ_p ; then

$$\log \Delta_p = A + B p$$

in which A of course denotes the log. range when $p = 0$ or at the surface, and B determines the common ratio of the geometrical progression according to which the range diminishes. From observations with two thermometers at different depths, A and B may be obtained *à posteriori*.

"Now when we consult M. POISSON'S work, we find that his equation (23.), page 497, which is equivalent to the preceding one, is thus composed. The quantity A , on which the superficial range depends, contains (1) astronomical constants of climate $\gamma, \mu, \alpha, \bar{\omega}$ already mentioned; (2) a temperature h , depending on the mean force of the solar rays which have traversed the atmosphere and entered into combination with the earth's surface by absorption at a given place; (3) the constant of conductivity k , and of specific heat c .

"The coefficient B , on which the *rate* of diminution of the range depends, is fortunately a very simple quantity, involving neither astronomical constants, nor those proper to the superficies. It is, in fact, an absolute number multiplied by $\sqrt{\frac{c}{k}}$, and from a knowledge of it (by

observations with two or more thermometers) this quantity may be very readily and accurately determined; and it affords the only unexceptionable manner of ascertaining the conductivity of the earth's crust on a large scale.

* * * *

“ The epochal retardations for the annual curves at the depth of a few feet, follow, generally speaking, a simple law, for they are propagated uniformly downwards with a velocity which is easily connected with the constants proper to the soil, determined from the range at two given depths, as just explained. It must not be concluded, however, that the epochs of earth-temperature at the surface coincide with those of air-temperature in the adjoining stratum. The difference of epoch may be obtained in terms of the conductivity and superficial characters of the solid stratum. But the complete expression for the epoch at any depth in terms of the dates of maximum and minimum at some other depth, and of the constants of conductivity and surface, derived from two observed ranges, is so complex, that, so far as I know, no attempt has been made to verify M. POISSON'S formulæ except in a single example by himself, taken from M. ARAGO'S observations.”

TABLES.

t_1 denotes the deepest, or 24 feet Thermometer, t_2 at 12 feet, t_3 at 6 feet, t_4 at 3 feet (French Measure); t_5 at the surface of the ground; T , the temperature of the air in the Thermometer box.

OBSERVATORY.

Es.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
17.																		
e 4	48.10	+03	+02	48.15	45.93	+01	+01	45.95	42.06	.00	.00	42.06	39.11	.00	-.02	39.09		
13	47.98	+03	+03	48.04	45.59	+01	+02	45.62	42.00		+01	42.01	40.54		+03	40.57		
20	47.87	+03	+02	47.92	45.37	+01	+01	45.39	42.17		.00	42.17	40.89		-.01	40.88		
27	47.79	+03	+02	47.84	45.22	+01	.00	45.23	42.05		-.01	42.04	38.90		-.03	38.87		
6	47.73	+03	-.01	47.75	45.09	+01	-.02	45.08	41.68		-.03	41.65	39.39		-.07	39.92		
13	47.63	+03	.00	47.66	44.94	+01	-.01	44.94	41.66		-.02	41.64	39.40		-.05	39.35		
20	47.52	+03	+03	47.58	44.75	+01	+01	44.77	41.28		.00	41.28	38.08		-.01	38.07		
27	47.42	+03	+01	47.46	44.59	+01	+02	44.62	40.90		+02	40.92	37.49		+02	37.51		
3	47.34	+03	+04	47.41	44.42	+01	+02	44.45	40.49		+02	40.51	37.11		+01	37.12		
10	47.29	+03	+01	47.33	44.26	+01	.00	44.27	40.32		-.01	40.31	37.69		-.04	37.65		
17	47.22	+03	.00	47.25	44.08	+01	-.01	44.08	40.45		-.02	40.43	38.72		-.04	38.68		
24	47.12	+03	+01	47.16	43.87	+01	.00	43.88	40.76		-.01	40.75	40.09		-.03	40.06		
a 1	47.05	+03	-.02	47.06	43.83	.00	-.02	43.81	41.37		-.04	41.33	41.73		-.08	41.65		
8	46.96	+02	.00	46.98	43.82	.00	.00	43.82	42.10		-.02	42.08	43.13		-.02	43.11		
15	46.92	+02	-.04	46.90	43.90	.00	-.03	43.87	42.72		-.07	42.65	43.23		-.13	43.10		
22	46.82	+02	-.01	46.83	44.00	.00	-.01	43.99	43.38		-.03	43.35	45.31		-.05	45.26		
29	46.78	+01	-.05	46.74	44.16	.00	-.04	44.12	44.21		-.09	44.12	46.57		-.15	46.42		
5	46.70	+01	-.03	46.68	44.35	.00	-.03	44.32	44.94		-.07	44.87	47.42		-.10	47.32		
12	46.65	+01	-.06	46.60	44.60	.00	-.05	44.55	45.81		-.13	45.68	48.69		-.20	48.49		
19	46.58	.00	-.07	46.51	44.88	-.01	-.04	44.83	46.80		-.14	46.66	50.99		-.10	50.89		
26	46.55	.00	-.05	46.50	45.18	-.01	-.05	45.12	47.79		-.13	47.66	52.69		-.17	52.52		
l 3	46.53	-.01	-.07	46.45	45.55	-.01	-.06	45.48	48.85		-.15	48.70	53.46		-.19	53.27		
10	46.52	-.01	-.08	46.43	45.98	-.01	-.08	45.89	49.87		-.21	49.66						
17	46.49	-.01	-.06	46.42	46.44	-.01	-.07	46.36	50.98		-.16	50.82						
24	46.48	-.01	-.06	46.41	46.82	-.01	-.06	46.75	51.50		-.13	51.37						
31	46.49	-.02	-.03	46.44	47.32	-.01	-.04	47.27	51.79		-.06	51.73						
7	46.53	-.02	-.06	46.45	47.78	-.01	-.07	47.70	51.90		-.12	51.78	53.88		-.15	53.73		
14	46.59	-.02	-.06	46.51	48.10	-.01	-.08	48.01	51.87		-.15	51.72	54.66		-.20	54.46		
21	46.65	-.02	-.05	46.58	48.38	-.01	-.07	48.30	52.23		-.10	52.13						
28	46.70	-.02	-.03	46.65	48.60	-.01	-.04	48.55	52.49		-.05	52.44	54.40		-.04	54.36		
4	46.78	-.02	-.03	46.73	48.87	-.01	-.04	48.82	52.11		-.05	52.06	52.67		-.05	52.62		
11	46.86	-.02	-.03	46.81	49.05	.00	-.03	49.02	51.60		-.03	51.57	52.25		-.04	52.21		
18	46.96	-.02	-.04	46.90	49.21	.00	-.05	49.16	51.41		-.07	51.34	51.48		-.10	51.38		
25	47.06	-.02	-.04	47.00	49.27	.00	-.05	49.22	51.20		-.08	51.12	52.12		-.10	52.02		
2	47.16	-.02	-.04	47.10	49.35	.00	-.07	49.28	51.09		-.11	50.98	51.10		-.15	50.95		
9	47.24	-.01	-.04	47.19	49.34	.00	-.04	49.30	50.91		-.07	50.84	51.80		-.09	51.71		
16	47.33	-.01	-.05	47.27	49.37	.00	-.05	49.32	50.88		-.08	50.80	50.84		-.11	50.73		
23	47.38	-.01	-.01	47.36	49.34	.00	.00	49.34	50.49		.00	50.49	50.28		.00	50.28		
30	47.43	-.01	+01	47.43	49.30	.00	+02	49.32	49.86		+02	49.88	47.30		+02	47.32		
6	47.50	-.01	+02	47.51	49.25	.00	+03	49.28	48.72		+05	48.77	45.05		+03	45.08		
13	47.57	-.01	+03	47.59	49.09	.00	+04	49.13	47.88		+05	47.93	45.73		+06	45.79		
20	47.63	.00	+03	47.66	48.86	.00	+04	48.90	47.15		+05	47.20	43.47		+02	43.49		
27	47.69	.00	+03	47.72	48.62	.00	+04	48.66	46.42		+04	46.46	43.35		+03	43.38		
4	47.74	+01	+03	47.78	48.35	+01	+04	48.40	45.80		+04	45.84	42.75		+03	42.78		
11	47.76	+01	+04	47.81	48.05	+01	+05	48.11	45.21		+05	45.26	41.70		+04	41.74		
18	47.82	+01	-.01	47.82	47.82	+01	-.01	47.82	44.72		+03	44.75	41.80		-.07	41.73		
25	47.84	+01	-.01	47.84	47.45	+01	-.01	47.45	44.41		+03	44.44	42.79		-.05	42.74		
32.																		
1	47.82	+01	+01	47.84	47.24	.00	+01	47.25	44.80		.00	44.80	43.65		.00	43.65		
8	47.79	+01	+05	47.85	47.02	.00	+05	47.07	44.70		+06	44.76	42.50		+07	42.57		
15	47.76	+01	+06	47.83	46.86	+01	+05	46.92	44.10		+06	44.16	40.20		+06	40.26		
22	47.75	+02	+04	47.81	46.70	+01	+03	46.74	43.30		+02	43.32	38.88		.00	38.88		
29	47.71	+02	+05	47.78	46.41	+01	+04	46.46	42.41		+03	42.44	37.80		+02	37.82		

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5
1838.																	
Feb.	5	17.69	+02	+04	17.75	16.14	+01	+03	16.18	11.79	.00	+02	41.81	37.50	.00	.00	37.50
	12	47.61	+03	+06	47.70	45.64	+01	+04	45.69	40.99		+03	41.02	36.81		-.02	36.79
	19	47.58	+03	+05	47.66	45.43	+01	+03	45.47	40.65		+02	40.67	35.80		.00	35.80
	26	47.51	+03	+06	47.60	45.13	+01	+04	45.18	40.17		+03	40.20	35.75		+02	35.77
Mar.	5	47.47	+03	+02	47.52	44.75	+01	+01	44.77	39.60		.00	39.60	35.70		-.02	35.68
	12	47.41	+03	+01	47.45	44.43	+01	.00	44.44	39.60		-.01	39.59	37.10		-.04	37.06
	19	47.34	+03	+01	47.38	44.17	+01	.00	44.18	40.06		-.01	40.05	38.25		-.03	38.22
	26	47.25	+03	+01	47.29	43.97	+01	.00	43.98	40.21		-.01	40.20	37.78		-.03	37.75
Apr.	2	47.16	+03	+01	47.20	43.85	+01	.00	43.86	40.46		-.01	40.45	39.52		-.02	39.50
	9	47.05	+03	.00	47.08	43.69	.00	.00	43.69	40.75		-.02	40.73	39.91		-.04	39.87
	16	46.96	+03	+02	47.01	43.68	.00	+01	43.69	41.15		.00	41.15	40.89		.00	40.89
	23	46.88	+03	+01	46.92	43.68	.00	.00	43.68	41.37		-.01	41.36	40.22		-.03	40.19
	30	46.80	+03	.01	46.82	43.70	.00	-.01	43.69	41.59		-.03	41.56	41.00		-.07	40.93
May	7	46.75	+02	-.02	46.75	43.73	.00	-.03	43.70	41.95		-.08	41.87	42.58		-.15	42.43
	14	46.64	+02	.00	46.66	43.75	.00	-.01	43.74	42.80		-.02	42.78	44.55		-.03	44.52
	21	46.57	+02	-.02	46.57	43.88	.00	-.02	43.86	43.37		-.04	43.33	44.02		-.09	43.93
	29	46.46	+01	-.01	46.46	44.05	.00	-.01	44.04	43.85		-.03	43.82	45.31		-.04	45.27
June	4	46.43	+01	-.04	46.40	44.21	.00	-.03	44.18	44.44		-.07	44.37	46.09		-.14	45.95
	11	46.37	+01	-.03	46.35	44.38	.00	-.02	44.36	45.11		-.06	45.05	47.29		-.09	47.20
	18	46.29	+01	-.01	46.29	44.62	.00	-.01	44.61	45.78		-.03	45.75	48.51		-.02	48.49
	25	46.26	.00	-.06	46.20	44.87	.00	-.05	44.82	46.57		-.12	46.45	49.58		-.21	49.37
July	2	46.24	.00	-.01	46.23	45.15	.00	-.02	45.13	47.36		-.03	47.33	50.82		-.01	50.81
	9	46.25	-.01	-.05	46.19	45.50	.00	-.06	45.43	48.20		-.12	48.08	52.38		-.19	52.19
	16	46.25	-.01	-.05	46.19	45.83	.00	-.06	45.76	49.10		-.12	48.98	53.26		-.17	53.09
	24	46.24	-.01	-.04	46.19	46.24	.00	-.05	46.18	49.66		-.11	49.55	52.53		-.15	52.38
	30	46.26	-.01	-.05	46.20	46.57	.00	-.06	46.50	49.84		-.12	49.72	52.39		-.18	52.21
Aug.	6	46.28	-.01	-.05	46.22	46.88	.00	-.06	46.81	50.14		-.12	50.02	53.37		-.17	53.20
	13	46.32	-.01	-.04	46.27	47.17	.00	-.05	47.11	50.55		-.10	50.45	53.44		-.12	53.32
	20	46.36	-.02	-.04	46.30	47.45	.00	-.05	47.39	50.70		-.10	50.60	53.11		-.12	52.99
	27	46.43	-.02	-.06	46.35	47.74	.00	-.08	47.65	50.84		-.15	50.69	52.50		-.24	52.26
Sept.	3	46.47	-.02	-.04	46.41	47.93	.00	-.05	47.88	50.76		-.08	50.68	52.50		-.12	52.38
	10	46.53	-.02	-.03	46.48	48.12	.00	-.04	48.08	50.70		-.05	50.65	51.04		-.08	50.96
	17	46.60	-.01	-.04	46.55	48.27	.00	-.04	48.23	50.31		-.06	50.25	51.78		-.07	51.71
	24	46.68	-.01	-.04	46.63	48.39	.00	-.05	48.34	50.42		-.08	50.34	51.20		-.11	51.09
Oct.	1	46.76	-.01	-.04	46.71	48.49	.00	-.05	48.44	50.30		-.09	50.21	51.29		-.12	51.17
	8	46.83	-.01	-.03	46.79	48.54	.00	-.04	48.50	50.10		-.06	50.04	49.71		-.11	49.60
	15	46.89	-.01	-.02	46.86	48.57	.00	-.03	48.54	49.59		-.03	49.56	48.06		-.07	47.99
	22	46.98	-.01	-.03	46.94	48.58	.00	-.04	48.54	48.96		-.08	48.88	47.81		-.12	47.69
	29	47.00	-.01	+01	47.00	48.46	.00	+02	48.48	48.70		+03	48.73	48.09		+05	48.14
Nov.	5	47.07	-.01	+01	47.07	48.39	.00	+02	48.41	48.16		+03	48.19	45.64		+01	45.65
	12	47.13	.00	+03	47.16	48.29	.00	+04	48.33	47.35		+05	47.40	44.80		+05	44.85
	19	47.19	.00	+02	47.21	48.14	.00	+03	48.17	46.61		+03	46.64	43.29		+02	43.31
	27	47.22	+01	+05	47.28	47.86	+01	+06	47.93	45.57		+08	45.65	41.85		+08	41.93
Dec.	3	47.29	+01	.00	47.30	47.69	+01	.00	47.70	45.00		-.02	44.98	42.52		-.06	42.46
	10	47.32	+01	.00	47.33	47.41	.00	.00	47.41	44.91		-.01	44.90	42.55		-.04	42.51
	17	47.34	+01	+02	47.37	47.15	.00	+03	47.18	44.68		+03	44.71	42.13		+02	42.15
	24	47.35	+01	+02	47.38	46.92	+01	+02	46.95	44.22		+01	44.23	41.30		-.01	41.29
	31	47.34	+01	+03	47.38	46.71	+01	+02	46.74	43.80		+02	43.82	41.04		+02	41.06
1839.																	
Jan.	7	47.33	+01	+03	47.37	46.47	+01	+03	46.51	43.50		+02	43.52	40.57		+02	40.59
	14	47.32	+02	+03	47.37	46.25	+01	+02	46.28	42.99		+02	43.01	40.10		+01	40.11
	21	47.29	+02	+03	47.34	46.02	+01	+03	46.06	42.61		+03	42.64	39.03		+01	39.04
	28	47.26	+02	+04	47.32	45.78	+01	+03	45.82	42.12		+03	42.15	38.85		+03	38.88
Feb.	4	47.24	+02	+01	47.27	45.54	+01	+01	45.56	41.68		-.01	41.67	37.63		-.04	37.59
	11	47.22	+02	.00	47.24	45.30	+01	-.08	45.31	41.34		-.01	41.33	39.21		-.05	39.16
	18	47.14	+02	+04	47.20	45.02	+01	+03	45.06	41.55		+02	41.57	39.31		+03	39.34
	25	47.09	+03	+02	47.14	44.83	+01	+01	44.85	41.30		.00	41.30	38.30		-.02	38.28
Mar.	4	47.04	+03	+02	47.09	44.66	+01	+01	44.68	41.09		.00	41.09	38.97		-.02	38.95
	11	46.97	+03	+02	47.02	44.48	+01	+01	44.50	41.03		.00	41.03	37.85		-.02	37.83
	18	46.90	+03	+02	46.95	44.30	+01	+01	44.32	40.65		.00	40.65	38.42		-.01	38.41
	25	46.84	+03	+01	46.88	44.12	+01	.00	44.13	40.69		-.01	40.68	38.86		-.03	38.83
Apr.	1	46.75	+03	+02	46.80	43.95	+01	+01	43.97	40.80		.00	40.80	38.92		.00	38.92
	8	46.72	+03	-.01	46.74	43.85	+01	-.01	43.85	40.72		-.03	40.69	38.30		-.08	38.22

	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
15	46.63	+03	.00	46.66	43.74	+01	-.01	43.74	40.68	.00	-.02	40.66	39.98	.00	-.06	39.92	41.60	48.0
22	46.57	+03	-.02	46.58	43.65	.00	-.01	43.64	41.26		-.03	41.23	41.00		-.10	40.90	43.80	51.4
29	46.51	+02	-.02	46.51	43.63	.00	-.02	43.61	41.89		-.05	41.84	42.68		-.11	42.57	47.50	53.8
6	46.41	+02	.00	46.43	43.65	.00	.00	43.65	42.67		-.01	42.66	44.50		-.01	44.49	43.60	45.6
13	46.34	+01	.00	46.35	43.76	.00	.00	43.76	43.33		-.01	43.32	44.32		-.01	44.31	44.50	45.6
20	46.30	+01	-.03	46.28	43.93	.00	-.02	43.91	43.60		-.07	43.53	44.18		-.13	44.05	48.60	57.6
27	46.26	+01	-.05	46.22	44.10	.00	-.04	44.06	44.09		-.10	43.99	45.57		-.20	45.37	51.60	64.0
3	46.18	+01	-.02	46.17	44.24	.00	-.02	44.22	44.80		-.04	44.76	47.21		-.06	47.15	48.20	51.9
10	46.15	+01	-.04	46.12	44.45	.00	-.03	44.42	45.57		-.08	45.49	48.64		-.14	48.50	53.80	59.9
17	46.13	.00	-.06	46.07	44.71	.00	-.05	44.66	46.42		-.14	46.28	49.54		-.25	49.29	58.10	68.8
24	46.06	-.01	-.05	46.00	44.99	-.01	-.04	44.94	47.65		-.11	47.54	51.72		-.15	51.57	55.00	63.0
1	46.06	-.01	-.06	45.99	45.36	-.01	-.06	45.29	48.32		-.14	48.18	51.04		-.22	50.82	53.50	67.5
8	46.06	-.01	-.07	45.98	45.72	-.01	-.07	45.64	48.76		-.16	48.60	52.69		-.26	52.43	59.20	70.0
15	46.04	-.01	-.04	45.99	46.05	-.01	-.05	45.99	49.49		-.10	49.39	53.00		-.14	52.86	54.40	61.7
22	46.05	-.01	-.05	45.99	46.42	-.01	-.06	46.35	49.87		-.11	49.76	52.81		-.15	52.66	56.20	63.2
29	46.09	-.02	-.05	46.02	46.76	-.01	-.06	46.69	50.19		-.13	50.06	53.48		-.19	53.29	56.70	65.1
5	46.14	-.02	-.06	46.06	47.01	-.01	-.08	46.92	50.62		-.16	50.46	54.12		-.22	53.90	58.00	68.3
12	46.16	-.02	-.04	46.10	47.35	-.01	-.05	47.29	50.99		-.08	50.91	53.90		-.09	53.81	54.30	60.2
19	46.19	-.02	-.01	46.16	47.62	-.01	-.01	47.60	51.02		+01	51.03	52.63		+05	52.68	49.70	49.8
26	46.27	-.02	-.04	46.21	47.89	-.01	-.05	47.83	50.82		-.08	50.74	52.58		-.10	52.48	54.40	59.9
2	46.33	-.02	-.04	46.27	48.07	-.01	-.06	48.00	50.81		-.09	50.72	52.50		-.12	52.38	53.40	60.9
9	46.40	-.02	-.04	46.34	48.24	.00	-.05	48.19	50.83		-.07	50.76	52.29		-.10	52.19	54.20	59.0
16	46.49	-.02	-.03	46.44	48.36	.00	-.04	48.32	50.79		-.05	50.74	51.51		-.09	51.42	52.00	57.2
23	46.58	-.02	-.04	46.52	48.50	.00	-.05	48.45	50.55		-.08	50.47	51.03		-.13	50.90	50.50	59.8
30	46.65	-.02	-.03	46.60	48.56	.00	-.04	48.52	50.28		-.06	50.22	50.61		-.08	50.53	49.20	57.0
7	46.72	-.01	-.03	46.68	48.60	.00	-.04	48.56	49.89		-.05	49.84	48.75		-.09	48.66	46.40	56.1
14	46.79	-.01	-.02	46.76	48.59	.00	-.02	48.57	49.30		-.03	49.27	49.02		-.05	48.97	48.50	53.0
21	46.88	-.01	-.03	46.84	48.55	.00	-.03	48.52	49.10		-.05	49.05	48.10		-.10	48.00	45.80	55.8
28	46.94	-.01	-.01	46.92	48.49	.00	.00	48.49	48.70		.00	48.70	48.05		.00	48.05	43.60	48.5
4	46.99	-.01	.00	47.00	48.39	.00	+01	48.40	48.23		+01	48.24	46.52		+01	46.53	44.00	46.0
11	47.06	.00	-.01	47.05	48.31	.00	-.01	48.30	47.74		-.01	47.73	46.30		-.05	46.25	45.00	49.9
18	47.11	.00	.00	47.11	48.18	.00	.00	48.18	47.42		.00	47.42	46.31		-.01	46.30	43.90	47.0
25	47.14	.00	+03	47.17	48.02	.00	+03	48.05	47.11		+05	47.16	45.12		+07	45.19	39.50	39.6
2	47.17	.00	+04	47.21	47.87	.00	+05	47.92	46.30		+06	46.36	42.99		+06	43.05	34.80	36.0
9	47.20	+01	+04	47.25	47.68	+01	+05	47.74	45.44		+06	45.50	41.60		+05	41.65	33.50	35.9
16	47.24	+01	+02	47.27	47.45	+01	+03	47.49	44.61		+02	44.63	41.59		+01	41.60	38.50	40.7
23	47.28	+01	+01	47.30	47.18	+01	.00	47.19	44.30		-.01	44.29	42.14		-.04	42.10	42.00	45.7
31	47.28	+01	+02	47.31	46.86	+01	+02	46.89	44.14		+01	44.15	40.63		-.01	40.62	37.40	42.0
6	47.27	+01	+04	47.32	46.65	+01	+04	46.70	43.60		+04	43.64	40.66		+04	40.70	33.00	36.0
13	47.29	+02	.00	47.31	46.44	+01	.00	46.45	43.16		-.02	43.14	40.08		-.06	40.02	41.80	47.0
20	47.25	+02	+02	47.29	46.18	+01	+02	46.21	43.07		+01	43.08	41.02		.00	41.02	36.50	40.6
27	47.22	+02	+04	47.28	45.91	+01	+03	45.95	42.90		+04	42.94	40.49		+05	40.54	33.00	34.9
3	47.20	+02	+03	47.25	45.72	+01	+02	45.75	42.44		+01	42.45	39.10		.00	39.10	36.20	39.5
11	47.17	+02	.00	47.19	45.48	+01	.00	45.49	42.08		-.02	42.06	39.59		-.05	39.54	37.40	45.8
17	47.13	+02	+02	47.17	45.30	+01	+01	45.32	42.05		+01	42.06	40.29		-.01	40.28	40.00	41.5
24	47.08	+02	+02	47.12	45.10	+01	+01	45.12	42.06		+01	42.07	39.14		-.01	39.13	33.40	39.9
2	47.02	+03	+02	47.07	44.95	+01	+01	44.97	41.59		.00	41.59	38.25		-.02	38.23	32.80	41.3
9	47.01	+03	-.03	47.01	44.81	+01	-.02	44.80	41.24		-.05	41.19	38.40		-.13	38.27	38.90	56.0
16	46.91	+03	+01	46.95	44.59	+01	.00	44.60	41.26		-.01	41.25	40.21		-.04	40.17	40.40	44.9
23	46.84	+02	+04	46.90	44.42	+01	+02	44.45	41.67		+02	41.69	40.45		+03	40.48	36.60	36.3
30	46.79	+02	.00	46.81	44.34	.00	.00	44.34	41.76		-.02	41.74	40.70		-.05	40.65	42.50	46.4
6	46.72	+02	+01	46.75	44.26	.00	.00	44.26	42.08		-.01	42.07	41.46		-.03	41.43	41.20	44.0
13	46.69	+02	-.04	46.67	44.26	.00	-.03	44.23	42.33		-.06	42.27	42.39		-.15	42.24	46.20	58.2
20	46.62	+02	-.03	46.61	44.24	.00	-.03	44.21	42.90		-.06	42.84	43.63		-.13	43.50	46.30	56.3
27	46.58	+01	-.05	46.54	44.30	.00	-.04	44.26	43.58		-.10	43.48	45.79		-.20	45.59	53.40	64.0
5	46.47	+01	+01	46.49	44.40	.00	.00	44.40	44.73		+01	44.74	47.71		+05	47.76	46.20	44.0
11	46.40	+01	+01	46.42	44.55	.00	.00	44.55	45.31		+01	45.32	45.72		+02	45.74	40.00	43.5
18	46.35	+01	-.01	46.35	44.80	.00	-.01	44.79	45.10		-.02	45.08	45.42		-.03	45.39	42.70	48.6
25	46.32	+01	-.02	46.31	44.97	.00	-.02	44.95	45.10		-.04	45.06	46.03		-.08	45.95	48.80	53.5
1	46.31	+01	-.05	46.27	45.10	.00	-.05	45.05	45.55		-.11	45.44	47.15		-.21	46.94	54.60	63.9
8	46.29	.00	-.07	46.22	45.24	.00	-.07	45.17	46.25		-.15	46.10	48.50		-.28	48.22	54.20	70.0
15	46.25	.00	-.04	46.21	45.40	.00	-.04	45.36	46.91		-.09	46.82	49.59		-.15	49.44	52.80	60.2

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5
1810.																	
June 22	46.24	.00	-.04	46.20	45.62	.00	-.04	45.58	47.42	.00	-.08	47.34	49.70	.00	-.13	49.57	52.50
29	46.26	-.01	-.05	46.20	45.89	.00	-.05	45.84	47.79		-.12	47.67	50.02		-.21	49.81	55.30
July 6	46.22	-.01	-.03	46.18	46.09	.00	-.03	46.06	48.25		-.06	48.19	50.60		-.07	50.53	51.70
13	46.26	-.01	-.05	46.20	46.36	-.01	-.06	46.29	48.63		-.12	48.51	50.98		-.18	50.80	52.20
20	46.26	-.01	-.04	46.21	46.59	-.01	-.04	46.54	48.99		-.08	48.91	51.43		-.13	51.30	55.70
27	46.31	-.01	-.07	46.23	46.84	-.01	-.08	46.75	49.42		-.17	49.25	51.87		-.26	51.61	57.40
Aug. 3	46.33	-.01	-.06	46.26	47.06	-.01	-.07	46.98	49.75		-.13	49.62	52.43		-.21	52.22	57.50
10	46.36	-.02	-.04	46.30	47.28	-.01	-.06	47.21	50.20		-.10	50.10	53.95		-.13	53.82	60.30
18	46.41	-.02	-.05	46.34	47.59	-.01	-.07	47.51	51.01		-.12	50.89	53.67		-.17	53.50	53.50
24	46.45	-.02	-.05	46.38	47.81	-.01	-.06	47.74	51.08		-.10	50.98	53.94		-.13	53.81	55.60
31	46.51	-.02	-.05	46.44	48.07	-.01	-.06	48.00	51.33		-.10	51.23	53.87		-.13	53.74	54.30
Sept. 7	46.57	-.02	-.04	46.51	48.30	-.01	-.06	48.23	51.40		-.09	51.31	52.90		-.12	52.78	54.80
14	46.60	-.02	-.01	46.57	48.45	.00	-.01	48.44	51.10		+0.01	51.11	51.92		+0.03	51.95	48.90
21	46.68	-.02	-.02	46.64	48.63	.00	-.02	48.61	50.69		-.02	50.67	50.39		-.06	50.33	49.20
28	46.76	-.02	-.05	46.69	48.74	.00	-.06	48.68	50.20		-.09	50.11	49.76		-.16	49.60	53.00
Oct. 5	46.82	-.01	-.02	46.79	48.74	.00	-.02	48.72	49.79		-.03	49.76	48.95		-.07	48.88	46.80
12	46.91	-.01	-.03	46.87	48.72	.00	-.04	48.68	49.31		-.06	49.25	48.40		-.10	48.30	46.10
19	46.96	-.01	-.02	46.93	48.64	.00	-.02	48.62	48.93		-.03	48.90	48.45		-.05	48.40	46.70
26	47.01	-.01	.00	47.00	48.54	.00	+0.01	48.55	48.65		+0.02	48.67	47.30		+0.02	47.32	41.10
Nov. 2	47.08	-.01	.00	47.07	48.46	.00	.00	48.46	48.03		.00	48.03	46.27		-.02	46.25	45.40
9	47.12	.00	+0.01	47.13	48.33	.00	+0.02	48.35	47.60		+0.03	47.63	45.90		+0.02	45.92	42.20
17	47.18	.00	+0.03	47.21	48.15	.00	+0.04	48.19	46.99		+0.05	47.04	44.41		+0.06	44.47	39.00
23	47.25	.00	-.02	47.23	48.06	.00	-.02	48.04	46.40		-.04	46.36	43.07		-.09	42.98	43.20
30	47.29	.00	-.02	47.27	47.86	.00	-.02	47.84	45.86		-.04	45.82	43.01		-.10	42.91	46.00
Dec. 7	47.30	+0.01	+0.01	47.32	47.61	.00	+0.01	47.62	45.50		.00	45.50	43.44		-.02	43.42	42.00
14	47.29	+0.01	+0.05	47.35	47.35	+0.01	+0.06	47.42	45.20		+0.07	45.27	42.64		+0.09	42.73	34.00
21	47.31	+0.01	+0.05	47.37	47.15	+0.01	+0.05	47.21	44.58		+0.05	44.63	41.25		+0.06	41.31	34.20
28	47.30	+0.01	+0.05	47.36	46.93	+0.01	+0.05	46.99	43.89		+0.05	43.94	39.80		+0.05	39.85	32.50
1811.																	
Jan. 5	47.29	+0.02	+0.05	47.36	46.63	+0.01	+0.05	46.69	43.18		+0.04	43.22	39.83		+0.05	39.88	31.00
11	47.28	+0.02	+0.05	47.35	46.38	+0.01	+0.05	46.44	42.72		+0.04	42.76	38.31		+0.04	38.35	30.20
18	47.27	+0.02	+0.04	47.33	46.10	+0.01	+0.04	46.15	41.96		+0.03	41.99	37.43		+0.01	37.44	30.20
25	47.25	+0.03	+0.04	47.32	45.79	+0.01	+0.03	45.83	41.39		+0.02	41.41	37.25		+0.01	37.26	30.80
Feb. 1	47.21	+0.03	+0.05	47.29	45.46	+0.01	+0.03	45.42	41.09		+0.03	41.12	38.43		+0.04	38.47	32.80
8	47.15	+0.03	+0.05	47.23	45.15	+0.01	+0.03	45.11	41.00		+0.03	41.03	37.56		+0.03	37.59	30.40
15	47.14	+0.03	+0.01	47.18	44.91	+0.01	.00	44.92	40.63		-.01	40.62	37.75		-.05	37.70	39.00
22	47.10	+0.03	-.01	47.12	44.66	+0.01	-.01	44.66	40.87		-.03	40.84	39.46		-.08	39.38	41.30
Mar. 1	47.01	+0.03	+0.01	47.05	44.45	+0.01	.00	44.46	41.29		-.01	41.28	39.92		-.02	39.90	36.80
8	46.97	+0.03	-.02	46.98	44.34	+0.01	-.02	44.33	41.40		-.04	41.36	39.89		-.11	39.78	41.00
15	46.91	+0.02	-.04	46.89	44.27	.00	-.03	44.24	41.78		-.06	41.72	41.87		-.15	41.72	47.20
22	46.82	+0.02	-.02	46.82	44.21	.00	-.02	44.19	42.45		-.04	42.41	42.88		-.09	42.79	44.60
29	46.75	+0.02	-.03	46.74	44.24	.00	-.02	44.22	42.92		-.05	42.87	43.37		-.11	43.26	41.10
Apr. 5	46.65	+0.02	.00	46.67	44.29	.00	.00	44.29	43.20		-.01	43.19	42.90		-.03	42.87	42.00
12	46.59	+0.02	.00	46.61	44.37	.00	-.01	44.36	43.30		-.02	43.28	42.89		-.06	42.83	40.80
19	46.53	+0.02	-.01	46.54	44.44	.00	-.01	44.43	43.38		-.04	43.34	43.10		-.09	43.01	42.80
26	46.48	+0.02	-.02	46.48	44.49	.00	-.01	44.48	43.50		-.04	43.46	43.26		-.10	43.16	45.20
May 3	46.41	+0.01	.00	46.42	44.53	.00	.00	44.53	43.80		-.02	43.78	44.96		-.03	44.93	42.00
10	46.39	+0.01	-.03	46.37	44.61	.00	-.03	44.58	44.30		-.07	44.23	45.23		-.14	45.09	49.40
17	46.34	+0.01	-.02	46.33	44.71	.00	-.02	44.69	44.80		-.04	44.76	46.55		-.09	46.46	49.00
24	46.34	+0.01	-.07	46.28	44.89	.00	-.05	44.84	45.49		-.14	45.35	47.29		-.28	47.01	56.20
31	46.31	.00	-.06	46.25	45.06	.00	-.05	45.01	46.15		-.14	46.01	49.18		-.26	48.92	56.60
June 7	46.25	.00	.00	46.23	45.24	.00	-.01	45.23	46.99		-.04	46.95	49.70		-.05	49.65	49.00
14	46.24	-.01	-.02	46.21	45.50	.00	-.02	45.48	47.50		-.05	47.45	50.39		-.06	50.33	52.20
21	46.24	-.01	-.05	46.18	45.80	-.01	-.05	45.74	48.10		-.11	47.99	50.62		-.18	50.44	54.50
28	46.21	-.01	-.03	46.17	46.07	-.01	-.03	46.03	48.45		-.06	48.39	50.60		-.11	50.49	52.60
July 5	46.22	-.01	-.03	46.18	46.35	-.01	-.03	46.31	48.80		-.06	48.76	51.90		-.09	51.81	53.60
12	46.25	-.01	-.05	46.19	46.63	-.01	-.06	46.56	49.30		-.11	49.19	51.30		-.20	51.10	54.00
19	46.27	-.01	-.04	46.22	46.87	-.01	-.04	46.82	49.44		-.09	49.35	51.79		-.13	51.66	56.00
26	46.30	-.01	-.05	46.24	47.11	-.01	-.06	47.04	49.82		-.11	49.71	52.37		-.20	52.17	58.80
Aug. 2	46.34	-.01	-.05	46.28	47.33	-.01	-.06	47.26	50.10		-.11	49.99	51.93		-.17	51.76	53.50
9	46.39	-.02	-.04	46.33	47.55	-.01	-.05	47.49	50.27		-.10	50.17	52.58		-.13	52.45	54.40
16	46.44	-.02	-.06	46.36	47.79	-.01	-.07	47.71	50.42		-.13	50.29	52.03		-.20	51.83	56.20
23	46.48	-.02	-.03	46.43	47.94	-.01	-.04	47.89	50.52		-.07	50.44	52.67		-.09	52.58	53.00

tes.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
111.																		
Ar. 30	46.54	-.02	-.03	46.49	48.10	-.01	-.05	48.04	50.70	-.00	-.08	50.62	53.05	-.00	-.12	52.93	57.00	60.8
Se. 6	46.59	-.02	-.02	46.54	48.26	-.01	-.03	48.22	50.90	-.00	-.03	50.87	52.05		-.04	52.01	48.90	54.7
13	46.69	-.02	-.03	46.64	48.47	-.00	-.04	48.43	50.71	-.00	-.06	50.65	52.19		-.10	52.09	58.30	58.8
20	46.75	-.02	-.04	46.69	48.56	-.01	-.05	48.50	50.96	-.00	-.08	50.88	53.25		-.12	53.13	56.80	60.7
27	46.80	-.02	-.02	46.76	48.67	-.01	-.03	48.63	51.09	-.00	-.02	51.07	52.34		-.03	52.31	51.30	54.3
4	46.87	-.02	-.01	46.84	48.80	-.00	-.02	48.78	50.92	-.00	-.01	50.91	51.34		-.03	51.31	47.80	53.5
11	46.94	-.02	-.01	46.91	48.91	-.00	-.01	48.90	50.43	-.00	-.01	50.42	50.07		-.03	50.04	48.60	51.9
18	47.00	-.01	.00	46.99	48.92	-.00	+.01	48.93	49.90	-.00	+.04	49.94	48.68		+.05	48.73	42.00	44.9
25	47.06	-.01	.00	47.05	48.89	-.00	+.01	48.90	49.03	-.00	+.03	49.06	46.48		+.01	46.49	41.10	44.7
N. 1	47.13	-.01	.00	47.12	48.80	-.00	.00	48.80	48.13	-.00	.00	48.13	45.41		-.02	45.39	43.00	47.4
8	47.21	.00	-.01	47.20	48.65	-.00	-.01	48.64	47.50	-.00	-.03	47.47	45.25		-.07	45.32	47.20	52.5
15	47.23	.00	+.03	47.26	48.39	.00	+.05	48.44	47.18	.00	+.07	47.25	44.75		+.10	44.85	33.50	35.6
22	47.28	.00	+.03	47.31	48.20	+.01	+.04	48.25	46.24	.00	+.05	46.29	41.74		+.03	41.77	35.50	38.5
29	47.34	.00	.00	47.34	47.98	+.01	.00	47.99	45.20	+.01	.00	45.21	41.15		-.04	41.11	39.00	46.4
6	47.39	+.01	.00	47.40	47.68	+.01	.00	47.69	44.75	.00	.00	44.75	42.61		-.03	42.58	42.80	46.0
13	47.41	+.01	+.01	47.44	47.37	+.01	+.01	47.39	44.77	.00	.00	44.77	42.42		-.02	42.40	41.00	44.0
20	47.38	+.01	+.05	47.44	47.07	+.01	+.06	47.14	44.40	.00	+.06	44.46	40.99		+.08	41.07	31.40	31.2
27	47.41	+.01	+.02	47.44	46.86	+.01	+.02	46.89	43.62	.00	+.02	43.64	39.85		-.04	39.81	34.30	39.8
112.																		
a. 3	47.40	+.02	+.04	47.46	46.62	+.01	+.03	46.66	43.24	.00	+.03	43.27	41.06		.00	41.06	36.20	36.5
10	47.35	+.02	+.06	47.43	46.32	+.01	+.05	46.38	43.03	.00	-.05	42.98	39.42		.00	39.42	31.20	30.0
17	47.34	+.02	+.03	47.39	46.10	+.01	+.03	46.14	42.45	.00	-.02	42.43	38.42		-.03	38.39	32.40	37.2
24	47.31	+.02	+.04	47.37	45.83	+.01	+.03	45.87	41.96	.00	-.02	41.94	38.63		-.02	38.61	32.00	35.3
31	47.28	+.02	+.02	47.32	45.57	+.01	+.01	45.59	41.63	.00	.00	41.63	38.26		-.02	38.24	37.00	40.8
7	47.22	+.02	+.03	47.27	45.29	+.01	+.03	45.33	41.50	.00	-.01	41.49	39.07		-.03	39.04	31.60	35.7
14	47.19	+.03	+.01	47.23	45.08	+.01	+.01	45.10	41.41	.00	.00	41.41	39.42		-.05	39.37	36.50	43.1
21	47.12	+.02	+.02	47.16	44.87	+.01	+.01	44.89	41.57	.00	.00	41.57	40.13		-.02	40.11	37.80	40.8
28	47.06	+.03	+.01	47.10	44.72	+.01	+.01	44.74	41.73	.00	.00	41.73	39.51		-.04	39.47	36.10	42.0
F. 7	47.02	+.03	-.02	47.03	44.61	+.01	-.01	44.61	41.56	.00	-.04	41.52	39.72		-.09	39.63	41.10	53.0
14	46.95	+.03	-.01	46.97	43.49	+.01	.00	44.50	41.64	.00	-.03	41.61	40.10		-.08	40.02	41.70	51.5
21	46.85	+.02	+.01	46.88	44.37	.00	+.01	44.38	41.94	.00	.00	41.94	41.11		-.04	41.07	35.90	41.1
28	46.81	+.02	-.02	46.81	44.33	.00	-.01	44.32	42.04	.00	-.04	42.00	40.65		-.08	40.57	44.20	54.0

EXPERIMENTAL GARDEN.

te.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
137.																		
4	48.13	+.05	+.03	48.21	45.41	+.02	+.01	45.44	40.99	.00	.00	40.99	38.39	.00	-.03	38.36		42
12	47.96	+.05	+.04	48.05	45.04	+.01	+.02	45.07	40.88	.00	.00	40.88	40.06		.00	40.06		40
20	47.88	+.05	+.03	47.96	44.75	+.01	+.01	44.77	41.11	.00	.00	41.11	40.53		-.01	40.52		42
27	47.85	+.05	+.03	47.93	44.61	+.01	+.01	44.63	41.21	.00	.00	41.21	38.63		-.03	38.60		42
6	47.70	+.05	+.02	47.77	44.45	+.01	.00	44.46	40.78	.00	-.01	40.77	38.99		-.04	38.95		44
13	47.63	+.05	+.01	47.69	44.25	+.01	-.01	44.25	40.72	.00	-.02	40.70	39.30		-.05	39.25		46
20	47.48	+.06	+.03	47.57	44.07	+.01	+.01	44.09	40.50	.00	.00	40.50	37.68		-.03	37.65		42
27	47.32	+.06	+.06	47.44	43.87	+.01	+.03	43.91	40.11	.00	+.02	40.13	37.58		+.02	37.60		35
3	47.20	+.06	+.06	47.32	43.68	+.01	+.03	43.72	39.79	.00	+.02	39.81	37.48		+.02	37.50		35
10	47.20	+.06	+.01	47.27	43.48	+.01	-.01	43.48	39.74	.00	-.02	39.72	38.03		-.06	37.97		46
17	47.07	+.06	-.04	47.09	43.35	+.01	-.04	43.32	39.98	.00	-.05	39.93	39.10		-.13	38.97		56
24	46.94	+.05	-.02	46.97	43.20	+.01	-.02	43.19	40.48	.00	-.04	40.44	40.63		-.08	40.55		50
1	46.86	+.05	-.06	46.85	43.08	.00	-.05	43.03	41.25	.00	-.08	41.17	42.52		-.16	42.36		60
8	46.74	+.04	-.05	46.73	43.18	.00	-.04	43.14	41.98	.00	-.07	41.91	43.90		-.15	43.75		58
15	46.70	+.04	-.11	46.63	43.35	.00	-.08	43.27	42.80	.00	-.14	42.66	44.81		-.28	44.53		70
22	46.58	+.04	-.05	46.57	43.51	.00	-.04	43.47	43.60	.00	-.07	43.53	46.42		-.13	46.29		57
29	46.50	+.03	-.08	46.45	43.79	-.01	-.06	43.72	44.81	-.01	-.10	44.70	47.98		-.18	47.80		62
5	46.45	+.03	-.10	46.38	44.09	-.01	-.07	44.01	45.62	-.01	-.13	45.48	48.80		-.23	48.57		66

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	1	
1837.																			
June 12	46.41	+01	-12	46.30	44.45	-01	-09	44.35	46.72	-01	-18	46.53	50.41	-00	-30	50.11	0	7	
2	46.39	-01	-13	46.25	44.95	-02	-11	44.82	48.12	-01	-22	47.89	53.00		-33	52.67	7	7	
26	46.35	-02	-13	46.20	45.30	-02	-12	45.16	49.29	-01	-22	49.06	54.80		-33	54.47	7	7	
July 3	46.34	-02	-13	46.19	45.89	-02	-13	45.74	50.62	-01	-24	50.37	55.80		-35	55.45	7	7	
10	46.35	-04	-17	46.14	46.49	-03	-17	46.29	51.98	-01	-32	51.65	57.82		-47	57.35	8	8	
17	46.35	-05	-13	46.17	47.16	-03	-15	46.98	53.34	-01	-25	53.08	57.91		-33	57.58	7	7	
24	46.39	-05	-13	46.21	47.74	-03	-15	47.56	53.82	-01	-23	53.58	58.09		-31	57.78	7	7	
31	46.14	-05	-08	46.31	48.51	-02	-08	48.41	54.30	-00	-10	54.20	56.72		-11	56.61	6	6	
Aug. 7	16.53	-05	-18	46.30	48.90	-02	-21	48.67	54.52	-00	-32	54.20	56.38		-47	55.91	8	8	
11	16.62	-05	-14	46.43	49.35	-02	-16	49.17	54.39	-00	-24	54.15	56.89		-32	56.57	7	7	
21	46.69	-06	-11	46.52	49.70	-02	-12	49.56	54.60	-01	-15	54.44	57.83		-18	57.65	6	6	
28	46.82	-06	-10	46.66	50.02	-02	-11	49.89	54.83	-00	-14	54.69	56.34		-19	56.15	6	6	
Sept. 4	46.93	-05	-07	46.81	50.30	-01	-08	50.21	54.30	-00	-09	54.21	54.40		-13	54.27	6	6	
11	47.03	-05	-05	46.93	50.46	-01	-04	50.41	53.54	-00	-03	53.51	53.62		-03	53.59	5	5	
18	47.21	-05	-10	47.06	50.62	-01	-11	50.50	53.23	-00	-15	53.08	52.68		-22	52.46	5	5	
25	47.34	-05	-05	47.24	50.58	-01	-04	50.53	52.74	-00	-03	52.71	53.10		-05	53.05	5	5	
Oct. 2	47.46	-04	-08	47.34	50.60	-00	-09	50.51	52.50	-00	-11	52.39	52.00		-16	51.84	6	6	
9	47.60	-04	-05	47.51	50.55	-00	-05	50.50	52.26	-00	-06	52.20	52.93		-08	52.85	5	5	
16	47.68	-01	-05	47.59	50.53	-00	-04	50.49	52.12	-00	-05	52.07	51.61		-08	51.53	5	5	
23	47.75	-01	+02	47.73	50.42	-00	+04	50.46	51.50	-00	+08	51.58	50.79		+10	50.89	4	4	
30	47.81	-03	+04	47.82	50.35	-00	+07	50.42	50.83	-00	+10	50.93	47.98		+10	48.08	4	4	
Nov. 6	47.96	-01	-03	47.92	50.27	+01	-02	50.26	49.30	+01	-04	49.27	44.81		-09	44.72	5	5	
13	47.98	-01	+04	48.01	50.02	+01	+06	50.09	48.29	-00	+06	49.35	45.95		+06	46.01	4	4	
20	48.04	-00	+04	48.08	49.65	+01	+06	49.72	47.34	+01	+05	47.40	43.05		+02	43.07	4	4	
27	48.09	-00	+01	48.13	49.30	+01	+06	49.37	46.41	+01	+04	46.46	43.08		+03	43.11	4	4	
Dec. 4	48.15	+03	+01	48.19	48.92	+01	+02	48.95	45.60	+01	-01	45.60	42.38		-04	42.34	4	4	
11	48.15	+03	+07	48.25	48.40	+02	+07	48.49	44.70	+01	+06	44.77	41.00		+04	41.04	3	3	
18	48.23	+03	-02	48.24	48.13	+02	-02	48.13	44.20	+01	-05	44.16	41.20		-10	41.10	4	4	
25	48.22	+03	-01	48.24	47.71	+01	-01	47.71	44.01	-00	-04	43.97	41.48		-08	41.40	5	5	
1838.																			
Jan. 1	48.18	+03	+03	48.24	17.35	+01	+03	47.39	44.20	-00	+01	44.21	43.34		+01	43.35	4	4	
8	18.10	+03	+09	48.22	17.06	+01	+08	47.15	44.06	-00	+07	44.13	42.03		+09	42.12	3	3	
15	18.00	+01	+09	48.13	16.80	+01	+07	46.88	43.49	+01	+06	43.56	39.62		+07	39.69	3	3	
22	48.03	+04	+06	48.13	16.55	+02	+05	46.62	42.58	+01	+03	42.62	38.42		+01	38.43	3	3	
29	17.90	+05	+08	48.03	16.20	+02	+05	46.27	41.70	+01	+04	41.75	37.60		+03	37.63	3	3	
Feb. 5	47.89	+05	+07	48.01	15.80	+02	+05	45.87	41.00	+01	+03	41.04	37.10		+01	37.11	3	3	
12	47.85	+05	+02	47.92	15.58	+02	+01	15.61	40.52	+01	-01	40.52	36.50		-04	36.46	4	4	
19	17.70	+07	+07	47.84	15.05	+02	+04	45.11	39.90	+01	+01	39.92	36.00		-00	36.00	3	3	
26	47.65	+07	+09	47.81	14.65	+02	+05	44.72	39.40	+01	+02	39.43	35.50		+02	35.52	3	3	
Mar. 5	47.65	+07	-07	47.65	14.45	+02	-06	44.41	39.02	+01	-07	38.96	35.38		-14	35.24	3	3	
12	17.55	+08	-03	47.60	14.01	+02	-03	44.00	38.62	+01	-04	38.59	35.06		-10	34.96	3	3	
19	17.42	+07	+01	47.50	13.59	+02	-01	43.60	38.60	-00	-02	38.58	37.20		-07	37.13	3	3	
26	17.30	+07	+01	47.38	13.35	+02	-01	43.36	38.90	-00	-02	38.88	37.40		-05	37.35	3	3	
Apr. 2	47.17	+07	+01	47.25	13.05	+01	-01	43.05	39.30	-00	-02	39.28	39.49		-05	39.44	4	4	
9	17.05	+07	-01	47.11	12.85	+01	-01	42.85	39.91	-00	-03	39.88	40.08		-07	40.01	4	4	
16	16.93	+06	+01	47.00	12.86	+01	-01	42.86	40.58	-00	-02	40.56	41.05		-04	41.01	4	4	
23	46.80	+05	+01	46.86	12.91	+01	-00	42.92	40.90	-00	-01	40.89	40.63		-03	40.60	4	4	
30	46.65	+05	+01	46.71	12.83	-00	-00	42.83	41.30	-00	-01	41.29	41.52		-03	41.49	41.2	4	
May 7	46.66	+04	-12	46.58	13.05	-00	-08	42.97	41.92	-00	-15	41.77	43.51		-28	43.23	31.4?	4	
14	46.50	+04	-01	46.53	13.12	-00	-02	43.10	42.99	-00	-02	42.97	45.70		-02	45.68	42.2	4	
21	46.41	+03	-04	46.40	13.32	-00	-03	43.29	43.70	-00	-06	43.64	45.23		-11	45.12	46.9	4	
28	46.30	+03	-05	46.28	13.62	-00	-04	43.58	44.50	-00	-07	44.43	46.42		-14	46.28	50.1	4	
June 4	46.30	+01	-08	46.23	14.00	-01	-07	43.92	45.32	-00	-13	45.19	47.80		-22	47.58	56.2	4	
11	46.20	+01	-05	46.16	14.25	-01	-05	44.19	46.10	-01	-08	46.01	49.10		-12	48.98	51.8	4	
18	46.15	-00	-03	46.12	14.64	-01	-03	44.60	46.90	-01	-04	46.85	50.20		-04	50.16	52.2	4	
25	46.20	-01	-11	46.08	15.08	-01	-10	44.99	48.03	-01	-18	47.84	51.71		-28	51.43	58.1	4	
July 2	46.12	-01	-04	46.07	15.50	-02	-03	45.45	48.98	-01	-04	48.93	52.81		-02	52.79	52.1	4	
9	46.15	-03	-07	46.05	16.00	-02	-07	45.91	50.01	-01	-11	49.92	54.51		-13	54.38	59.1	4	
16	46.16	-01	-09	46.03	16.13	-02	-09	46.32	51.08	-01	-16	50.91	55.50		-20	55.30	60.4	4	
23	46.26	-01	-11	46.11	17.00	-02	-11	46.87	51.63	-01	-18	51.44	55.00		-25	54.75	63.0	4	
30	46.30	-01	-11	46.15	17.50	-02	-12	47.36	52.10	-00	-18	51.92	54.83		-25	54.58	56.5	4	
Aug. 6	46.30	-05	-09	46.16	17.90	-02	-10	47.78	52.50	-01	-14	52.35	55.90		-17	55.73	59.1	4	
13	46.42	-05	-09	46.28	18.37	-02	-11	48.24	52.91	-00	-14	52.77	55.82		-17	55.65	56.1	4	

tes.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
138.																		
Ar. 20	46.49	-.05	-.06	46.38	48.75	-.02	-.07	48.66	53.12	.00	-.08	53.04	55.51	.00	-.08	55.43	56.1	60
27	46.61	-.05	-.14	46.42	49.12	-.02	-.16	48.94	53.29	.00	-.24	53.05	54.82		-.33	54.49	59.2	75
Se. 3	46.66	-.05	-.09	46.52	49.35	-.01	-.11	49.23	53.11	.00	-.14	52.97	54.63		-.18	54.45	54.9	66
10	46.80	-.05	-.09	46.66	49.51	-.01	-.10	49.40	52.80	.00	-.13	52.67	52.50		-.19	52.31	53.2	65
17	46.86	-.05	-.07	46.74	49.72	-.01	-.08	49.63	52.40	.00	-.10	52.30	53.51		-.14	53.37	54.1	62
24	47.01	-.04	-.10	46.87	49.85	-.01	-.11	49.73	52.48	.00	-.16	52.32	52.71		-.22	52.49	50.6	67
1	47.15	-.04	-.11	47.00	49.95	-.01	-.13	49.81	52.21	.00	-.18	52.03	52.60		-.25	52.35	53.1	69
8	47.25	-.04	-.10	47.11	49.97	.00	-.11	49.86	51.82	.00	-.16	51.66	50.70		-.23	49.47	50.2	67
15	47.30	-.03	-.08	47.19	49.90	.00	-.09	49.81	50.99	.00	-.12	50.87	48.81		-.19	48.62	48.2	63
22	47.42	-.03	-.07	47.32	49.89	.00	-.07	49.82	50.22	.00	-.10	50.12	48.68		-.16	48.52	52.8	61
29	47.48	-.03	-.01	47.44	49.69	.00	+.01	49.70	49.82	.00	+.01	49.83	48.89		.00	48.89	42.0	49
N. 5	47.59	-.01	+.04	47.62	49.52	.00	+.06	49.58	49.10	+.01	+.08	49.19	45.78		+.07	45.85	40.2	40
12	47.61	-.01	+.02	47.62	49.45	+.01	+.03	49.49	48.03	+.01	+.02	48.06	45.00		.00	45.00	33.4	45
19	47.67	.00	+.02	47.69	49.10	+.01	+.03	49.14	47.01	+.01	+.02	47.04	43.04		-.01	43.03	35.0	44
26	47.70	+.01	+.05	47.70	48.72	+.01	+.06	48.79	45.82	+.01	+.05	45.88	42.03		+.03	42.06	33.3	39
3	47.71	+.01	+.03	47.69	48.44	+.01	-.04	48.41	45.10	+.01	-.06	45.05	42.61		-.11	42.50	43.1	54
10	47.82	+.01	.00	47.83	48.01	+.01	.00	48.02	44.91	.00	-.02	44.89	42.05		-.06	41.99	42.1	48
17	47.81	+.03	+.03	47.87	47.61	+.01	+.03	47.65	44.51	.00	+.01	44.52	41.70		.00	41.70	36.2	42
24	47.80	+.03	+.03	47.86	47.30	+.01	+.02	47.33	44.91	+.01	+.01	44.93	40.53		-.02	40.51	38.8	43
31	47.81	+.04	+.01	47.86	46.95	+.01	+.01	46.97	43.20	.00	-.02	43.18	40.50		-.05	40.45	39.2	46
139.																		
7	47.75	+.04	+.05	47.84	46.60	+.01	+.04	46.65	42.91	.00	+.02	42.93	40.01		+.01	40.02	35.8	39
14	47.72	+.04	+.04	47.80	46.32	+.02	+.03	46.37	42.28	+.01	+.01	42.30	39.30		-.01	39.29	37.0	40
21	47.70	+.05	+.06	47.81	46.02	+.02	+.04	46.08	41.82	+.01	+.02	41.85	38.03		.00	38.03	33.0	38
28	47.70	+.05	+.07	47.82	45.61	+.02	+.05	45.68	41.20	+.01	+.03	41.24	37.90		+.02	37.92	30.4	35
4	47.53	+.05	+.06	47.64	45.30	+.02	+.04	45.36	40.60	+.01	+.02	40.63	37.01		.00	37.01	30.4	37
11	47.51	+.04	+.01	47.56	45.01	+.01	.00	45.02	44.32?	+.01	-.01	44.32	38.53		-.04	38.49	40.2	45
18	47.42	+.05	+.04	47.51	44.60	+.01	+.02	44.63	40.60	.00	.00	40.60	38.52		-.01	38.51	31.8	40
25	47.38	+.05	+.02	47.45	44.35	+.02	.00	44.37	40.30	.00	-.01	40.29	37.51		-.03	37.48	32.2	43
4	47.30	+.06	+.03	47.39	44.21	+.02	+.01	44.24	40.01	.00	-.01	40.00	38.30		-.03	38.27	36.2	42
11	47.32	+.06	+.02	47.40	43.85	+.02	.00	43.87	40.04	.00	-.01	40.03	37.40		-.04	37.36	31.9	43
18	47.05	+.06	+.01	47.12	43.70	+.01	.00	43.71	39.71	.00	-.02	39.69	38.15		-.05	38.10	35.1	45
25	47.01	+.06	.00	47.07	43.50	+.01	-.01	43.50	39.99	.00	-.02	39.97	38.80		-.05	38.75	37.0	46
1	46.85	+.06	+.03	46.94	43.32	+.01	+.01	43.34	40.05	.00	-.00	40.05	38.99		-.01	38.98	36.0	40
8	46.81	+.06	-.04	46.83	43.25	+.01	-.03	43.23	40.12	.00	-.06	40.06	38.41		-.13	38.28	38.2	55
15	46.75	+.06	-.04	46.77	43.25	+.01	-.03	43.23	40.20	.00	-.06	40.12	40.30		-.13	40.17	43.4	55
22	46.60	+.05	-.03	46.62	43.05	+.01	-.03	43.03	40.97	.00	-.05	40.92	41.51		-.10	41.41	44.8	53
29	46.51	+.05	-.04	46.52	43.20	+.01	-.03	43.18	41.01	.00	-.06	40.95	41.72		-.12	41.60	43.5?	55
6	46.30	+.03	-.02	46.31	43.20	.00	-.02	43.18	42.90	.00	-.04	42.86	45.30		-.06	45.24	45.8	50
13	46.39	+.03	-.01	46.41	43.41	.00	-.01	43.40	43.70	.00	-.02	43.68	45.60		-.02	45.58	45.2	48
20	46.30	+.02	-.05	46.27	43.70	.00	-.04	43.66	44.30	.00	-.08	44.22	46.00		-.13	45.87	50.1	57
27	46.40	+.02	-.11	46.31	44.00	-.01	-.09	43.90	44.90	.00	-.15	44.75	47.30		-.28	47.02	54.0	70
3	46.10	+.01	-.05	46.06	44.40	-.01	-.05	44.34	45.96	-.01	-.07	45.88	49.40		-.11	49.29	50.1	57
10	46.02	.00	-.08	45.94	44.60	-.01	-.07	44.52	47.26	-.01	-.14	47.11	51.60		-.19	51.41	57.2	65
17	46.03	-.01	-.14	45.88	44.98	-.02	-.13	44.83	48.20	-.01	-.25	47.94	52.50		-.40	52.10	63.0	78
24	46.10	-.02	-.09	45.99	45.40	-.02	-.08	45.30	49.51	-.01	-.14	49.36	54.60		-.18	54.42	56.0	66
1	46.20	-.03	-.13	46.04	46.00	-.02	-.13	45.85	50.50	-.01	-.24	50.25	53.60		-.33	53.27	56.1	75
8	46.20	-.03	-.13	46.04	46.50	-.02	-.14	46.34	51.20	-.01	-.24	50.95	56.10		-.33	55.77	61.0	75
15	46.20	-.04	-.10	46.06	47.01	-.02	-.11	46.88	52.10	-.01	-.17	51.92	56.10		-.21	55.89	58.8	68
22	46.30	-.04	-.09	46.17	47.50	-.02	-.09	47.39	52.60	-.01	-.13	52.46	55.61		-.16	55.45	59.9	65
29																		
5	46.40	-.04	-.13	46.23	48.49	-.02	-.15	48.32	53.52	-.01	-.22	53.29	57.10		-.30	56.80	60.4	74
12	46.41	-.06	-.09	46.26	48.85	-.02	-.10	48.73	53.73	.00	-.12	53.61	56.70		-.14	56.56	57.3	65
19	46.49	-.06	-.02	46.41	49.28	-.02	-.10	49.25	53.80	.00	+.03	53.83	55.00		+.07	55.07	51.0	51
25	46.55	-.06	-.08	46.41	49.53	-.01	-.09	49.43	53.52	.00	-.11	53.41	54.81		-.15	54.66	56.2	64
2	46.73	-.06	-.07	46.60	49.90	-.01	-.08	49.81	53.30	.00	-.10	53.20	54.30		-.13	54.17	52.2	62
9	46.85	-.05	-.10	46.70	49.96	-.01	-.12	49.83	53.30	.00	-.16	53.14	54.38		-.23	54.15	57.4	68
16	46.92	-.05	-.06	46.81	50.05	-.01	-.07	49.97	53.02	.00	-.08	52.94	53.20		-.11	53.09	54.4	60
23	47.08	-.05	-.10	46.93	50.18	-.01	-.11	50.06	52.70	.00	-.15	52.55	52.60		-.21	52.39	52.8	67
30	47.17	-.04	-.09	47.04	50.22	-.01	-.10	50.11	52.28	.00	-.14	52.14	52.12		-.20	51.92	50.0	65
7	47.26	-.04	-.06	47.16	50.19	.00	-.06	50.13	51.68	.00	-.07	51.61	49.82		-.12	49.70	47.4	59
14	47.30	-.04	-.04	47.22	50.20	.00	-.03	50.17	50.84	.00	-.04	50.80	50.01		-.07	49.94	49.0	55
21	47.42	-.03	-.07	47.32	50.01	.00	-.07	49.94	50.61	.00	-.09	50.52	48.82		-.14	48.68	43.0	60

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T.
1837.																		
June 12	46.11	+01	-12	46.30	44.45	-01	-09	44.35	46.72	-01	-18	46.53	50.41	-00	-30	50.11	°	72
?	46.39	-01	-13	46.25	44.95	-02	-11	44.82	48.12	-01	-22	47.89	53.00		-33	52.67		75
26	46.35	-02	-13	46.20	45.30	-02	-12	45.16	49.29	-01	-22	49.06	54.80		-33	54.47		76
July 3	46.31	-02	-13	46.19	45.89	-02	-13	45.74	50.62	-01	-24	50.37	55.80		-35	55.45		77
10	46.35	-04	-17	46.14	46.49	-03	-17	46.29	51.98	-01	-32	51.65	57.82		-47	57.35		80
17	46.35	-05	-13	46.17	47.16	-03	-15	46.98	53.34	-01	-25	53.08	57.91		-33	57.58		70
24	46.39	-05	-13	46.21	47.74	-03	-15	47.56	53.82	-01	-23	53.58	58.09		-31	57.78		75
31	46.44	-05	-08	46.31	48.51	-02	-08	48.41	54.30	-00	-10	54.20	56.72		-11	56.61		68
Aug. 7	46.53	-05	-18	46.30	48.90	-02	-21	48.67	54.52	-00	-32	54.20	56.38		-47	55.91		85
14	46.62	-05	-14	46.43	49.35	-02	-16	49.17	54.39	-00	-24	54.15	56.89		-32	56.57		78
21	46.69	-06	-11	46.52	49.70	-02	-12	49.56	54.60	-01	-15	54.44	57.83		-18	57.65		67
28	46.82	-06	-10	46.66	50.02	-02	-11	49.89	54.83	-00	-14	54.69	56.34		-19	56.15		67
Sept. 4	46.93	-05	-07	46.81	50.30	-01	-08	50.21	54.30	-00	-09	54.21	54.40		-13	54.27		60
11	47.03	-05	-05	46.93	50.46	-01	-04	50.41	53.54	-00	-03	53.51	53.62		-03	53.59		59
18	47.21	-05	-10	47.06	50.62	-01	-11	50.50	53.23	-00	-15	53.08	52.68		-22	52.46		60
25	47.34	-05	-05	47.24	50.58	-01	-04	50.53	52.74	-00	-03	52.71	53.10		-05	53.05		59
Oct. 2	47.46	-01	-08	47.34	50.60	-00	-09	50.51	52.50	-00	-11	52.39	52.00		-16	51.84		50
9	47.60	-01	-05	47.51	50.55	-00	-05	50.50	52.26	-00	-06	52.20	52.93		-08	52.85		50
16	47.68	-04	-05	47.59	50.53	-00	-04	50.49	52.12	-00	-05	52.07	51.61		-08	51.53		50
23	47.75	-04	+02	47.73	50.42	-00	+04	50.46	51.50	-00	+08	51.58	50.79		+10	50.89		49
30	47.81	-03	+04	47.82	50.35	-00	+07	50.42	50.83	-00	+10	50.93	47.98		+10	48.08		49
Nov. 6	47.96	-01	-03	47.92	50.27	+01	-02	50.26	49.30	+01	-04	49.27	44.81		-09	44.72		50
13	47.98	-01	+04	48.01	50.02	+01	+06	50.09	48.29	-00	+06	49.35	45.95		+06	46.01		49
20	48.04	-00	+04	48.08	49.65	+01	+06	49.72	47.34	+01	+05	47.40	43.05		+02	43.07		49
27	48.09	-00	+04	48.13	49.30	+01	+06	49.37	46.41	+01	+04	46.46	43.08		+03	43.11		49
Dec. 4	48.15	+03	+01	48.19	48.92	+01	+02	48.95	45.60	+01	-01	45.60	42.38		-04	42.34		49
11	48.15	+03	+07	48.25	48.40	+02	+07	48.49	44.70	+01	+06	44.77	41.00		+04	41.04		49
18	48.23	+03	-02	48.24	48.13	+02	-02	48.13	44.20	+01	-05	44.16	41.20		-10	41.10		50
25	48.22	+03	-01	48.24	47.71	+01	-01	47.71	44.01	-00	-04	43.97	41.48		-08	41.40		50
1838.																		
Jan. 1	48.18	+03	+03	48.24	47.35	-01	+03	47.39	44.20	-00	+01	44.21	43.34		+01	43.35		49
8	48.10	+03	+09	48.22	47.06	+01	+08	47.15	44.06	-00	+07	44.13	42.03		+09	42.12		49
15	48.00	+01	+09	48.13	46.80	+01	+07	46.88	43.49	+01	+06	43.56	39.62		+07	39.69		49
22	48.03	+04	+06	48.13	46.55	+02	+05	46.62	42.58	+01	+03	42.62	38.42		+01	38.43		49
29	47.90	+05	+08	48.03	46.20	+02	+05	46.27	41.70	+01	+04	41.75	37.60		+03	37.63		49
Feb. 5	47.89	+05	+07	48.01	45.80	+02	+05	45.87	41.00	+01	+03	41.04	37.10		+01	37.11		49
12	47.85	+05	+02	47.92	45.58	+02	+01	45.61	40.52	+01	-01	40.52	36.50		-04	36.46		49
19	47.70	+07	+07	47.84	45.05	+02	+04	45.11	39.90	+01	+01	39.92	36.00		-00	36.00		49
26	47.65	+07	+09	47.81	44.65	+02	+05	44.72	39.40	+01	+02	39.43	35.50		+02	35.52		49
Mar. 5	47.65	+07	-07	47.65	44.15	+02	-06	44.11	39.02	+01	-07	38.96	35.38		-14	35.24		49
12	47.55	+08	-03	47.60	44.01	+02	-03	44.00	38.62	+01	-04	38.59	35.06		-10	34.96		49
19	47.42	+07	+01	47.50	43.59	+02	-01	43.60	38.60	-00	-02	38.58	37.20		-07	37.13		49
26	47.30	+07	+01	47.38	43.35	+02	-01	43.36	38.90	-00	-02	38.88	37.10		-05	37.35		49
Apr. 2	47.17	+07	+01	47.25	43.05	+01	-01	43.05	39.30	-00	-02	39.28	39.49		-05	39.44		49
9	47.05	+07	-01	47.11	42.85	+01	-01	42.85	39.91	-00	-03	39.88	40.08		-07	40.01		49
16	46.93	+06	+01	47.00	42.86	+01	-01	42.86	40.58	-00	-02	40.56	41.05		-04	41.01		49
23	46.80	+05	+01	46.86	42.91	+01	-00	42.92	40.90	-00	-01	40.89	40.63		-03	40.60		49
30	46.65	+05	+01	46.71	42.83	-00	-00	42.83	41.30	-00	-01	41.29	41.52		-03	41.49	41.2	49
May 7	46.66	+01	-12	46.58	43.05	-00	-08	42.97	41.92	-00	-15	41.77	43.51		-28	43.23	31.42	49
14	46.50	+01	-01	46.53	43.12	-00	-02	43.10	42.99	-00	-02	42.97	45.70		-02	45.68	42.2	49
21	46.41	+03	-01	46.40	43.32	-00	-03	43.29	43.70	-00	-06	43.64	45.23		-11	45.12	46.9	49
28	46.30	+03	-05	46.28	43.62	-00	-04	43.58	44.50	-00	-07	44.43	46.42		-14	46.28	50.1	49
June 4	46.30	+01	-08	46.23	44.00	-01	-07	43.92	45.32	-00	-13	45.19	47.80		-22	47.58	56.2	49
11	46.20	+01	-05	46.16	44.25	-01	-05	44.19	46.10	-01	-08	46.01	49.10		-12	48.98	51.8	49
18	46.15	-00	-03	46.12	44.64	-01	-03	44.60	46.90	-01	-04	46.85	50.20		-04	50.16	52.2	49
25	46.20	-01	-11	46.08	45.08	-01	-10	44.99	48.03	-01	-18	47.84	51.71		-28	51.43	58.1	49
July 2	46.12	-01	-04	46.07	45.50	-02	-03	45.45	48.98	-01	-04	48.93	52.81		-02	52.79	52.1	49
9	46.15	-03	-07	46.05	46.00	-02	-07	45.91	50.04	-01	-11	49.92	54.51		-13	54.38	59.1	49
16	46.16	-01	-09	46.03	46.43	-02	-09	46.32	51.08	-01	-16	50.91	55.50		-20	55.30	60.4	49
23	46.26	-01	-11	46.11	47.00	-02	-11	46.87	51.63	-01	-18	51.44	55.00		-25	54.75	63.0	49
30	46.30	-01	-11	46.15	47.50	-02	-12	47.36	52.10	-00	-18	51.92	54.83		-25	54.58	56.5	49
Aug. 6	46.30	-05	-09	46.16	47.90	-02	-10	47.78	52.50	-01	-14	52.35	55.90		-17	55.73	59.1	49
13	46.42	-05	-09	46.28	48.37	-02	-11	48.24	52.91	-00	-14	52.77	55.82		-17	55.65	56.1	49

ites.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
38.																		
A. 20	46.49	-05	-06	46.38	48.75	-02	-07	48.66	53.12	00	-08	53.04	55.51	00	-08	55.43	56.1	60
27	46.61	-05	-14	46.42	49.12	-02	-16	48.94	53.29	00	-24	53.05	54.82		-33	54.49	59.2	75
St. 3	46.66	-05	-09	46.52	49.35	-01	-11	49.23	53.11	00	-14	52.97	54.63		-18	54.45	54.9	66
10	46.80	-05	-09	46.66	49.51	-01	-10	49.40	52.80	00	-13	52.67	52.50		-19	52.31	53.2	65
17	46.86	-05	-07	46.74	49.72	-01	-08	49.63	52.40	00	-10	52.30	53.51		-14	53.37	54.1	62
24	47.01	-04	-10	46.87	49.85	-01	-11	49.73	52.48	00	-16	52.32	52.71		-22	52.49	50.6	67
O. 1	47.15	-04	-11	47.00	49.95	-01	-13	49.81	52.21	00	-18	52.03	52.60		-25	52.35	53.1	69
8	47.25	-04	-10	47.11	49.97	00	-11	49.86	51.82	00	-16	51.66	50.70		-23	49.47	50.2?	67
15	47.30	-03	-08	47.19	49.90	00	-09	49.81	50.99	00	-12	50.87	48.81		-19	48.62	48.2	63
22	47.42	-03	-07	47.32	49.89	00	-07	49.82	50.22	00	-10	50.12	48.68		-16	48.52	52.8	61
29	47.48	-03	-01	47.44	49.69	00	+01	49.70	49.82	00	+01	49.83	48.89		00	48.89	42.0	49
N. 5	47.59	-01	+04	47.62	49.52	00	+06	49.58	49.10	+01	+08	49.19	45.78		+07	45.85	40.2	40
12	47.61	-01	+02	47.62	49.45	+01	+03	49.49	48.03	+01	+02	48.06	45.00		00	45.00	33.4	45
19	47.67	00	+02	47.69	49.10	+01	+03	49.14	47.01	+01	+02	47.04	43.04		-01	43.03	35.0	44
26	47.70	+01	+05	47.70	48.72	+01	+06	48.79	45.82	+01	+05	45.88	42.03		+03	42.06	33.3	39
D. 3	47.71	+01	+03	47.69	48.44	+01	-04	48.41	45.10	+01	-06	45.05	42.61		-11	42.50	43.1	54
10	47.82	+01	00	47.83	48.01	+01	00	48.02	44.91	00	-02	44.89	42.05		-06	41.99	42.1	48
17	47.81	+03	+03	47.87	47.61	+01	+03	47.65	44.51	00	+01	44.52	41.70		00	41.70	36.2	42
24	47.80	+03	+03	47.86	47.30	+01	+02	47.33	44.91	+01	+01	44.93	40.53		-02	40.51	38.8	43
31	47.81	+04	+01	47.86	46.95	+01	+01	46.97	43.20	00	-02	43.18	40.50		-05	40.45	39.2	46
139.																		
J. 7	47.75	+04	+05	47.84	46.60	+01	+04	46.65	42.91	00	+02	42.93	40.01		+01	40.02	35.8	39
14	47.72	+04	+04	47.80	46.32	+02	+03	46.37	42.28	+01	+01	42.30	39.30		-01	39.29	37.0	40
21	47.70	+05	+06	47.81	46.02	+02	+04	46.08	41.82	+01	+02	41.85	38.03		00	38.03	33.0	38
28	47.70	+05	+07	47.82	45.61	+02	+05	45.68	41.20	+01	+03	41.24	37.90		+02	37.92	30.4	35
E. 4	47.53	+05	+06	47.64	45.30	+02	+04	45.36	40.60	+01	+02	40.63	37.01		00	37.01	30.4	37
11	47.51	+04	+01	47.56	45.01	+01	00	45.02	44.32?	+01	-01	44.32	38.53		-04	38.49	40.2	45
18	47.42	+05	+04	47.51	44.60	+01	+02	44.63	40.60	00	00	40.60	38.52		-01	38.51	31.8	40
25	47.38	+05	+02	47.45	44.35	+02	00	44.37	40.30	00	-01	40.29	37.51		-03	37.48	32.2	43
M. 4	47.30	+06	+03	47.39	44.21	+02	+01	44.24	40.01	00	-01	40.00	38.30		-03	38.27	36.2	42
11	47.32	+06	+02	47.40	43.85	+02	00	43.87	40.04	00	-01	40.03	37.40		-04	37.36	31.9	43
18	47.05	+06	+01	47.12	43.70	+01	00	43.71	39.71	00	-02	39.69	38.15		-05	38.10	35.1	45
25	47.01	+06	00	47.07	43.50	+01	-01	43.50	39.99	00	-02	39.97	38.80		-05	38.75	37.0	46
A. 1	46.85	+06	+03	46.94	43.32	+01	+01	43.34	40.05	00	-00	40.05	38.99		-01	38.98	36.0	40
8	46.81	+06	-04	46.83	43.25	+01	-03	43.23	40.12	00	-06	40.06	38.41		-13	38.28	38.2	55
15	46.75	+06	-04	46.77	43.25	+01	-03	43.23	40.20	00	-06	40.12	40.30		-13	40.17	43.4	55
22	46.60	+05	-03	46.62	43.05	+01	-03	43.03	40.97	00	-05	40.92	41.51		-10	41.41	44.8	53
29	46.51	+05	-04	46.52	43.20	+01	-03	43.18	41.01	00	-06	40.95	41.72		-12	41.60	43.5?	55
M. 6	46.30	+03	-02	46.31	43.20	00	-02	43.18	42.90	00	-04	42.86	45.30		-06	45.24	45.8	50
13	46.39	+03	-01	46.41	43.41	00	-01	43.40	43.70	00	-02	43.68	45.60		-02	45.58	45.2	48
20	46.30	+02	-05	46.27	43.70	00	-04	43.66	44.30	00	-08	44.22	46.00		-13	45.87	50.1	57
27	46.40	+02	-11	46.31	44.00	-01	-09	43.90	44.90	00	-15	44.75	47.30		-28	47.02	54.0	70
J. 3	46.10	+01	-05	46.06	44.40	-01	-05	44.34	45.96	-01	-07	45.88	49.40		-11	49.29	50.1	57
10	46.02	00	-08	45.94	44.60	-01	-07	44.52	47.26	-01	-14	47.11	51.60		-19	51.41	57.2	65
17	46.03	-01	-14	45.88	44.98	-02	-13	44.83	48.20	-01	-25	47.94	52.50		-40	52.10	63.0	78
24	46.10	-02	-09	45.99	45.40	-02	-08	45.30	49.51	-01	-14	49.36	54.60		-18	54.42	56.0	66
J. 1	46.20	-03	-13	46.04	46.00	-02	-13	45.85	50.50	-01	-24	50.25	53.60		-33	53.27	56.1	75
8	46.20	-03	-13	46.04	46.50	-02	-14	46.34	51.20	-01	-24	50.95	56.10		-33	55.77	61.0	75
15	46.20	-04	-10	46.06	47.01	-02	-11	46.88	52.10	-01	-17	51.92	56.10		-21	55.89	58.8	68
22	46.30	-04	-09	46.17	47.50	-02	-09	47.39	52.60	-01	-13	52.46	55.61		-16	55.45	59.9	65
29																		
A. 5	46.40	-04	-13	46.23	48.49	-02	-15	48.32	53.52	-01	-22	53.29	57.10		-30	56.80	60.4	74
12	46.41	-06	-09	46.26	48.85	-02	-10	48.73	53.73	00	-12	53.61	56.70		-14	56.56	57.3	65
19	46.49	-06	-02	46.41	49.28	-02	-01	49.25	53.80	00	+03	53.83	55.00		+07	55.07	51.0	51
25	46.55	-06	-08	46.41	49.53	-01	-09	49.43	53.52	00	-11	53.41	54.81		-15	54.66	56.2	64
Se. 2	46.73	-06	-07	46.60	49.90	-01	-08	49.81	53.30	00	-10	53.20	54.30		-13	54.17	52.2	62
9	46.85	-05	-10	46.70	49.96	-01	-12	49.83	53.30	00	-16	53.14	54.38		-23	54.15	57.4	68
16	46.92	-05	-06	46.81	50.05	-01	-07	49.97	53.02	00	-08	52.94	53.20		-11	53.09	54.4	60
23	47.08	-05	-10	46.93	50.18	-01	-11	50.06	52.70	00	-15	52.55	52.60		-21	52.39	52.8	67
30	47.17	-04	-09	47.04	50.22	-01	-10	50.11	52.28	00	-14	52.14	52.12		-20	51.92	50.0	65
O. 7	47.26	-04	-06	47.16	50.19	00	-06	50.13	51.68	00	-07	51.61	49.82		-12	49.70	47.4	59
14	47.30	-04	-04	47.22	50.20	00	-03	50.17	50.84	00	-04	50.80	50.01		-07	49.94	49.0	55
21	47.42	-03	-07	47.32	50.01	00	-07	49.94	50.61	00	-09	50.52	48.82		-14	48.68	43.0	60

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5
1842.																	
Mar. 14	47.10	+05	-02	47.13	43.80	+01	-03	43.78	40.80	-00	-04	40.76	39.70	-00	-10	39.60	41.25
22	47.00	+05	-03	47.02	43.70	+01	-03	43.68	41.30	-00	-05	41.25	40.60		-11	40.49	36.25
28	46.90	+05	-02	46.93	43.75	+01	-02	43.74	41.42	-00	-04	41.38	40.39		-09	40.30	41.50
Apr. 4	46.80	+05	-06	46.79	43.63	+01	-05	43.59	41.60	-00	-08	41.52	41.50		-16	41.34	40.75
11	46.71	+04	-04	46.71	43.62	-00	-03	43.60	42.00	-00	-06	41.94	42.50		-11	42.39	45.50
18	46.70	+04	-07	46.67	43.70	-00	-05	43.65	42.62	-00	-09	42.53	43.50		-17	43.33	44.75
25	46.55	+03	-06	46.52	43.80	-00	-04	43.76	43.50	-00	-08	43.42	45.70		-16	45.54	50.00
May 2	46.50	+02	-08	46.44	44.10	-00	-06	44.04	44.70	-00	-11	44.59	47.50		-18	47.32	31.50
9	46.41	+01	-07	46.35	44.20	-01	-05	44.14	45.50	-00	-09	45.41	48.00		-15	47.85	47.00
16	46.35	+01	-10	46.26	44.60	-01	-07	44.52	46.14	-00	-14	46.00	48.50		-22	47.28	53.25
23	46.25	-00	-08	46.18	45.00	-01	-06	44.93	47.00	-00	-11	46.89	49.30		-17	49.13	51.75
30	46.24	-00	-09	46.15	45.34	-01	-07	45.26	47.50	-00	-12	47.38	50.10		-19	49.91	55.00
June 6	46.30	-01	-13	46.16	45.60	-01	-10	45.49	48.30	-01	-20	48.09	52.30		-30	52.00	59.25
13	46.30	-02	-17	46.11	46.10	-02	-17	45.91	49.65	-01	-29	49.35	55.24		-46	54.78	63.00
20	46.20	-03	-08	46.09	46.40	-02	-08	46.30	51.10	-01	-14	50.95	56.50		-14	56.36	57.25
27	46.30	-04	-09	46.17	47.10	-02	-10	46.98	52.20	-01	-15	52.04	55.60		-18	55.42	57.25
July 4	46.30	-04	-06	46.20	47.50	-02	-06	47.42	52.34	-00	-08	52.26	55.10		-08	55.02	55.75
11	46.30	-06	-06	46.18	48.10	-02	-07	48.01	52.50	-00	-08	52.42	54.60		-08	54.52	54.25
18	46.40	-05	-09	46.26	48.45	-02	-09	48.34	52.70	-00	-13	52.57	55.80		-16	55.61	59.50
25																	
Aug. 1	46.70	-06	-16	46.58	49.20	-02	-20	48.98	54.30	-01	-31	53.98	58.40		-42	57.98	64.25
8	46.50	-07	-08	46.35	49.50	-02	-10	49.38	54.90	-01	-12	54.77	58.60		-11	58.49	61.25
15	46.80	-07	-10	46.63	50.00	-02	-14	49.94	55.35	-00	-19	55.16	58.60		-22	58.38	64.25
22																	
29	47.10	-07	-14	46.99	50.80	-02	-16	50.62	55.80	-00	-24	55.56	58.10		-34	57.76	60.00
Sept. 5	41.10	-07	-04	46.99	50.90	-02	-03	50.85	55.70	-00	-00	55.70	57.60		+04	57.64	54.75
12	47.30	-07	-14	47.09	51.26	-01	-17	51.08	55.70	-00	-23	55.47	56.72		-31	56.41	57.80
19	47.40	-07	-02	47.31	51.30	-01	-00	51.29	55.30	-00	+03	55.27	56.70		+09	56.79	51.25
26	47.50	-06	-04	47.40	51.50	-01	-03	51.46	55.10	-00	-01	55.09	55.00		-02	54.98	50.25
Oct. 3	47.60	-06	-03	47.51	51.55	-01	-02	51.52	54.30	-00	-01	54.29	53.10		-03	53.07	49.25
10	47.80	-05	-05	47.70	51.65	-00	-04	51.61	53.51	-00	-04	53.47	52.10		-09	52.01	50.00
17	47.87	-04	-04	47.79	51.55	-00	-02	51.53	52.90	-00	-03	52.87	51.70		-04	51.66	50.02
24	47.96	-04	+01	47.93	51.40	-00	+03	51.37	52.10	-00	+06	52.16	48.90		+04	48.94	40.25
31	48.10	-02	+01	48.09	51.22	+01	+05	51.16	50.80	+01	+06	50.87	46.50		+01	46.51	42.25
Nov. 7	48.12	-01	+03	48.10	50.95	+01	+06	50.88	49.80	-00	+06	49.86	47.00		+05	47.05	40.50
14	48.25	-01	+01	48.25	50.65	+01	+04	50.60	49.10	-00	+03	49.13	46.10		+01	46.11	39.25
21	48.30	-01	+04	48.33	50.28	+01	+07	50.20	48.20	+01	+06	48.27	44.31		+04	44.35	34.75

CRAIGLEITH.

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5
1837.																	
Feb. 4	46.54	+03	+02	46.59	43.54	-00	-00	43.54	41.02	-00	-00	41.02	39.15	-00	-01	39.14	
12	46.30	+02	+02	46.34	42.90	-00	+01	42.91	41.42	-00	-00	41.42	40.69		-00	40.69	
21	46.02	+02	+01	46.05	42.88	-00	-00	42.88	41.52	-00	-00	41.52	40.18		-01	40.17	
27	45.87	+03	+01	45.91	42.76	-00	-00	42.76	40.47	-01	-01	40.46	38.21		-02	38.19	
Mar. 6	45.69	+02	+01	45.72	42.70	-00	-00	42.70	40.77	-01	-01	40.76	39.50		-02	39.48	
13	45.57	+02	-00	45.59	42.63	-00	-00	42.63	40.93	-01	-01	40.92	39.10		-03	39.07	
20	45.38	+03	+01	45.42	42.15	-00	-00	42.15	39.70	-00	-00	39.70	38.00		-02	37.98	
27	45.22	+03	+02	45.27	41.98	-00	-00	41.98	39.71	-00	-00	39.71	37.70		-00	37.70	
Apr. 4	45.02	+03	+02	45.07	41.84	-00	-00	41.84	39.52	-00	-00	39.52	37.86		-00	37.86	
10	44.92	+03	-01	44.94	41.74	-00	-01	41.73	39.77	-02	-02	39.75	38.73		-04	38.69	
17	44.71	+02	+01	44.74	41.73	-00	-00	41.73	40.42	-01	-01	40.41	39.68		-01	39.67	
24	44.60	+02	-01	44.61	41.68	-00	-01	41.67	41.38	-03	-03	41.35	41.25		-04	41.21	
May 1	44.50	+01	-03	44.48	42.10	-00	-02	42.08	42.52	-05	-05	42.47	43.15		-09	43.06	
8	44.37	+01	-00	44.38	42.62	-00	-00	42.62	43.60	-00	-00	43.60	44.45		-01	44.44	

Des.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
187.																		
Ma 15	44.36	+01	-03	44.34	43.10	-00	-02	43.08	44.00	-00	-06	43.94	44.39	-00	-09	44.30	°	57
22	44.38	-00	-03	44.35	43.57	-00	-02	43.55	45.28		-07	45.21	46.58		-09	46.49		58
30	44.44	-01	-04	44.39	44.19	-00	-04	44.15	46.18		-10	46.08	47.68		-14	47.54		64
Jul 5	44.53	-01	-04	44.48	44.66	-00	-04	44.62	47.10		-11	46.99	49.05		-15	48.90		64
12	44.67	-01	-06	44.60	45.28	-00	-07	45.21	47.87		-14	47.73	49.68		-19	49.49		69
19	44.88	-02	-07	44.79	46.25	-01	-08	46.16	49.90		-16	49.74	52.40		-21	52.19		70
27	45.04	-02	-06	44.96	46.84	-01	-07	46.76	51.00		-12	50.88	54.00		-14	53.86		65
Jul 3	45.22	-03	-07	45.12	47.45	-01	-10	47.34	51.50		-18	51.32	53.98		-23	53.75		72
10	45.50	-03	-08	45.39	48.12	-01	-12	47.99	52.10		-20	51.90	54.60		-25	54.35		74
17	45.77	-03	-08	45.66	48.82	-01	-10	48.71	52.81		-15	52.66	Above Scale.					69
24	46.10	-03	-08	45.99	49.68	-01	-12	49.55	Above Scale.									71
31	46.38	-03	-05	46.30	50.17	-01	-06	50.10										60
Aug 7	46.72	-03	-09	46.60	50.62	-01	-11	50.50										71
14	46.94	-03	-09	46.82	50.47	-01	-11	50.35										70
21	47.15	-03	-07	47.05	50.66	-00	-08	50.58										65
28	47.35	-03	-04	47.28	51.00	-00	-04	50.90					54.60		-04	54.56		58
Sept 4	47.58	-03	-04	47.51	51.00	-00	-05	50.95					53.40		-08	53.32		60
12	47.84	-02	-07	47.75	51.00	-00	-08	50.92	52.83		-11	52.72	53.50		-13	53.37		65
19	48.02	-02	-04	47.96	51.00	-00	-04	50.96	52.30		-05	52.25	52.45		-06	52.39		57
25	48.12	-02	-04	48.06	50.70	-00	-05	50.65	52.37		-07	52.30	52.62		-07	52.55		59
Oct 2	48.25	-02	-06	48.17	50.60	-00	-06	50.54	51.72		-09	51.63	51.46		-12	51.34		62
10	48.34	-01	-05	48.28	50.45	-00	-06	50.39	51.85		-07	51.78	52.10		-09	52.01		60
17	48.39	-01	-04	48.34	50.37	-00	-04	50.33	51.18		-06	51.12	50.69		-08	50.61		58
23	48.42	-01	-01	48.40	50.16	-00	-00	50.16	50.66		-00	50.66	49.95		-00	49.95		50
30	48.45	-01	-00	48.44	49.83	-00	+01	49.84	49.30		+01	49.31	47.08		-01	47.07		48
Nov 6	48.47	-00	+02	48.49	49.22	-00	+03	49.25	47.50		+03	47.53	46.70		+03	46.73		43
13	48.40	-00	+05	48.45	48.54	-00	+06	48.60	47.27		+07	47.34	45.35		+07	45.42		36
20	48.40	+01	+03	48.44	47.90	-00	+03	47.93	45.70		+03	45.73	43.40		+01	43.41		41
27	48.27	+01	+04	48.32	47.20	-00	+03	47.23	45.10		+03	45.13	42.95		+03	42.98		39
Dec 4	48.37	+01	+05	48.43	46.84	-00	+04	46.88	44.43		+03	44.46	42.55		+04	42.59		37
11	48.00	+02	+05	48.07	46.10	-00	+04	46.14	43.60		+04	43.64	41.20		+03	41.23		36
18	47.17	+01	-01	47.17	45.70	-00	-01	45.69	43.18		-03	43.15	41.70		-05	41.65		50
25	47.61	+03	-01	47.63	45.21	-00	-01	45.20	43.57		-03	43.54	42.90		-05	42.85		50
Jan 8.																		
15	47.33	+01	+02	47.36	45.24	-00	+01	45.25	44.17		+01	44.18	43.00		+01	43.01		41
22	47.14	+01	+05	47.20	45.15	-00	+03	45.18	43.60		+04	43.64	41.48		+04	41.52		34
29	46.92	+02	+07	47.01	44.87	-00	+05	44.92	42.34		+05	42.39	39.40		+04	39.44		29
Feb 5	46.77	+02	+04	46.83	44.35	+01	+01	44.37	41.15		+01	41.16	37.85		-00	37.85		38
12	46.65	+03	+04	46.72	43.42	+01	+01	43.44	40.00		+01	40.01	37.27		+01	37.28		35
19	46.45	+03	+04	46.52	42.85	+01	+01	42.87	39.62		+01	39.63	37.00		-00	37.00		37
26	46.20	+03	+06	46.29	42.32	+01	+01	42.34	39.00		+02	39.02	36.45		+02	36.47		30
Mar 5	45.90	+03	+02	45.95	42.14	+01	-00	42.15	38.50		-00	38.50	35.36		-01	35.35		39
12	45.68	+03	+05	45.76	41.42	+01	+01	41.44	37.75		+01	37.76	35.10		-01	35.09		31
19	45.45	+04	+02	45.51	40.80	+01	-00	40.81	37.45		-00	37.45	35.50		-01	35.49		39
26	45.20	+03	+01	45.24	40.70	-00	-00	40.70	38.08		-01	38.07	37.15		-02	37.13		43
Apr 2	44.92	+03	-01	44.94	40.07	-00	-00	40.07	39.20		-02	39.18	38.17		-03	38.14		47
9	44.65	+03	-00	44.68	40.85	-00	-00	40.85	39.37		-01	39.36	38.38		-03	38.35		45
16	44.40	+02	-01	44.41	41.20	-00	-01	41.19	40.50		-03	40.47	39.80		-05	39.75		50
23	44.30	+02	-00	44.32	41.34	-00	-00	41.34	41.02		-01	41.01	40.58		-02	40.56		45
30	44.20	+01	+01	44.22	41.63	-00	-00	41.63	41.68		+01	41.69	41.88		+01	41.89		40
May 6	44.07	+01	-00	44.08	41.80	-00	-00	41.80	41.38		-01	41.37	40.90		-02	40.88		44
13	44.02	+01	-00	44.03	42.05	-00	-00	42.05	42.10		-01	42.09	42.05		-02	42.03	39.0	45
20	44.04	+01	-06	43.99	42.38	-00	-04	42.34	43.35		-13	43.22	45.00?		-22	44.78	42.3	73
27	44.00	-00	-01	43.99	42.85	-00	-01	42.84	44.58		-01	44.57	45.62		-01	45.61	43.2	47
Jun 3	44.03	-00	-02	44.01	43.38	-00	-01	43.37	44.45		-01	44.41	45.20		-06	45.14	46.0	52
10	44.17	-00	-02	44.15	43.85	-00	-01	43.84	45.15		-04	45.11	46.58		-05	46.53	47.4	52
17	44.23	-00	-03	44.20	44.22	-00	-03	44.19	45.80		-06	45.74	47.25		-10	47.15	49.2	57
24	44.32	-01	-02	44.29	44.68	-00	-02	44.66	46.85		-04	46.81	48.73		-05	48.68	50.0	54
Jul 1	44.54	-01	-01	44.52	45.44	-00	-02	45.42	47.90		-03	47.87	50.10		-02	50.08	51.3	52
8	44.70	-01	-04	44.65	46.03	-00	-06	45.97	48.80		-10	48.70	51.10		-15	50.95	54.2	63
15	44.88	-02	-02	44.84	46.65	-01	-02	46.62	49.70		-03	49.67	51.77		-02	51.75	52.2	54
22	45.08	-02	-07	44.99	47.15	-01	-09	47.05	50.40		-17	50.23	53.08		-23	52.85	57.2	70
29	45.27	-03	-05	45.19	47.65	-01	-05	47.59	51.20		-10	51.10	53.75		-11	53.64	56.4	62
Aug 5	45.47	-03	-03	45.41	48.14	-01	-04	48.09	51.33		-05	51.28	53.17		-05	53.12	54.4	57

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
1838.																		
July 30	45.74	-.03	-.05	45.66	48.54	-.01	-.07	48.46	51.32	-.00	-.10	51.22	53.02	-.00	-.13	52.89	56.1	63
Aug. 6	46.00	-.03	-.06	45.91	48.82	-.01	-.08	48.73	51.74		-.12	51.62	54.00		-.15	53.85	56.0	65
13	46.25	-.03	-.06	46.16	49.17	-.01	-.07	49.09	52.15		-.10	52.05	54.22		-.13	54.09	55.2	63
20	46.45	-.03	-.05	46.37	49.45	.00	-.06	49.39	52.05		-.07	51.98	53.62		-.08	53.54	55.2	60
27	46.73	-.03	-.08	46.62	49.68	.00	-.10	49.58	52.07		-.15	51.92	53.52		-.20	53.32	56.3	69
Sept. 3	46.90	-.03	-.06	46.81	49.80	.00	-.08	49.72	52.08		-.11	51.97	53.25		-.15	53.10	53.2	61
10	47.26	-.02	-.04	47.20	50.19	.00	-.04	50.15	51.75		-.04	51.71	52.00		-.06	51.94	48.4	57
17	47.28	-.02	-.04	47.22	49.85	.00	-.04	49.81	51.45		-.05	51.40	52.80		-.05	52.75	53.2	57
24	47.43	-.02	-.05	47.36	49.89	.00	-.05	49.84	51.35		-.08	51.27	51.88		-.10	51.78	51.1	60
Oct. 1	47.55	-.02	-.04	47.49	49.84	.00	-.04	49.80	51.10		-.06	51.04	51.90		-.07	51.83	52.2	58
8	47.62	-.01	.00	47.61	49.72	.00	+.01	49.73	50.20		+.01	50.21	49.68		+.01	49.69	47.1	49
15	47.72	-.01	-.02	47.69	49.45	.00	-.03	49.42	49.25		-.04	49.21	47.80		-.06	47.74	46.0	54
22	47.80	-.01	-.04	47.75	49.07	.00	-.04	49.03	48.60		-.06	48.54	48.16		-.09	48.07	49.2	57
29	47.78	-.01	+.01	47.78	48.76	.00	+.01	48.77	48.64		+.02	48.66	47.90		+.02	47.92	44.0	46
Nov. 5	47.85	.00	.00	47.85	48.48	.00	.00	48.48	47.20		.00	47.20	45.68		-.01	45.67	43.3	47
12	47.82	+.01	+.05	47.88	47.92	.00	+.05	47.97	46.32		+.06	46.38	44.70		+.08	44.78	37.3	36
19	47.74	+.01	+.04	47.79	47.43	.00	+.03	47.46	45.40		+.03	45.43	43.08		+.02	43.10	37.3	40
26	47.62	+.01	+.01	47.64	46.50	+.01	+.01	46.52	43.72		.00	43.72	41.42		-.03	41.39	40.2	45
Dec. 3	47.57	+.01	.00	47.58	46.12	.00	-.01	46.11	43.78		-.02	43.76	43.02		-.04	42.98	42.2	48
10	47.35	+.01	.00	47.36	45.85	.00	-.01	45.84	43.83		-.02	43.81	42.40		-.04	42.36	41.3	48
17	47.14	+.01	+.03	47.18	45.55	.00	+.02	45.57	43.40		+.01	43.41	41.68		+.01	51.69	38.3	46
27	46.95	+.02	+.03	47.00	44.85	.00	+.02	44.87	42.38		+.01	42.39	40.50		+.01	40.51	36.2	38
31	46.81	+.02	+.03	46.89	44.64	.00	+.01	44.65	22.12		+.01	42.13	40.55		+.01	40.86	36.3	36
1839.																		
Jan. 7	46.68	+.02	+.02	46.72	44.21	.00	+.01	44.22	41.77		+.01	41.78	40.00		.00	40.00	36.2	40
14	46.48	+.02	+.02	46.52	43.70	.00	+.01	43.71	40.95		.00	40.95	39.90		.00	39.90	37.4	40
21	46.28	+.03	+.02	46.33	43.40	+.01	+.01	43.42	40.25		.00	40.25	38.25		.00	38.25	34.3	36
28	46.06	+.03	+.03	46.12	43.07	+.01	+.01	43.09	40.04		+.01	40.05	38.02		+.01	38.03	33.0	36
Feb. 4	45.85	+.03	+.01	45.89	42.58	+.01	.00	42.59	39.28		.00	39.28	37.24		-.02	37.22	33.1	41
11	45.67	+.03	.00	45.70	42.12	.00	-.01	42.11	39.70		-.02	39.68	39.12		-.04	39.08	39.0	46
18	45.43	+.03	+.03	45.49	42.07	.00	+.01	42.08	40.10		+.01	40.11	38.90		+.02	38.92	31.0	38
25	45.23	+.03	+.01	45.26	41.95	.00	.00	41.95	39.33		-.01	39.32	37.98		-.02	37.96	35.0	42
Mar. 4	45.00	+.03	+.01	45.04	41.84	.00	.00	41.84	39.67		-.01	39.66	38.78		-.02	38.76	37.1	42
11	44.80	+.03	+.01	44.84	41.75	.00	.00	41.75	39.30		-.01	39.29	37.42		-.02	37.40	34.3	42
19	44.70	+.03	-.01	44.72	41.60	.00	.00	41.60	39.45		-.02	39.43	38.57		-.04	38.53	36.2	47
25	44.55	+.02	+.01	44.58	41.37	.00	.00	41.37	39.75		.00	39.75	39.43		-.02	39.41	36.4	42
Apr. 1	44.37	+.02	+.01	44.40	41.45	.00	.00	41.45	40.08		.00	40.08	39.22		-.01	39.21	36.3	40
8	44.22	+.02	-.01	44.23	41.55	.00	.00	41.55	39.68		-.02	39.66	38.65		-.05	38.60	47.2	49
15	44.10	+.02	-.01	44.11	41.45	.00	.00	41.45	40.26		-.02	40.24	40.98		-.05	40.93	41.3	48
22	44.00	+.01	-.02	43.99	41.68	.00	-.01	41.67	41.21		-.04	41.17	41.70		-.08	41.62	43.4	51
29	43.92	+.01	-.03	43.90	41.98	.00	-.02	41.96	42.32		-.06	42.26	43.41		-.11	43.30	46.2	55
May 6	43.90	.00	-.03	43.87	42.74	.00	-.02	42.72	43.40		-.06	43.34	43.80		-.10	43.70	45.2	50
13	43.88	.00	-.01	43.87	43.10	.00	-.01	43.09	43.90		-.03	43.87	44.06		-.13	43.93	44.0	50
21	43.93	.00	-.03	43.90	43.34	.00	-.03	43.31	44.15		-.07	44.08	45.50		-.12	45.38	47.3	58
27	43.98	.00	-.04	43.94	43.62	.00	-.04	43.58	44.80		-.09	44.71	46.10		-.16	45.94	48.1	61
June 3	44.07	-.01	-.02	44.04	44.04	.00	-.02	44.02	45.77		-.06	45.71	47.17		-.08	47.09	46.4	58
10	44.17	-.01	-.03	44.13	44.52	.00	-.03	44.49	46.58		-.07	46.51	48.60		-.10	48.50	49.4	59
17	44.31	-.01	-.06	44.24	45.05	.00	-.07	44.98	47.38		-.15	47.23	49.82		-.23	49.59	55.3	70
24	44.53	-.01	-.05	44.47	45.84	-.01	-.07	45.76	49.04		-.13	48.91	51.35		-.18	51.17	54.2	66
July 1	44.68	-.02	-.06	44.60	46.40	-.01	-.08	46.31	49.60		-.15	49.45	51.75		-.21	51.54	54.0	66
7	44.82	-.02	-.06	44.74	46.85	-.01	-.08	46.76	50.49		-.15	50.34	53.50		-.19	53.31	57.0	66
15	45.20	-.03	-.04	45.13	47.87	-.01	-.05	47.81	51.42		-.08	51.34	53.72		-.08	53.64	51.3	66
22	45.33	-.03	-.05	45.25	48.10	-.01	-.07	48.02	51.49		-.11	51.38	53.84		-.12	53.72	56.4	66
29	45.58	-.03	-.06	45.49	48.57	-.01	-.09	48.47	52.10		-.14	51.96	54.68		-.17	54.51	57.0	66
Aug. 5	45.87	-.03	-.08	45.76	49.04	-.01	-.11	48.92	52.47		-.18	52.29	54.70		-.23	54.47	56.4	71
11	46.17	-.03	-.05	46.09	49.44	.00	-.06	49.38	52.50		-.08	52.42	54.15		-.08	54.07	53.3	66
19	46.35	-.03	-.03	46.29	49.58	.00	-.03	49.55	52.10		-.01	52.06	53.27		-.04	53.23	52.3	55
26	46.62	-.03	-.06	46.53	49.68	.00	-.07	49.61	51.92		-.10	51.82	53.43		-.13	53.30	54.4	66
Sept. 3	46.85	-.02	-.05	46.78	49.78	.00	-.06	49.72	52.03		-.08	51.95	53.27		-.11	53.16	53.3	66
10	47.07	-.02	-.06	46.99	49.90	.00	-.07	49.83	51.95		-.09	51.86	53.17		-.12	53.05	53.4	66
16	47.28	-.02	-.04	47.22	50.09	.00	-.04	50.05	51.67		-.04	51.63	52.30		-.06	52.24	52.1	55
23	47.37	-.02	-.06	47.29	49.94	.00	-.07	49.87	51.30		-.10	51.20	51.67		-.13	51.54	52.0	66
30	47.47	-.02	-.04	47.41	49.78	.00	-.04	49.74	50.88		-.06	50.82	51.17		-.09	51.08	50.0	55
Oct. 7	47.60	-.01	-.04	47.55	47.65	.00	-.03	47.62	50.05		-.06	49.99	49.17		-.09	49.08	48.2	55

ates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
39.																		
C. 14	47.66	-01	-02	47.63	49.34	-00	-02	49.32	49.62	-00	-02	49.60	49.64	-00	-03	49.61	49.0	53
21	47.62	-01	-03	47.58	49.15	-00	-03	49.12	49.12		-05	49.07	48.20		-08	48.12	46.2	56
28	47.80	-01	00	47.79	48.88	-00	00	48.88	48.77		00	48.77	48.37		-01	48.36	45.1	49
N. 4	47.77	-00	00	47.77	48.59	-00	+01	48.60	47.94		+01	47.95	46.98		-00	46.98	45.1	47
11	47.80	-00	00	47.80	48.27	-00	00	48.27	47.47		-01	47.46	46.98		-01	46.97	44.3	48
18	47.77	-00	00	47.77	47.97	-00	00	47.97	47.20		-01	47.19	46.65		-01	46.64	44.2	48
25	47.68	-00	+03	47.71	47.68	-00	+03	47.71	46.55		+04	46.59	45.30		+04	45.34	41.3	40
I. 2	47.60	+01	+06	47.67	47.20	-00	+05	47.25	45.16		+06	45.22	43.30		+07	43.37	36.2	34
9	47.47	+01	+05	74.53	46.66	-00	+05	46.71	44.20		+05	44.25	41.68		+06	41.74	35.2	34
16	47.43	+02	+03	47.48	45.94	-00	+02	45.96	43.48		+01	43.49	42.26		+01	42.27	39.3	40
23	47.28	+02	00	47.30	45.50	-00	00	45.50	43.40		-01	43.39	42.68		-02	42.66	42.1	46
30	47.03	+02	+06	47.11	45.25	-00	+04	45.29	43.00		+05	43.05	40.50		+06	40.56	33.1	32
40.																		
J. 6	46.90	+02	+05	46.97	44.63	-00	+03	44.66	42.20		+03	42.23	40.39		+04	40.43	34.2	34
13	46.69	+02	-01	46.70	44.27	-00	-01	44.26	41.63		-03	41.60	40.73		-06	40.67	41.0	50
20	46.53	+02	+02	46.57	43.96	-00	+01	43.97	41.88		+01	41.89	40.83		+01	40.84	37.1	40
27	46.32	+02	+04	46.38	43.73	-00	+02	43.75	41.60		+02	41.62	40.20		+03	40.23	34.4	35
Fl. 3	46.14	+03	+02	46.19	43.41	-00	+01	43.42	40.62		00	40.62	39.00		-01	38.99	36.3	40
10	45.97	+03	00	46.00	43.03	-00	00	43.03	40.62		-01	40.61	39.40		-03	39.37	38.0	45
17	45.73	+02	+01	45.76	42.94	-00	00	42.94	41.00		-01	40.99	40.64		-02	40.62	40.1	44
24	45.50	+02	+02	45.54	42.92	-00	+01	42.93	40.80		00	40.80	38.92		-01	38.91	34.4	40
N. 2	45.34	+03	+01	45.38	42.65	-00	00	42.65	39.92		00	39.92	37.82		-01	37.81	34.0	41
9	45.20	+03	-03	45.20	42.29	-00	-02	42.27	39.54		-04	39.50	38.00		-09	37.91	37.4	56
17	45.02	+02	-03	45.01	42.04	-00	-02	42.02	40.44		-04	40.40	40.46		-10	40.36	40.4	56
24	44.80	+02	00	44.82	42.14	-00	00	42.14	40.76		-01	40.75	39.97		-04	39.93	37.2	46
30	44.68	+02	00	44.70	42.16	-00	00	42.16	41.00		-02	40.98	41.28		-03	41.25	42.0	46
41	44.55	+01	-01	44.55	42.34	-00	-01	42.33	41.70		-02	41.68	41.95		-05	41.90	42.3	49
14	44.45	+01	-02	44.44	42.55	-00	-01	42.54	42.38		-04	42.34	43.10		-08	43.02	46.1	53
20	44.41	-00	-04	44.37	42.82	-00	-03	42.79	43.18		-07	43.11	43.88		-12	43.76	44.2	59
27	44.40	-00	-05	44.35	43.22	-00	-04	43.18	44.32		-11	44.21	46.22		-19	46.03	52.1	66
N. 4	44.38	-01	-02	44.35	43.76	-00	-02	43.74	45.56		-04	45.52	47.56		-06	47.50	48.4	54
11	44.40	-00	00	44.40	44.32	-00	00	44.32	45.38		+01	45.39	45.20		+01	45.21	41.3	44
19	44.60	-00	-02	44.58	44.60	-00	-02	44.58	45.38		-04	45.34	45.93		-07	45.86	44.0	53
25	44.64	-01	-03	44.60	44.70	-00	-03	44.67	45.89		-05	45.84	47.38		-08	47.30	48.3	55
J. e 1	44.72	-01	-05	44.66	45.05	-00	-05	45.00	46.65		-10	46.55	48.68		-15	48.53	53.3	63
8	44.84	-01	-05	44.78	45.57	-00	-06	45.51	47.66		-11	47.55	49.56		-17	49.39	53.1	65
15	44.94	-01	-06	44.87	45.98	-00	-06	45.92	48.48		-12	48.36	50.50		-18	50.32	52.4	65
22	45.18	-02	-06	45.10	46.49	-00	-07	46.42	49.03		-12	48.91	50.78		-17	50.61	53.2	65
29	45.26	-02	-06	45.18	46.94	-00	-08	46.86	49.42		-14	49.28	51.76		-19	51.57	55.1	67
J. y 6	45.50	-02	-05	45.43	47.55	-00	-05	47.50	50.26		-08	50.18	52.10		-10	52.00	54.4	60
13	45.63	-02	-05	45.56	47.84	-00	-06	47.78	50.70		-09	50.61	52.90		-12	52.78	53.0	62
20	45.88	-02	-07	45.79	48.30	-00	-10	48.20	51.32		-16	51.16	53.68		-21	53.45	56.2	69
27	46.05	-03	-07	45.95	48.68	-00	-08	48.60	51.70		-12	51.58	53.90		-15	53.75	56.0	65
A. 3	46.26	-03	-08	46.15	49.10	-00	-10	49.00	52.20		-17	52.03	54.39		-21	54.18	58.0	69
10	46.47	-03	-07	46.37	49.45	-01	-09	49.35	52.76		-12	52.64	55.58		-15	55.43	59.0	66
18	46.78	-03	-07	46.68	50.09	-00	-08	50.01	52.95		-11	52.84	54.28		-14	54.14	56.3	64
24	46.93	-03	-07	46.83	50.11	-00	-09	50.02	52.97		-13	52.84	54.81		-16	54.65	54.3	66
31	47.15	-03	-07	47.05	50.34	-00	-09	50.25	53.00		-12	52.88	54.80		-14	54.66	52.0	65
St. 8	47.35	-03	-06	47.26	50.46	-00	-07	50.39	52.70		-09	52.61	53.72		-11	53.61	50.1	62
14	47.50	-02	-05	47.43	50.44	-00	-05	50.39	52.16		-06	52.10	52.58		-07	52.51	46.3	58
21	47.68	-02	-03	47.63	50.34	-00	-03	50.31	52.30		-03	52.27	51.36		-05	51.31	47.0	55
28	47.83	-02	-07	47.74	50.14	-00	-08	50.06	50.79		-11	50.68	50.75		-16	50.59	47.0	64
O. 4	47.88	-01	-02	47.85	49.88	-00	-02	49.86	50.13		-02	50.11	49.56		-03	49.53	42.0	53
12	47.95	-01	-01	47.93	49.55	-00	00	49.55	49.42		-01	49.41	48.92		-01	48.91		50
19	48.00	-01	-02	47.97	49.23	-00	-02	49.21	49.08		-02	49.06	49.00		-03	48.97		52
26	47.98	-00	00	47.98	48.95	-00	00	48.95	48.46		00	48.46	46.98		-01	46.97		48
N. 2	48.02	-00	-02	48.00	48.55	-00	-01	48.54	47.50		-03	47.47	46.60		-05	46.55		52
9	47.92	-00	+01	47.93	48.18	-00	+01	48.19	47.08		+01	47.09	45.70		+01	45.71		45
16	47.94	+01	00	47.95	47.74	-00	00	47.74	46.10		-01	46.09	44.30		-03	44.27		48
24	47.82	+01	+02	47.85	47.14	-00	+01	47.15	44.78		+01	44.79	43.01		-01	43.00		44
30	47.70	+01	-02	47.69	46.84	-00	-02	46.82	44.68		-04	44.64	42.18		-07	42.11		52
D. 7	47.54	+01	+01	47.56	46.34	-00	00	46.34	44.30		00	44.30	43.25		-02	43.23		45
14	47.34	+01	+05	47.40	46.00	-00	+04	46.04	43.88		+04	43.92	41.88		+05	41.93		35
21	47.25	+02	+04	47.31	45.40	-00	+03	45.43	42.76		+03	42.79	40.90		+03	40.93		36

Dates.	t_1 uncor- rected.	Cor. for Col.	Cor. for Air.	t_1 cor- rected.	t_2 uncor- rected.	Cor. for Col.	Cor. for Air.	t_2 cor- rected.	t_3 uncor- rected.	Cor. for Col.	Cor. for Air.	t_3 cor- rected.	t_4 uncor- rected.	Cor. for Col.	Cor. for Air.	t_4 cor- rected.	t_5	T
1840.																		
Dec. 28	47.02	+02	+05	47.09	45.04	+01	+03	45.08	41.70	-00	+03	41.73	38.98	-00	+03	39.01		33
1841.																		
Jan. 4	46.87	+03	+05	46.95	44.30	-00	+02	44.32	41.15	-00	+02	41.17	39.40		+02	39.42		35
12	46.58	+03	+05	46.66	43.84	+01	+02	43.87	40.15	-00	+02	40.17	37.17		+01	37.18		31
18	46.44	+03	+05	46.52	43.12	+01	+02	43.15	39.38	-00	+02	39.40	36.78		+02	36.80		32
25	41.15	+03	+04	46.22	42.75	+01	+02	42.78	38.98	-00	+01	38.99	36.50		+01	36.51		33
Feb. 1	45.90	+03	+01	45.97	42.32	-00	+01	42.33	39.28	-00	+01	39.29	38.28		+02	38.30		31
8	45.60	+03	+05	45.68	42.13	-00	+01	42.14	39.13	-00	+02	39.15	37.03		+03	37.06		31
15	45.45	+03	+01	45.49	41.68	-00	-00	41.68	38.92	-00	-01	38.91	38.40		-03	38.37		13
22	45.22	+03	-01	45.21	41.64	-00	-01	41.63	39.92	-00	-02	39.90	39.45		-06	39.39		49
Mar. 2	44.93	+02	+01	44.96	41.93	-00	-00	41.93	40.48	-00	-01	40.47	39.60		-01	39.57		42
8	44.80	+02	-03	44.79	42.02	-00	-01	42.01	40.48	-00	-04	40.44	40.28		-09	40.19		55
15	44.66	+01	-02	44.65	42.14	-00	-01	42.13	41.74	-00	-04	41.70	42.58		-09	42.49		51
22	44.51	+01	-02	44.53	42.54	-00	-01	42.53	42.70	-00	-03	42.67	43.20		-06	43.14		51
29	44.48	+01	-03	44.46	42.94	-00	-02	42.92	43.31	-00	-06	43.25	43.97		-10	43.87		56
Apr. 5	44.45	+01	-01	44.45	43.25	-00	-01	43.24	43.38	-00	-02	43.36	43.48		-04	43.44		48
12	44.45	+01	-01	44.45	43.44	-00	-01	43.43	43.48	-00	-02	43.46	43.54		-03	43.51		48
19	44.50	+01	-02	44.49	43.62	-00	-02	43.60	43.85	-00	-04	43.81	44.20		-08	44.12		51
26	44.53	+01	-01	44.53	43.83	-00	-01	43.82	44.10	-00	-03	44.07	44.40		-06	44.34		52
May 3	44.56	-00	-01	44.55	44.08	-00	-01	44.07	45.26	-00	-03	45.23	46.78		-02	46.72		50
10	44.64	-00	-04	44.60	44.54	-00	-04	44.50	45.84	-00	-10	45.74	47.20		-17	47.03		61
17	44.70	-01	-03	44.66	45.02	-00	-04	44.98	47.09	-00	-08	47.01	48.95		-11	48.84		59
24	44.84	-01	-06	44.77	45.62	-00	-08	45.54	47.80	-00	-17	47.63	49.58		-26	49.32		73
31	44.97	-01	-06	44.90	46.14	-00	-07	46.07	48.29	-01	-13	48.15	51.42		-20	51.22		67
June 7	45.12	-02	-03	45.07	46.73	-00	-04	46.69	49.50	-00	-07	49.43	51.19		-08	51.11		58
14	45.30	-02	-04	45.24	47.17	-01	-05	47.11	50.05	-00	-09	49.96	52.48		-11	52.37		61
21	45.52	-02	-06	45.44	47.62	-00	-06	47.56	50.19	-00	-12	50.07	51.94		-16	51.78		65
28	45.72	-02	-04	45.66	47.90	-00	-06	47.84	50.28	-00	-09	50.19	52.00		-12	51.88		62
July 6	45.94	-02	-03	45.87	48.19	-00	-06	48.13	51.02	-00	-09	50.93	53.17		-12	53.05		62
12	46.10	-02	-06	46.02	48.50	-00	-07	48.43	50.97	-00	-11	50.86	52.46		-15	52.31		64
19	46.31	-02	-05	46.24	48.72	-00	-07	48.65	51.18	-00	-10	51.08	53.12		-13	52.99		63
27	46.50	-02	-05	46.43	48.95	-00	-05	48.90	51.40	-00	-08	51.32	53.48		-10	53.38		60
Aug. 2	46.66	-02	-08	46.56	49.18	-00	-10	49.08	51.39	-00	-17	51.22	52.64		-22	52.42		70
9	46.80	-02	-04	46.74	49.27	-00	-05	49.22	51.46	-00	-08	51.38	53.10		-09	53.01		60
16	47.00	-02	-06	46.92	49.45	-00	-07	49.38	51.30	-00	-10	51.20	52.43		-14	52.29		63
23	47.20	-02	-04	47.14	49.71	-00	-05	49.66	51.76	-00	-07	51.69	52.98		-09	52.89		60
30	47.27	-02	-06	47.19	49.68	-00	-08	49.60	51.86	-00	-12	51.74	53.58		-16	53.42		66
Sept. 6	47.36	-01	-06	47.29	49.83	-00	-02	49.81	51.60	-00	-02	51.58	51.98		-04	51.94		55
13	47.55	-02	-08	47.45	49.88	-00	-10	49.78	51.40	-00	-12	51.28	52.94		-21	52.73		69
20	47.66	-02	-06	47.58	49.98	-00	-06	49.92	52.25	-00	-09	52.16	53.82		-11	53.71		62
27	47.80	-02	-02	47.76	50.28	-00	-02	50.26	52.00	-00	-02	51.98	52.68		-01	52.67		54
Oct. 4	47.88	-02	-02	47.84	50.25	-00	-01	50.24	51.55	-00	-00	51.55	51.58		-01	51.57		53
12	48.08	-01	-01	48.06	50.22	-00	-01	50.21	50.58	-00	-00	50.58	50.48		-02	50.46		52
18	48.15	-01	+02	48.16	49.95	-00	+04	49.99	49.73	-00	+05	49.78	48.86		+06	48.92		43
26	48.15	-00	+02	48.17	49.28	-00	+04	49.32	47.96	-00	+04	48.00	46.32		+04	46.36		42
Nov. 1	48.15	-00	-00	48.15	48.75	-00	-00	48.75	46.96	-00	-02	46.94	45.45		-04	45.41		50
9	48.12	+01	-01	49.12	48.06	-00	-01	48.05	46.22	-00	-03	46.19	45.58		-05	45.53		53
15	48.05	+01	+05	48.11	47.54	-00	+05	47.59	45.88	-00	+05	45.93	44.00		+06	44.06		30
22	47.90	+01	+04	47.95	47.04	-00	+03	47.07	43.95	-00	+03	43.98	41.21		+02	41.23		38
29	47.82	+02	+02	47.86	46.03	-00	+01	46.04	42.70	-00	-00	42.70	40.78		-01	40.77		41
Dec. 6	47.64	+02	-00	47.66	45.36	-00	-01	45.35	43.08	-00	-03	43.05	42.43		-05	42.38		41
13	47.38	+02	+02	47.42	45.10	-00	-00	45.10	43.08	-00	-00	43.08	42.02		-00	42.02		41
20	47.14	+02	+06	47.22	44.86	-00	+03	44.89	42.30	-00	+04	42.34	39.90		+05	39.95		31
27	46.95	+03	+03	47.01	44.34	-00	+01	44.35	41.24	-00	+01	41.25	39.26		+01	39.26		31
1842.																		
Jan. 3	46.74	+02	+03	46.79	43.93	-00	+01	43.94	41.61	-00	+01	41.62	40.88		+02	40.90		31
10	46.47	+02	+04	46.53	43.78	-00	+02	43.80	40.85	-00	+03	40.88	38.46		+02	38.48		31
17	46.25	+03	+01	46.32	43.35	+01	+01	43.37	40.03	-00	+01	40.04	37.68		+02	37.68		31
25	46.07	+03	+02	46.12	42.60	-00	-00	42.60	39.57	-00	-00	39.57	37.72		-00	37.72		31
31	45.88	+03	+01	45.92	42.27	-00	-00	42.27	39.19	-00	-00	39.19	37.61		-01	37.60		41
Feb. 7	45.58	+03	+04	45.65	42.19	-00	+01	42.20	39.72	-00	+02	39.74	38.17		+03	38.20		31
15	45.40	+03	-00	45.43	41.85	-00	-00	41.85	39.76	-00	-01	39.75	38.98		-04	38.94		41
21	45.17	+02	+02	45.21	41.90	-00	-00	41.90	40.22	-00	-00	40.22	39.96		+01	39.97		31
Mar. 2	44.90	+02	+01	44.93	42.02	-00	-00	42.02	39.95	-00	-00	39.95	38.82		-00	38.82		41
7	44.88	+02	-01	44.89	41.78	-00	-00	41.78	39.95	-00	-02	39.93	39.62		-05	39.57		41

XIX.—On a Formula representing the Mean Height of the Barometer at the Level of the Sea. By Professor HANSTEEN of Christiania, in a Letter addressed to Professor FORBES, Secretary of the Royal Society of Edinburgh.

OBSERVATORY NEAR CHRISTIANIA, 26th September 1846.

SIR,—You have communicated to me, that the Royal Society of Sciences in Edinburgh has done me the honour to elect me as a corresponding member. I beg you to render my humble thanks to the Society, and to assure, that it shall be my earnest wish to fulfil every task in my power which the Royal Society should demand.

That this letter may not reach your hands without any scientific communication, I subjoin the following :—From November 1822 to April 1824 inclusive, I observed the height of the barometer in Christiania, and found the mean reduced to 0° R., and to the level of the sea = $757^m \cdot 763 = 335''' \cdot 913$ lign. de Paris. As the mean height of the barometer observed at Paris by Bouvard, and reduced to 0°, and the level of the sea is = $337''' \cdot 53$, I was surprised at the great difference of $1''' \cdot 62$ between Paris and Christiania. If p denotes the pressure of the atmosphere at the level of the sea, m and h the density of the mercury and its height in the tube, g the force of gravity, we have $p = mgh$, and, in another place, $p' = mg'h'$.

If $p' = p$ is $gh = g'h'$, or $h' = \frac{g}{g'}h$. If, in the first place, the latitude is = ϕ , in the

second, = ϕ' , we have $\frac{g}{g'} = \frac{1 - 0.0025911 \cos 2\phi}{1 - 0.0025911 \cos 2\phi'} = 1 - 0.0025911 (\cos 2\phi - \cos 2\phi')$;

$h - h' = h, 0.0025911 (\cos 2\phi - \cos 2\phi')$. Taking $\phi = 0^\circ$, $\phi' = 90^\circ$, we have $h - h' = 1''' \cdot 74$; and when $\phi = 48^\circ 50'$ (Paris), $\phi' = 59^\circ 55'$ (Christiania), we have $h - h' = 0''' \cdot 32$. But the observations have given for Paris and Christiania $h - h' = 1''' \cdot 62$; consequently, the mean pressure of the atmosphere is not the in different latitudes (“*Magazin for Naturvidensk.*” 1824, page 282-291).

Professor SCHOUW in Copenhagen has, in the Memoirs of the Royal Society of Sciences at Copenhagen for 1832 (page 291-342), collected all the known observations of the mean height of the barometer, which, with exactness, could be reduced to the level of the sea, and to 0° R. In the following table I have added the result of five years' observations here at the Observatory, and of the year 1844 at Bosekop. I have found that the observations can tolerably be represented by the formula

$$\downarrow = 336''' \cdot 8097 + 1''' \cdot 3038 \cos 2\phi - 0''' \cdot 7478 \cos 4\phi - 0''' \cdot 9145 \cos 6\phi + 0''' \cdot 5435 \cos 8\phi.$$

Place.	Observer.	Time.	ϕ	ψ		Difference.
				Observed.	Calculated.	
Christiansborg . . .	Trentepohl & Chenon	22 mo.	5° 30'	336° 95	337° 017	-0° 067
Guayra	Boussingault . . .	12 days	10 36	6° 98	7° 113	-0° 131
St Thomas	Hornbeck	1 y.	18 19	7° 13	7° 497	-0° 367
Rio Janeiro . . .	Eschwege	3 mo.	-22 54	8° 69	7° 876	+0° 814
Santa Cruz, Teneriffe	Escolar	3 y.	28 28	8° 77	8° 360	+0° 410
Madeira	Heineken	2 y.	32 36	9° 20	8° 635	+0° 565
Cape of Good Hope	Puhlman and Wahlst	9 y.	-33 55	8° 24	8° 684	-0° 444
Palermo	Cacciatore	35 y.	38 7	8° 21	8° 698	-0° 488
Naples	Brioschi	7 y.	40 51	7° 94	8° 554	-0° 614
Florence	Inghirami	9 y.	43 47	7° 76	8° 262	-0° 502
Avignon	Guérin	10 y.	43 57	7° 80	8° 242	-0° 442
Bologna	Caturegli and Moratti	5 y.	44 30	7° 87	8° 170	-0° 300
Padua	The Astronomers . .	15 y.	45 24	7° 87	8° 044	-0° 174
Paris	Bouvard	11 y.	48 50	7° 53	7° 468	+0° 062
London	Royal Society . . .	7 y.	51 29	7° 33	6° 954	+0° 376
Altona	Schumacher	6 y.	53 33	7° 09	6° 552	+0° 438
Danzig	Strehlke	2 y.	54 21	6° 95	6° 371	+0° 579
Königsberg	Sommer	8 y.	54 43	6° 95	6° 297	+0° 653
Apenrade	Neuber	5 y.	55 3	6° 72	6° 231	+0° 489
Edinburgh	Forbes	3 y.	55 58	6° 13	6° 051	+0° 079
Christiania	Hansteen	5 y.	59 55	6° 18	5° 362	+0° 818
Reikiavik	Thorslenson	12 y.	63 55	3° 36	4° 882	-1° 522
Godthaab	Mühlenpfort	5 y.	64 10	3° 33	4° 861	-1° 531
Godhaven	Graah and Fasting	2½ y.	68	4° 19	4° 694	-0° 504
Bosekop	Thomas	1 y.	69 58	5° 39	4° 715	+0° 675
Melville Island . .	Parry	1 y.	74½	5° 61	4° 992	+0° 618
Spitzbergen	Scoresby	6-12 y.*	75½	5° 47	5° 086	+0° 384

The formula gives a *minimum* for $\phi = 0^\circ$, and $\phi = 68^\circ 23' 8''$, and a *maximum* for $\phi = 36^\circ 12' 6''$, and $\phi = 90^\circ$. The following table gives ψ for every fifth degree of latitude.

ϕ	ψ	ϕ	ψ
0	336° 995	45	338° 101
5	7° 012	50	7° 246
10	7° 096	55	6° 240
15	7° 291	60	5° 345
20	7° 623	65	4° 801
25	8° 057	70	4° 715
30	8° 478	75	5° 037
35	8° 714	80	5° 561
40	8° 612	85	6° 034
45	8° 101	90	6° 216

* So in the original, and also in Schouw's Tables; but surely a mistake.—J. D. F.

The greatest observed difference is between Madeira and Godthaab, $339''' \cdot 2 - 333''' \cdot 3 = 5''' \cdot 9$; the formula gives only $4''' \cdot 0$. According to this table I have constructed a curve, which represents the variation of ψ from equator to the pole; but it is too voluminous for a letter.

If the observations are correct, there is a discontinuity between the eastern and western coasts of the Northern Atlantic Ocean; for instance, Christiania, Bosekop (Norway), compared with Reikiavik (Iceland), and Godthaab, Godhaven (Greenland); as also St Thomas (West Indies), compared with Santa Cruz, Teneriffe, and Madeira. If the whole globe was only an ocean, there would certainly be no such irregularities.

The Norwegian Government has, according to the demand of the Royal Society in London, resolved, that magnetical and meteorological observations, which stopped at the end of June 1843, shall be continued here a year. They were again commenced the 15th August this year, and will be continued to the same date in 1847. The unifilar is observed every tenth minute, mean time Göttingen; the bifilar and meteorological phenomena, every full hour. I am, Sir, sincerely yours,

CHRISTOPHER HANSTEEN.

XX.—On General Differentiation. Part III. By the Rev. P. KELLAND, M.A., F.R.SS.L.&E., F.C.P.S., late Fellow of Queen's College, Cambridge; Professor of Mathematics, &c., in the University of Edinburgh.

(Read December 21, 1846.)

Nearly six years ago, I presented to the Society two Memoirs on the subject of Differentiation, with fractional indices. The method which I adopted to extend the signification of a differential coefficient consisted in assuming that the function $\sqrt[n]{x}$, which enters into the value of the coefficient deduced from a particular hypothesis, is limited only by the definition $\sqrt[n+1]{x} = \sqrt[n]{x} \cdot \sqrt[n+1]{x}$. This generalization appears to be perfectly satisfactory, and promises to offer, if not the only, certainly the best extension of the Differential Calculus. Considering the length of the interval which has elapsed since the publication of my former Memoirs, it is remarkable that so little addition has been made to our knowledge of this branch of analysis. With the exception of one or two papers in LIOUVILLE'S *Journal*, and a few remarks by Professor DE MORGAN, in his Treatise on the Differential Calculus (pp. 598–600), I am not aware that anything has been written on this subject since that time. Seeing, therefore, that others are not willing to enter on this very promising field, I consider it not improper that I should make known a number of extensions of this science to which I have been subsequently led, many of which have been in my possession a considerable time.

I must premise, that the object of this generalization of the differential calculus is, not only to extend the bounds of research beyond the limits of that science, but also to group and classify the results of the science itself. It is, perhaps, as important in the latter aspect as in the former; for its very first consequence is the union of the elementary forms of the two separate branches of that science—the differential and the integral calculus—into one, so that the integral becomes simply the negative differential. Now it is evident that this can only be done by extending to some form, which is general for the existing calculus, a universal and unrestrained interpretation. Such a form, properly selected, becomes, in the new science, a defining property, precisely in the same way that the common differential coefficient is the defining property of the differential calculus. There are several forms which might appear appropriate to this purpose: that which I have adopted is the differential coefficient of x^n . The assumption, therefore, on which the science is based, is the following: that

$\frac{d^\mu x^r}{d x^\mu} = (-1)^\mu \frac{r - n + \mu}{-n} x^{n-\mu}$, whatever be n and μ . This form can be proved to be the correct one in every interpretable case, and can be deduced from the generalization of $\frac{d^\mu e^{c x}}{d x^\mu}$ when n is negative.* We shall at present assume it as the *defining property* or *definition* of $\frac{d^\mu x^n}{d x^\mu}$.

When, from this definition, we can deduce the differential coefficients of $e^{c x}$ and of $\log x$, that is, of the ascending and descending index-function, we are in possession of the three fundamental forms from which all others may be derived. The following mode of arriving at those differential coefficients is different from that which has hitherto been given, and appears to leave nothing to be desired.

1. To find $\frac{d^\mu e^{c x}}{d x^\mu}$.

$$\begin{aligned} e^{c x} &= 1 + c x + \frac{c^2 x^2}{1 \cdot 2} + \frac{c^3 x^3}{1 \cdot 2 \cdot 3} + \&c. = \sum \frac{c^r x^r}{r!} \\ \therefore \frac{d^\mu e^{c x}}{d x^\mu} &= (-1)^\mu \sum \frac{\sqrt{-r+\mu}}{-r} \frac{c^r x^{r-\mu}}{r+1} \\ &= (-c)^\mu \frac{\sqrt{\mu}}{\sqrt{0}} \left\{ (c x)^{-\mu} + \frac{(c x)^{1-\mu}}{1-\mu} + \frac{(c x)^{2-\mu}}{(1-\mu)(2-\mu)} + \&c. \right\} \\ &= (-c)^\mu \frac{\sqrt{\mu}}{\sqrt{0}} \left\{ z^{-\mu} + \frac{d^{-1}}{d z^{-1}} z^{-\mu} + \frac{d^{-2}}{d z^{-2}} z^{-\mu} + \&c. \right\}, \text{ where } z = c x; \end{aligned}$$

* See Part I., and the excellent Memoir of M. LIOUVILLE, referred to in that Treatise. Another formula has been proposed, viz.

$$\frac{d^\mu x^n}{d x^\mu} = \frac{\sqrt{1+n}}{\sqrt{1+n-\mu}} x^{n-\mu}.$$

I have lately received from Mr W. CENTER, of Langside, some judicious remarks on these formulæ, contrasting the results arrived at by them respectively. He shews that (without continual introduction of an infinite arbitrary constant) the latter formula is inapplicable in many of the most simple cases: for example, in d^μ of $\frac{1}{1+x}$ expanded positively, it gives, when applied, infinity on one side and not on the other, and when expanded negatively, infinity on both sides; and again, it gives for $\frac{d^\mu a}{d x^\mu}$ or

$\frac{d^\mu a x^0}{d x^\mu}$ - the value $\frac{1}{\sqrt{1-n}} x^{-\mu}$, which is a function of x when μ is a positive proper fraction.

$$= (-c)^{\mu} \frac{\sqrt{\mu}}{0} \left\{ 1 - \left(\frac{d}{dz} \right)^{-1} \right\}^{-1} z^{-\mu} = c^{\mu} \left(\frac{d}{dz} - 1 \right)^{-1} \frac{d^{\mu+1}}{dz^{\mu+1}} \cdot 1$$

Let $y = \left(\frac{d}{dz} - 1 \right)^{-1} \frac{d^{\mu+1}}{dz^{\mu+1}} \cdot 1$; then

$$\frac{dy}{dz} - y = \frac{d^{\mu+1}}{dz^{\mu+1}} \cdot 1$$

$$y = e^z \left(C + \int dz e^{-z} \frac{d^{\mu+1}}{dz^{\mu+1}} \cdot 1 \right)$$

Now $\frac{d^{\mu+1}}{dz^{\mu+1}} 1 = 0$, except when μ is a negative whole number; in which case

$$\frac{d^{\mu+1}}{dz^{\mu+1}} 1 = \frac{z^{-\mu-1}}{-\mu-1}.$$

$\therefore y = C e^z$; except when μ is a negative whole number, in which case

$$y = C e^z - \frac{z^{-\mu-1}}{\mu-2} - \frac{z^{-\mu-1}}{\mu-2} - \&c.$$

Now, in all cases we omit the arbitrary functions in differentiation to any index; they being readily supplied when required. But $\frac{z^{-\mu-1}}{-\mu-1} + \&c.$, is evidently included in the arbitrary function, in the case in question; we may therefore omit it, and write generally,

$$y = C e^z, \text{ or}$$

$$\frac{d^{\mu} e^{c x}}{dx^{\mu}} = c^{\mu} C e^z = c^{\mu} C e^{c x} \quad . \quad . \quad . \quad (1)$$

This result has been deduced from the definition without any assumption whatever relative to the function $\sqrt[n]{}$, except that it satisfies the condition $\sqrt[n+1]{} = n \sqrt[n]{}$. We may, consequently, obtain the value of the constant C , by admitting, that when n is positive, $\sqrt[n]{}$ coincides with LEGENDRE'S function $\sqrt[n]{}$. In this case,

$$\frac{\sqrt[n]{}}{x^n} = \int_0^{\infty} e^{-\alpha x} \alpha^{n-1} d\alpha.$$

Therefore, differentiating, to the index μ ,

$$\frac{\sqrt[n+\mu]{}}{x^{n+\mu}} = C \int \alpha^{\mu+n-1} e^{-\alpha x} d\alpha, \text{ by the definition and equation (1).}$$

But if $n + \mu$ be positive, $\sqrt[n+\mu]{}$ also coincides with LEGENDRE'S function, therefore,

$$\frac{\sqrt[n+\mu]{}}{x^{n+\mu}} = \int e^{-\alpha x} \alpha^{n+\mu-1} d\alpha, \text{ or } C = 1.$$

Now C is altogether independent of n : if, therefore, we take n positive and greater than $(-\mu)$, which can always be done, we shall have proved generally, that

$$\frac{d^\mu e^{cx}}{dx^\mu} = c^\mu e^{cx} \dots (2).$$

It will be observed that the properties on which the truth of equation (2) is based, are these,—

1. $\frac{d^\mu x^n}{dx^\mu} = (-1)^\mu \frac{(-n+\mu)}{(-n)} x^{n-\mu}$
2. $\sqrt[n+1]{} = n\sqrt[n]{} \}$ whatever be n .
3. $\frac{\sqrt[n]{} }{x^n} = \int_0^x e^{-ax} a^{n-1} da$, when n is positive.

2. To find $\frac{d^\mu \log x}{dx^\mu}$.

In my previous Memoir, Art. 19, I obtained an expression for $\frac{d^\mu \log x}{dx^\mu}$, by assuming that $\int \frac{dx}{x} = \log x$; an assumption which owes its correctness to the admitted possibility of the introduction of an arbitrary constant of integration. Consequently, the conclusions at which I arrived can only be correct generally, by the aid of an arbitrary function of differentiation. Now, it is our object to avoid the use of such functions, and to obtain expressions for the general differential coefficient of all functions which shall be complete in themselves, so far as relates to the satisfaction of every law of combination to which they may be subjected. It becomes necessary, therefore, to reject the equation $\int \frac{dx}{x} = \log x$, and to substitute in its place some other function of x . The following process appears to be perfectly satisfactory.

$$\begin{aligned} \text{The value of } \frac{x^p - x^q}{p} \text{ is } & \frac{1+p \log x + \&c. - 1 - q \log x - \&c.}{p} \\ & = \log x - \frac{q}{p} \log x + A p + \&c. \end{aligned}$$

If, therefore, q be of a higher order than p , such as p^2 , it is manifest that $\frac{x^p - x^q}{p}$ will be a simple representation of $\log x$, provided $p=0$ and $\frac{q}{p}=0$.

By adopting this mode of representation we obtain,

$$\frac{d^\mu \log x}{dx^\mu} = (-1)^\mu \frac{\mu-p}{p, -p} \frac{1}{x^{\mu-p}} - (-1)^\mu \frac{\mu-q}{p, -q} \frac{1}{x^{\mu-q}}.$$

This expression comprehends every case, and appears to be the most simple

form under which the μ th differential coefficient of a logarithm can be represented.

We shall reduce it in the different cases :

1. When μ is a negative whole number $= -m$.

$$\begin{aligned}\sqrt{\mu-p} &= \sqrt{-(m+p)}; \text{ and } \sqrt{-p} = (-p-1)\sqrt{-p-1} \\ &= (-p-1)(-p-2)\dots(-p-m)\sqrt{-(m+p)} \\ &= (-1)^m \frac{\sqrt{1+m+p}}{\sqrt{1+p}} \sqrt{-(m+p)}\end{aligned}$$

$$\therefore \frac{\sqrt{\mu-p}}{\sqrt{-p}} = (-1)^\mu \frac{\sqrt{1+p}}{\sqrt{1-\mu+p}}$$

$$\text{and } \frac{\sqrt{\mu-q}}{\sqrt{-q}} = (-1)^\mu \frac{\sqrt{1+q}}{\sqrt{1-\mu+q}}$$

Hence
$$\frac{d^\mu \log x}{d x^\mu} = (-1)^{2\mu} \frac{\sqrt{1+p}}{\sqrt{1-\mu+p}} \frac{x^{-\mu+p}}{p} - (-1)^{2\mu} \frac{\sqrt{1+q}}{\sqrt{1-\mu+q}} \frac{x^{-\mu+q}}{p}$$

But
$$\frac{\sqrt{1+p}}{\sqrt{1-\mu+p}} = \frac{1}{\sqrt{1-\mu}} (1-p A + \&c.) \text{ where } A = \frac{1}{1} + \frac{1}{2} + \dots \frac{1}{-\mu}$$

and $x^p = 1 + p \log x + \&c.$

also
$$\frac{\sqrt{1+q}}{\sqrt{1-\mu+q}} = \frac{1}{\sqrt{1-\mu}} (1-q A + \&c.)$$

 $x^q = 1 + q \log x + \&c.$

$$\begin{aligned}\therefore \frac{d^\mu \log x}{d x^\mu} &= \frac{x^{-\mu}}{\sqrt{1-\mu}} \cdot \frac{(1-p A + \&c.) (1+p \log x + \&c.)}{p} \\ &\quad - \frac{x^{-\mu}}{\sqrt{1-\mu}} \cdot \frac{(1-q A + \&c.) (1+q \log x + \&c.)}{p}\end{aligned}$$

$$= \frac{x^{-\mu}}{\sqrt{1-\mu}} \left(\log x - A - \frac{q}{p} \log x + \frac{q A}{p} + \&c. \right)$$

$$= \frac{x^{-\mu}}{\sqrt{1-\mu}} (\log x - A), \text{ since } p \text{ and } \frac{q}{p} \text{ are both equal to } 0.$$

Hence
$$\frac{d^{-m} \log x}{d x^{-m}} = \frac{x^m}{\sqrt{m+1}} \left\{ \log x - \left(\frac{1}{1} + \frac{1}{2} + \&c. + \frac{1}{m} \right) \right\} \text{ which is a well known}$$

expression for $\int^{(m)} d x^m \log x$

2. If μ be not a negative whole number, $\sqrt{\mu}$ is finite; and

$$\frac{\sqrt{\mu-p}}{p\sqrt{-p}} = -\frac{\sqrt{\mu-p}}{\sqrt{1-p}} = -\sqrt{\mu} (1 + B p + \&c.)$$

by supposing this function (which is finite) expanded in terms of p ;

similarly

$$\frac{\sqrt{\mu-q}}{q\sqrt{-q}} = -\sqrt{\mu}(1+Bq+\&c.);$$

and from the expression in Art 2.

$$\begin{aligned} \frac{d^\mu \log x}{d x^\mu} &= (-1)^{\mu+1} \frac{\sqrt{\mu}}{x^\mu} (1+Bp+\&c.) (1+p \log x + \&c.) \\ &\quad - (-1)^{\mu+1} \frac{\sqrt{\mu}}{x^\mu} (1+Bq+\&c.) (1+q \log x + \&c.) \frac{q}{p} \\ &= (-1)^{\mu+1} \frac{\sqrt{\mu}}{x^\mu}. \end{aligned}$$

3. The expression given above for the differential coefficient of a logarithm is, therefore, perfectly general, and is applicable to all cases. It is essentially analytical in its nature, and does not appear to be reducible to a more arithmetical form so as to retain its general character. The expression which I previously gave exhibits very simply the n th differential coefficient of a logarithm as well as its n th integral, when n is a whole number, and may be, consequently, regarded as the most comprehensive arithmetical form of this function which we can at present obtain.

It may not be considered out of place here to introduce the deduction of the value of $\frac{d^n \log x}{d x^n}$, when n is a positive or a negative whole number, from this form also.

The equation is

$$\begin{aligned} \frac{d^n \log x}{d x^n} &= \frac{\sqrt{n}(-1)^{n+1}}{\sqrt{-1} x^n} \left\{ \log x - (n+1)n \left(\frac{1}{(n+1)n} + \frac{1}{n(n-1)} + \right. \right. \\ &\quad \left. \left. \frac{1}{2} \frac{1}{(n-1)(n-2)} + \frac{1}{3} \frac{1}{(n-2)(n-3)} + \&c. \right) \right\} \\ &\quad \text{(Part I, Art. 21.)} \end{aligned}$$

(1.) If n be a positive whole number, the only terms in this expression which are not indefinitely small, are,

$$\begin{aligned} &\frac{\sqrt{n}(-1)^{n+2}}{\sqrt{-1} x^n} (n+1)n \left(\frac{1}{n(n-n+1)(n-n)} + \frac{1}{(n+1)(n-n)(n-n-1)} \right) \\ &= \frac{\sqrt{n}(-1)^{n+2}}{\sqrt{-1} x^n} (n+1)n \left(\frac{1}{n(n-n)} - \frac{1}{(n+1)(n-n)} \right) \\ &= \frac{\sqrt{n}(-1)^{n+2}}{\sqrt{-1} x^n (n-n)} = \frac{\sqrt{n}(-1)^{n+2} \sqrt{n-n}}{x^n (n-n-1)(n-n+1)} \\ &= \frac{\sqrt{n}(-1)^{n+2} (n-n-1)}{x^n} = \frac{\sqrt{n}(-1)^{n+3}}{x^n} = \frac{(-1)^{n+1} 1 \cdot 2 \dots (n-1)}{x^n} \end{aligned}$$

the well known form.

(2.) If n be a negative integer $= -m$;

$$\begin{aligned}\text{Let } y &= \frac{z^{m+1}}{n(n-1)} + \frac{1}{2} \frac{z^{m+2}}{(n-1)(n-2)} + \frac{1}{3} \frac{z^{m+3}}{(n-2)(n-3)} + \&c. \\ &= \frac{z^{m+1}}{m(m+1)} + \frac{1}{2} \frac{z^{m+2}}{(m+1)(m+2)} + \frac{1}{3} \frac{z^{m+3}}{(m+2)(m+3)} + \&c. \\ \therefore \frac{d^2 y}{dz^2} &= z^{m-1} + \frac{1}{2} z^m + \&c. \\ &= -z^{m-2} \log(1-z)\end{aligned}$$

whence, by integration,

$$\begin{aligned}y &= -\frac{z^m}{m(m-1)} \log(1-z) + \frac{1}{m(m-1)} \left\{ \frac{z^m}{m} + \frac{z^{m-1}}{m-1} + \&c. + z \right\} \\ &+ \frac{1}{m(m-1)} \log(1-z) + \frac{1}{m-1} \left\{ \frac{z^m}{m(m-1)} + \frac{z^{m-1}}{(m-1)(m-2)} + \&c. + \frac{z^2}{2 \cdot 1} \right\} \\ &+ \frac{z}{m-1} \log(1-z) - \frac{z}{m-1} - \frac{1}{m-1} \log(1-z).\end{aligned}$$

Consequently, the value of y between the limits 0 and 1 is

$$\begin{aligned}y &= \frac{1}{m(m-1)} \left(\frac{1}{1} + \frac{1}{2} + \&c. \dots + \frac{1}{m} \right) \\ &+ \frac{1}{m-1} \left(\frac{1}{1 \cdot 2} + \frac{1}{2 \cdot 3} + \&c. + \frac{1}{(m-1)m} \right) - \frac{1}{m-1} \\ &= \frac{1}{m(m-1)} \left(\frac{1}{1} + \frac{1}{2} + \&c. + \frac{1}{m} \right) + \frac{1}{m} - \frac{1}{m-1} \\ &= \frac{1}{m(m-1)} \left\{ \frac{1}{2} + \frac{1}{3} + \&c. + \frac{1}{m} \right\}\end{aligned}$$

$$\begin{aligned}\text{and } \frac{d^{-m} \log x}{dx^{-m}} &= \frac{(-m-1)^{-m+1}}{(-1) x^{-m}} \left\{ \log x - 1 - m(m-1)y \right\} \\ &= \frac{x^m (-1)^{-m+1}}{(-m)(-m+1) \dots (-2)} \left\{ \log x - \left(\frac{1}{1} + \frac{1}{2} + \&c. + \frac{1}{m} \right) \right\} \\ &= \frac{x^m}{m(m-1) \dots 2} \left\{ \log x - \left(\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \&c. + \frac{1}{m} \right) \right\}\end{aligned}$$

which is the expression for $\int^{(m)} dx^m \log x$.

4. In my previous memoirs, I have obtained the general differential coefficients of several functions, and have applied the results to the solution of analytical and mechanical problems. It will be my object at present, to extend the science itself by exhibiting the solution of differential equations, and by investigating some of the properties of finite differences. In every instance I shall select the most simple problems which will serve to illustrate the process employed. Of the process itself, consisting entirely of the application of the *calculus of opera-*

tions, it is, perhaps, necessary to say a few words. The principle on which that calculus is founded is this :

If the laws which regulate the combinations of symbols of operation be the same as those which regulate the combinations of symbols of quantity, then all forms which would be equivalent relative to the latter, must also be equivalent relative to the former.

The laws to which symbols of quantity are subject, may be briefly classed under the seven following heads.

1. Their affections by numbers, or numerical quantities, are the same as if they themselves were numbers, or numerical quantities.

2. The law of signs.

3. The order of simple operations is indifferent.

4. The order of combined operations is indifferent.

5. Combined operations may be distributed.

6. and 7. The laws of indices.

Hence, if d , ϕ , ψ are any symbols of operation, subject to these laws (a and b being numerical quantities) :

$$1. (a \pm b) \phi = a \phi \pm b \phi = a \phi \pm \phi b; \text{ \&c.}$$

$$2. (a \pm \phi) (b \mp \psi) = a b \mp a \psi \pm b \phi - \phi \psi; \text{ \&c.}$$

$$3. \phi + \psi = \psi + \phi$$

$$4. \phi \psi = \psi \phi$$

$$5. d(\phi + \psi) = d\phi + d\psi$$

$$6. d^a d^b = d^{a+b}$$

$$7. (d^a)^b = d^{a \cdot b}$$

results which would be equivalent were d , ϕ , ψ numerical quantities, are equivalent when they are operations. For example,

$$(d + \phi)^n = d^n + n d^{n-1} \phi + \frac{n(n-1)}{1 \cdot 2} d^{n-2} \phi^2 + \text{\&c.}$$

The symbols of differentiation $\frac{d}{dx}$, $\frac{d}{dy}$ and of difference Δ_x , Δ_y satisfy these conditions.

It must be observed, in applying the principle which I have laid down, that it is inapplicable, unless it hold with respect to *every symbol* which enters into the operation. It will evidently apply to the ordinary symbols d and Δ as combined with each other, and to the symbols x , y as combined with each other; but it will not apply to the symbols d and x as combined with each other, because the fourth law is violated by their combination : For example,

$$d \Delta x^2 = 2, \quad \Delta d x^2 = 2$$

$$\therefore d \Delta x^2 \neq \Delta d x^2;$$

$$\begin{aligned} \text{But} \quad x dx^2 &= 2x^2, \quad dx x^2 = 3x^2 \\ \therefore x dx^2 &\text{ is not equal to } dx x^2. \end{aligned}$$

In proof of the sufficiency of the principle here laid down, it may be remarked, that both symbols of operation and symbols of quantity are defined or characterized by the above laws. The symbols of combination are indeed originally framed from arithmetic, but are subsequently generalized, and the basis of generalization is *obedience to these laws*. Thus the symbols + and - are generalized by *collective* symbols the reverse of each other, expressed by the equation $+a - a = +0 = -0$; where +0 is arithmetical, or signifies (as an operation *strictly*) *increased by 0*: \times and \div are 'cumulative symbols the reverse of each other,' expressed by the equation $\times a \div a = \times 1 = \div 1$; where $\times 1$ signifies *strictly multiplied by 1*. These definitions are in perfect conformity with the above laws. And a similar remarks applies to the general definition of an index.

Now certain symbols of operation, although not, like symbols of quantity, framed with direct reference to the above laws, do, notwithstanding, satisfy them. Consequently, *algebraic formulæ which are results of these laws and of nothing else, must be correct forms also when the algebraic symbols are replaced by such symbols of operation.*

SECTION I. LINEAR DIFFERENTIAL EQUATIONS.

Preliminary Theorems.

5. Since $\left(\frac{d}{dx}\right)^\mu e^{cx} = c^\mu e^{cx}$, it is evident that if $f\left(\frac{d}{dx}\right)$ be any function whatever of $\frac{d}{dx}$, we shall have $f\left(\frac{d}{dx}\right)e^{cx} = f(c)e^{cx}$. (A).

Let u be a function of x , and suppose it expanded in the form $u = \sum a_m e^{mx}$; then

$$e^{rx} u = \sum a_m e^{(m+r)x}; \text{ and hence}$$

$$\begin{aligned} \left(\frac{d}{dx}\right)^\mu \cdot e^{rx} u &= \sum a_m (m+r)^\mu e^{(m+r)x}, \text{ by (A)} \\ &= e^{rx} \sum a_m (m+r)^\mu e^{mx} \\ &= e^{rx} \sum a_m \left(\frac{d}{dx} + r\right)^\mu e^{mx} \text{ by (A)} \\ &= e^{rx} \left(\frac{d}{dx} + r\right)^\mu \sum a_m e^{mx} \\ &= e^{rx} \left(\frac{d}{dx} + r\right)^\mu \cdot u \end{aligned}$$

$$\therefore f\left(\frac{d}{dx}\right) \cdot e^{rx} u = e^{rx} f\left(\frac{d}{dx} + r\right) \cdot u \quad (\text{B})$$

Let $x = e^{\theta}$, and suppose u expanded in the form $u = \sum a_n x^{-n}$: also write D for $\frac{d}{d\theta}$: then

$$\begin{aligned} x^{\mu} \left(\frac{d}{dx}\right)^{\mu} \cdot \frac{1}{x^{\mu}} &= (-1)^{\mu} \frac{\overline{n+\mu}}{n} x^{-n} \\ \therefore x^{\mu} \left(\frac{d}{dx}\right)^{\mu} \cdot u &= (-1)^{\mu} \sum a_n \frac{\overline{n+\mu}}{n} x^{-n} \\ &= (-1)^{\mu} \sum a_n \frac{\overline{n+\mu}}{n} e^{-n\theta} \\ &= (-1)^{\mu} \sum a_n \frac{\overline{-D+\mu}}{\overline{-D}} e^{-n\theta} \quad \text{by (A)} \\ &= (-1)^{\mu} \frac{\overline{-D+\mu}}{\overline{-D}} \sum a_n x^{-n} \\ &= (-1)^{\mu} \frac{\overline{-D+\mu}}{\overline{-D}} \cdot u \quad (\text{C}) \end{aligned}$$

As a particular case of formula (B) we have

$$e^{r\theta} \frac{\overline{-D+\mu}}{\overline{-D}} \cdot u = \frac{\overline{-D+\mu+r}}{\overline{-D+r}} \cdot e^{r\theta} u \quad (\text{D})$$

These four theorems will be found of the utmost importance in reducing differential equations. Formulæ somewhat analogous have been applied to the solution of common differential equations by M. CAUCHY, *Exercices*, vol. i., p. 163, and *Exercices d'Analyse*, ii., 343; by Mr GREGORY, *Cambridge Mathematical Journal*, i., 22, &c.; and by Mr BOOLE, *Philosophical Transactions*, 1844, 225. Under the different heads in which we shall arrange differential equations, we shall solve only the most simple examples, our object being to illustrate the method of proceeding rather than to exhibit its power.

CLASS I. *Equations which are capable of solution without transformation.*

6. EX. I. $\frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} - a^{\frac{1}{2}} y = 0.$

By writing d for $\frac{d}{dx}$, this equation becomes

$$(d^{\frac{1}{2}} - a^{\frac{1}{2}}) y = 0 \quad \text{or} \quad y = (d^{\frac{1}{2}} - a^{\frac{1}{2}})^{-1} \cdot 0.$$

Suppose $y = \Sigma b_m e^{m x}$; then by (A)

$$\Sigma b_m (m^{\frac{1}{2}} - a^{\frac{1}{2}}) e^{m x} = 0; \text{ which can only be satisfied when } m = a.$$

$\therefore y = A e^{a x}$ is the solution of the equation.

We might have proceeded in a somewhat different manner, as follows:

Put $0 e^{m x}$ for 0, then

$$y = (d^{\frac{1}{2}} - a^{\frac{1}{2}})^{-1} \cdot 0 e^{m x} = \frac{0 e^{m x}}{m^{\frac{1}{2}} - a^{\frac{1}{2}}} \text{ by (A).}$$

But $\frac{0}{m^{\frac{1}{2}} - a^{\frac{1}{2}}}$ is finite only when $m = a$; and then it is constant; $\therefore y = A e^{a x}$,

as before.

Ex. 2. $\frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} - a^{\frac{1}{2}} y = X$; X being any function of x .

We have $y = (d^{\frac{1}{2}} - a^{\frac{1}{2}})^{-1} \cdot X + (d^{\frac{1}{2}} - a^{\frac{1}{2}})^{-1} \cdot 0$.

If $X = \Sigma b_r e^{r x}$

$$y = A e^{a x} + \Sigma \frac{b_r}{r^{\frac{1}{2}} - a^{\frac{1}{2}}} e^{r x} \quad (\text{Ex. 1.})$$

Cor. 1. If $r = a$, $\frac{b_r}{r^{\frac{1}{2}} - a^{\frac{1}{2}}} e^{r x}$ becomes infinite. In this case put $a + \alpha$ in place of r ;

then $\frac{b_r}{r^{\frac{1}{2}} - a^{\frac{1}{2}}} e^{r x}$ becomes $b_r e^{a x} \frac{1 + \alpha x + \&c.}{2 a^{\frac{1}{2}} + \&c.}$

$$= \frac{2 a^{\frac{1}{2}} b_r}{a} e^{a x} + 2 b_r a^{\frac{1}{2}} x e^{a x}, \text{ when } \alpha = 0;$$

of which the first term may be incorporated with $A e^{a x}$; and the complete solution is

$$y = A e^{a x} + 2 b_r a^{\frac{1}{2}} x e^{a x} + \Sigma \frac{b_s e^{s x}}{s^{\frac{1}{2}} - a^{\frac{1}{2}}}$$

Cor. 2. If $X = b x^{-n}$, we have, by the well-known formula

$$\frac{1}{x^n} = \frac{1}{n} \int_0^\infty e^{-\alpha x} \alpha^{n-1} d\alpha,$$

$$\therefore (d^{\frac{1}{2}} - a^{\frac{1}{2}})^{-1} \cdot \frac{1}{x^n} = \frac{1}{n} \int_0^\infty \frac{e^{-\alpha x} \alpha^{n-1} d\alpha}{(-\alpha)^{\frac{1}{2}} - a^{\frac{1}{2}}} \text{ by (A.)}$$

$$= -\frac{1}{n} \int_0^\infty \left(\frac{1}{a^{\frac{1}{2}}} + \frac{(-\alpha)^{\frac{1}{2}}}{a} + \frac{(-\alpha)}{a^{\frac{3}{2}}} + \&c. \right) e^{-\alpha x} \alpha^{n-1} d\alpha$$

$$\begin{aligned}
&= -\frac{1}{n} \left(\frac{\bar{n}}{a^{\frac{1}{2}} x^n} + \frac{(-1)^{\frac{1}{2}} \overline{n+1}}{a x^{n+\frac{1}{2}}} - \frac{\overline{n+1}}{a^{\frac{3}{2}} x^{n+1}} - \&c. \right) \\
&= -\frac{1}{a^{\frac{1}{2}}} \left(\frac{1}{x^n} - \frac{n}{a x^{n+1}} + \frac{n(n+1)}{a^2 x^{n+2}} - \&c. \right) \\
&= -\sqrt{-1} \frac{\overline{n+\frac{1}{2}}}{\sqrt{n}} \left(\frac{1}{a x^{n+\frac{1}{2}}} - \frac{n+\frac{1}{2}}{a^2 x^{n+\frac{3}{2}}} + \frac{(n+\frac{1}{2})(n+\frac{3}{2})}{a^3 x^{n+\frac{5}{2}}} + \&c. \right) \\
&= a^{\frac{1}{2}} e^{\alpha x} \int \frac{e^{-\alpha x}}{x^n} dx + \sqrt{-1} \frac{\overline{n+\frac{1}{2}}}{\sqrt{n}} e^{\alpha x} \int \frac{e^{-\alpha x}}{x^{n+\frac{1}{2}}} dx \\
\therefore y &= A e^{\alpha x} + B e^{\alpha x} \left\{ a^{\frac{1}{2}} \int \frac{e^{-\alpha x}}{x^n} dx + \sqrt{-1} \frac{\overline{n+\frac{1}{2}}}{n} \int \frac{e^{-\alpha x}}{x^{n+\frac{1}{2}}} dx \right\}
\end{aligned}$$

7. The solution of the foregoing examples might have been obtained very differently, thus :

$$\text{If} \quad d^{\frac{1}{2}} y - a^{\frac{1}{2}} y = X; \quad y = \frac{x}{d^{\frac{1}{2}} - a^{\frac{1}{2}}} = \frac{d^{\frac{1}{2}} + a^{\frac{1}{2}}}{d - a} \cdot X$$

Now $\frac{1}{d-a} X$ is the solution of the ordinary differential equation $\frac{dv}{dx} - av = X$; its value is, consequently, $e^{\alpha x} \left(\int e^{-\alpha x} X dx + C \right)$. Hence

$$y = \frac{d^{\frac{1}{2}}}{d - a} \cdot e^{\alpha x} \left(\int e^{-\alpha x} X dx + C \right) + a^{\frac{1}{2}} e^{\alpha x} \int (e^{-\alpha x} X dx + C)$$

For instance, if $X=0$, the solution of the equation is

$$y = 2 a^{\frac{1}{2}} C e^{\alpha x};$$

which is the same as that given above.

$$\text{8. Ex. 3.} \quad \frac{dy}{dx} + \frac{a d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} + b y = 0$$

This may be written $(d + a d^{\frac{1}{2}} + b) \cdot y = 0$; or $(d^{\frac{1}{2}} - \alpha^{\frac{1}{2}})(d^{\frac{1}{2}} - \beta^{\frac{1}{2}}) \cdot y = 0$; where $\alpha^{\frac{1}{2}} + \beta^{\frac{1}{2}} = -a$, and $(\alpha \beta)^{\frac{1}{2}} = b$, or $\alpha^{\frac{1}{2}}, \beta^{\frac{1}{2}}$ are the roots of the equation $z^2 + az + b = 0$.

$$\begin{aligned}
\therefore y &= A(d^{\frac{1}{2}} - \alpha^{\frac{1}{2}})^{-1} \cdot 0 + B(d^{\frac{1}{2}} - \beta^{\frac{1}{2}})^{-1} \cdot 0 \\
&= A e^{\alpha x} + B e^{\beta x} \quad (\text{Ex. 1.})
\end{aligned}$$

COR. 1. If $\alpha = \beta$, we must write $\alpha + e$ instead of β , and proceed as in similar cases.

$$\text{The result is} \quad y = A e^{\alpha x} + B x e^{\alpha x}$$

COR. 2. In precisely the same way we may find the solution of the equation

$$\frac{d^{\frac{2}{3}} y}{d x^{\frac{2}{3}}} + a \frac{dy}{dx} + b \frac{d^{\frac{1}{3}} y}{d x^{\frac{1}{3}}} + e y = 0.$$

If $\alpha^{\frac{1}{2}}, \beta^{\frac{1}{2}}, \gamma^{\frac{1}{2}}$ be the roots of the equation $z^3 + a z^2 + b z + c = 0$, the solution is

$$y = A e^{\alpha x} + B e^{\beta x} + C e^{\gamma x}$$

And a similar process applies to equations of all orders, with constant coefficients.

9. It will be seen that in solving these equations, we treat symbols of operation in exactly the same way as if they were symbols of quantity. Our justification for so doing is an appeal to the fact, that the laws which regulate the combination of the former symbols are precisely the same as those which regulate the combination of the latter. Were it otherwise,—were one of the symbols, for instance, to be subject to a different law relative to its combination with one class of symbols from that which regulates its combination with another, we should not be at liberty to separate the operations of such symbols, nor even to combine them otherwise than in the form in which they are actually presented to us. An example will illustrate this remark. The combination $(d^m d^n) \times (d^m d^n) \cdot u$ may be written $(d^m \times d^m)^2 \cdot u$, in which form it is equivalent to $d^{2m} d^{2n} \cdot u$: but the combination $(d^m x^n) \times (d^m x^n) \cdot u$, when written (as we shall write it) $(d^m x^n)^2 \cdot u$, is not equivalent to $d^{2m} x^{2n} \cdot u$. The commutative law, or the law according to which operations may be taken in *any order*, is not true of the symbols d^m, x^n , in their combination with one another.

We may remark, in addition, that when an operation on y has been changed into the reciprocal operation on 0 or on X , giving the solution

$$y = \frac{1}{(D^{\frac{1}{2}} - \alpha^{\frac{1}{2}})(D^{\frac{1}{2}} - \beta^{\frac{1}{2}})} 0, \text{ for instance; the operation } \frac{1}{(D^{\frac{1}{2}} - \alpha^{\frac{1}{2}})(D^{\frac{1}{2}} - \beta^{\frac{1}{2}})} \text{ is resolved}$$

into the two operations $\frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \frac{1}{D^{\frac{1}{2}} - \alpha^{\frac{1}{2}}} - \frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \frac{1}{D^{\frac{1}{2}} - \beta^{\frac{1}{2}}}$, in the same manner as a

fraction is resolved into its equivalent partial fractions. On this subject the reader may consult an excellent paper by Mr BOOLE, in the Cambridge Mathematical Journal, vol. ii., p. 114, where this method is first employed.

10. Ex. 4.
$$\frac{dy}{dx} + a \frac{d^2 y}{dx^2} + b y = X.$$

This gives
$$y = (d^{\frac{1}{2}} - \alpha^{\frac{1}{2}})^{-1} (d^{\frac{1}{2}} - \beta^{\frac{1}{2}})^{-1} \cdot (X + 0)$$

Now
$$\frac{X}{(d^{\frac{1}{2}} - \alpha^{\frac{1}{2}})(d^{\frac{1}{2}} - \beta^{\frac{1}{2}})} = \frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \frac{X}{d^{\frac{1}{2}} - \alpha^{\frac{1}{2}}} - \frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \frac{X}{d^{\frac{1}{2}} - \beta^{\frac{1}{2}}}$$

$\therefore y = A e^{\alpha x} + B e^{\beta x} + \frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \left\{ (d^{\frac{1}{2}} - \alpha^{\frac{1}{2}})^{-1} X - (d^{\frac{1}{2}} - \beta^{\frac{1}{2}})^{-1} X \right\} \quad (\text{Ex. 3.})$

Cor. 1. If
$$X = \sum b_r e^{r x};$$

$$y = A e^{\alpha x} + B e^{\beta x} + \frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \sum b_r e^{r x} \left(\frac{1}{r^{\frac{1}{2}} - \alpha^{\frac{1}{2}}} - \frac{1}{r^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \right)$$

$$= A e^{\alpha x} + B e^{\beta x} + \Sigma \frac{b_r e^{r x}}{r + a r^{\frac{1}{2}} + b}$$

Cor. 2. If $X = b_r$ a constant, $\therefore r = 0$ and

$$y = A e^{\alpha x} + B e^{\beta x} + \frac{b_r}{b}$$

Cor. 3. If $r + a r^{\frac{1}{2}} + b = 0$, r must be equal either to a or to β . Suppose $r = a$:

then $b_r e^{r x} \frac{1}{r + a r^{\frac{1}{2}} + b}$ becomes, by writing $\alpha^{\frac{1}{2}} + c$ in place of $r^{\frac{1}{2}}$,

$$b_r \frac{e^{\alpha x} (1 + 2 \alpha^{\frac{1}{2}} c x + \&c.)}{2 \alpha^{\frac{1}{2}} c + a c} = C e^{\alpha x} + \frac{b_r x e^{\alpha x} 2 \alpha^{\frac{1}{2}}}{2 \alpha^{\frac{1}{2}} + a}$$

$$\text{and } y = A e^{\alpha x} + B e^{\beta x} + b_r \frac{2 \alpha^{\frac{1}{2}} x e^{\alpha x}}{2 \alpha^{\frac{1}{2}} + a} + \Sigma b_s \frac{e^{s x}}{s + a s^{\frac{1}{2}} + b}$$

Ex. 5. $\frac{d^{-1} y}{d x^{-1}} + a \frac{d^{-\frac{1}{2}} y}{d x^{-\frac{1}{2}}} + b y = X.$

This gives $(d^{-1} + a d^{-\frac{1}{2}} + b) \cdot y = X.$

or $(d^{-\frac{1}{2}} - \alpha^{-\frac{1}{2}})(d^{-\frac{1}{2}} - \beta^{-\frac{1}{2}}) \cdot y = X$; where $\alpha^{-\frac{1}{2}}, \beta^{-\frac{1}{2}}$ are the roots of the equation $z^2 + a z + b = 0$;

$$\begin{aligned} \therefore y &= A e^{\alpha x} + B e^{\beta x} + \frac{(d^{-\frac{1}{2}} - \alpha^{-\frac{1}{2}})^{-1} \cdot X}{\alpha^{-\frac{1}{2}} - \beta^{-\frac{1}{2}}} - \frac{(d^{-\frac{1}{2}} - \beta^{-\frac{1}{2}})^{-1} \cdot X}{\alpha^{-\frac{1}{2}} - \beta^{-\frac{1}{2}}} \\ &= A e^{\alpha x} + B e^{\beta x} + \frac{\alpha^{\frac{1}{2}} \beta^{\frac{1}{2}}}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \left\{ (d^{\frac{1}{2}} - \alpha^{\frac{1}{2}})^{-1} \cdot \alpha^{\frac{1}{2}} \frac{d^{\frac{1}{2}} X}{d x^{\frac{1}{2}}} - (d^{\frac{1}{2}} - \beta^{\frac{1}{2}})^{-1} \beta^{\frac{1}{2}} \frac{d^{\frac{1}{2}} X}{d x^{\frac{1}{2}}} \right\} \end{aligned}$$

which is reduced to Ex. 2.

In precisely the same manner we may solve the more general equation $\frac{d^n y}{d x^n}$

$$+ a \frac{d^{n-\alpha} y}{d x^{n-\alpha}} + b \frac{d^{n-2\alpha} y}{d x^{n-2\alpha}} + \&c. + y = X, n \text{ being a multiple of } \alpha.$$

CLASS II. *Elementary Equations.*

11. The form to which more complicated equations can generally be reduced is $y - m x^n \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} = X$; and it is with equations of this form that we are now to be occupied. The simplest case, when $n = 0$, we have already solved.

Ex. 1. $y - m \sqrt{x} \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} = 0.$

By (C) this is reduced to $y - m \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}} \cdot y = 0,$

or

$$y = \left(1 - m \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}}\right)^{-1} \cdot 0.$$

Suppose

$$y = \sum a_n e^{-n\theta}; \text{ then}$$

$$\sum a_n \left(e^{n-\theta} - m \sqrt{-1} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}} e^{-n\theta} \right) = 0 \text{ by (A);}$$

which can be satisfied only by making $1 - m \sqrt{-1} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}} = 0$; giving, consequently, only one value of n :

Hence $y = \frac{A}{x^n}$ is the complete solution.

COR. If

$$m = \frac{2}{\sqrt{-1} \sqrt{\pi}} = \frac{2}{\sqrt{-1} \sqrt{\frac{1}{2}}}$$

$$\frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}} = \frac{\frac{1}{2} \sqrt{\frac{1}{2}}}{\sqrt{\frac{1}{2}}} = \sqrt{\frac{1}{2}} = \frac{\sqrt{1 + \frac{1}{2}}}{\sqrt{1}};$$

$$\therefore n = 1 \text{ and } y = \frac{A}{x}.$$

EX. 2.

$$y - m \sqrt{x} \frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} = X.$$

The equation in θ is $y - m \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}} y = F e^{-\theta}$

$$\begin{aligned} \therefore y &= \left(1 - m \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}}\right)^{-1} \cdot 0 + \left(1 - m \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}}\right)^{-1} \cdot F e^{-\theta} \\ &= \frac{A}{x^n} + \sum b_r \left(1 - m \sqrt{-1} \frac{\sqrt{r + \frac{1}{2}}}{\sqrt{r}}\right)^{-1} e^{-r\theta} \text{ (Ex. 1 and A)} \end{aligned}$$

COR. If $r = n$; this expression becomes infinite. We must, in this case, write $n + c$ in place of r , expand in terms of c , and finally put $c = 0$.

$$\begin{aligned} \text{We have, thus, } \frac{e^{-r\theta}}{1 - m \sqrt{-1} \frac{\sqrt{r + \frac{1}{2}}}{\sqrt{r}}} &= \frac{e^{-n\theta} (1 - c\theta + \&c.)}{1 - m \sqrt{-1} \left(\frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}} + \frac{d}{dn} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}} \cdot c + \&c. \right)} \\ &= \frac{e^{-n\theta}}{1 - m \sqrt{-1} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}}} + \frac{c e^{-n\theta} \theta}{c m \sqrt{-1} \frac{d}{dn} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}}} + \&c. \\ &= \frac{e^{-n\theta}}{1 - m \sqrt{-1} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}}} + \frac{e^{-n\theta} \theta}{m \sqrt{-1} \frac{d}{dn} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}}} \end{aligned}$$

$$\therefore y = \frac{A}{x^n} + \frac{b_r}{m\sqrt{-1}} \frac{\log x}{x^n} \frac{1}{\frac{d}{dn} \sqrt{n+\frac{1}{2}}} \\ + \Sigma \frac{b_s}{x^s} \frac{1}{1-m\sqrt{-1} \frac{s+\frac{1}{2}}{\sqrt{s}}}$$

Ex. 3. $y - a\sqrt{-1} x \frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} = 0.$

Suppose $y = \Sigma a_r x^{-r}$; then

$$\Sigma (a_r x^{-r} + a \frac{\sqrt{r+1}}{\sqrt{r+\frac{1}{2}}} a_{r+\frac{1}{2}} x^{-r}) = 0$$

or $a_{r+\frac{1}{2}} = -\frac{1}{a} \frac{\sqrt{r+\frac{1}{2}}}{\sqrt{r+1}} a_r. \quad (1.)$

Hence the lowest value of r is 0, and the values succeed at intervals of $\frac{1}{2}$.

$\therefore y = A + \frac{A_1}{\sqrt{x}} + \frac{A_2}{x} + \&c.,$ with the relation expressed by (1). By substitution

$$A_1 = -\frac{1}{a} \frac{\sqrt{\frac{1}{2}}}{\sqrt{1}} A, A_2 = -\frac{1}{a} \frac{\sqrt{1}}{\sqrt{\frac{3}{2}}} A_1, A_3 = -\frac{1}{a} \frac{\sqrt{\frac{3}{2}}}{\sqrt{2}} \&c.$$

$$A_1 = -\frac{1}{a} \sqrt{\pi} A; A_2 = \frac{1}{a^2} \frac{\sqrt{1} \sqrt{\frac{1}{2}}}{\sqrt{\frac{3}{2}} \sqrt{1}} A = \frac{2}{a^2} A$$

$$A_3 = -\frac{1}{a^3} \frac{\sqrt{\frac{1}{2}} \sqrt{1} \sqrt{\frac{3}{2}}}{\sqrt{1} \sqrt{\frac{3}{2}} \sqrt{2}} A = -\frac{\sqrt{\pi}}{a^3} A, A_4 = \frac{1}{a^4} \frac{\sqrt{\frac{1}{2}} \sqrt{1} \sqrt{\frac{3}{2}} \sqrt{2}}{\sqrt{1} \sqrt{\frac{3}{2}} \sqrt{2} \sqrt{\frac{5}{2}}} A = \frac{1}{a^4} \frac{2^2}{1 \cdot 3} A$$

$$A_5 = -\frac{\sqrt{\pi}}{a^5} \cdot \frac{\sqrt{2} \sqrt{\frac{5}{2}}}{\sqrt{\frac{5}{2}} \sqrt{3}} A = -\frac{\sqrt{\pi}}{a^5} \frac{1}{1 \cdot 2} A, A_6 = \frac{1}{a^6} \frac{2^2}{1 \cdot 3} A \frac{\sqrt{\frac{5}{2}} \sqrt{3}}{\sqrt{\frac{7}{2}}} = \frac{1}{a^6} \frac{2^3}{1 \cdot 3 \cdot 5} A$$

&c., &c., so that

$$y = A \left\{ 1 + \frac{2}{a^2 x} + \frac{2^2}{1 \cdot 3 a^4 x^2} + \frac{2^3}{1 \cdot 3 \cdot 5 a^6 x^3} + \&c. \right. \\ \left. - \sqrt{\pi} \left(\frac{1}{a \sqrt{x}} + \frac{1}{1 \cdot a^3 x^{\frac{3}{2}}} + \frac{1}{1 \cdot 2 a^5 x^{\frac{5}{2}}} + \&c. \right) \right\}$$

Let $y_1 = 1 + \frac{2}{a^2 x} + \frac{2^2}{1 \cdot 3 a^4 x^2} + \&c.$

then $\frac{d}{dx} (\sqrt{x} \cdot y_1) = \frac{1}{2 \sqrt{x}} - \frac{1}{a^2 x^{\frac{3}{2}}} - \&c. \\ = \frac{1}{2 \sqrt{x}} - \frac{1}{a^2 x^{\frac{3}{2}}} y_1$

or $\frac{d y_1}{d x} + \left(\frac{1}{2 x} + \frac{1}{a^2 x^2} \right) y_1 = \frac{1}{2 x}.$

Again, let
$$y_2 = \frac{1}{\sqrt{x}} + \frac{1}{1 \cdot \alpha^3 x^{\frac{3}{2}}} + \&c.$$

then
$$\begin{aligned} \frac{d}{dx} (\sqrt{x} y_2) &= -\frac{1}{1 \cdot \alpha^3 x^{\frac{3}{2}}} - \&c. \\ &= -\frac{1}{\alpha^2 x^{\frac{3}{2}}} y_2 \end{aligned}$$

or
$$\frac{dy_2}{dx} + \left(\frac{1}{2x} + \frac{1}{\alpha^2 x^{\frac{3}{2}}} \right) y_2 = 0$$

and
$$y = A \left(y_1 - \frac{\sqrt{\pi}}{\alpha} y_2 \right).$$

By solving the equations for y_1 and y_2 we obtain finally

$$y = \frac{A e^{\frac{1}{\alpha^2 x}}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\alpha^2 x}}}{\sqrt{x}} dx - \frac{\sqrt{\pi}}{\alpha} \right\}$$

The equations from which y_1 and y_2 are determined differ only in the term which does not contain y ; and it will be seen hereafter that similar equations serve to give the solution of the other differential equations of this class, when n is an integer. If $\alpha\sqrt{-1} = m$, these equations are

$$\begin{aligned} \frac{dy_1}{dx} + \left(\frac{1}{2x} - \frac{1}{m^2 x^2} \right) y_1 &= \frac{1}{2x} \\ \frac{dy_2}{dx} + \left(\frac{1}{2x} - \frac{1}{m^2 x^2} \right) y_2 &= 0. \end{aligned}$$

12. OTHERWISE. The following method of solving this equation has the advantage of not appearing to take for granted the form in which y is expressed in terms of x .

$$\begin{aligned} y - \alpha \sqrt{-1} x \frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} &= 0 \text{ gives } y = \frac{0}{1 - \alpha \sqrt{-1} x d^{\frac{1}{2}}} \\ &= \frac{1 + \alpha \sqrt{-1} x d^{\frac{1}{2}}}{1 + \alpha^2 x d^{\frac{1}{2}} x d^{\frac{1}{2}}} \cdot 0 \end{aligned}$$

Now $\frac{0}{1 + \alpha^2 x d^{\frac{1}{2}} x d^{\frac{1}{2}}}$ is the solution of the equation

$$v + \alpha^2 x d^{\frac{1}{2}} x d^{\frac{1}{2}} v = 0, \text{ or of}$$

$$\frac{v}{\alpha^2} + x^2 \frac{dv}{dx} + \frac{1}{2} x v = 0,$$

or of
$$\frac{dv}{dx} + \left(\frac{1}{2x} + \frac{1}{\alpha^2 x^2} \right) v = 0;$$

which is the equation for determining y_2 given above.

$$\therefore v = \frac{A}{\sqrt{x}} e^{\frac{1}{a^2 x}};$$

and

$$y = (1 + a\sqrt{-1}x d^{\frac{1}{2}})v \\ = \frac{A}{\sqrt{x}} e^{\frac{1}{a^2 x}} + a\sqrt{-1}x \frac{d^{\frac{1}{2}}v}{dx^{\frac{1}{2}}}$$

which will be seen to coincide with the solution already given.

This second method of solving the equation is by far the most simple and satisfactory, when once the principles of the calculus of operations are thoroughly mastered. For the purpose, however, of exhibiting the analogy amongst the differential equations which determine the values of the different series which make up a function satisfying the conditions $y - m x^2 \frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} = 0$, I shall employ the first method in the three following examples.

13. Ex. 4. $y - m x^{\frac{3}{2}} \frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} = 0.$

let $y = A_0 + \frac{A_1}{x} + \frac{A_2}{x^2} + \&c.$

then $\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} = (-1)^{\frac{1}{2}} \left\{ \frac{\sqrt{\frac{3}{2}}}{1} \frac{A_1}{x^{\frac{3}{2}}} + \frac{\sqrt{2}}{\sqrt{3}} \frac{A_2}{x^{\frac{5}{2}}} - \&c. \right\}$

and $A_0 + \frac{A_1}{x} + \frac{A_2}{x^2} + \&c. = m(-1)^{\frac{1}{2}} \left\{ \frac{\sqrt{\frac{3}{2}}}{1} A_1 + \frac{\sqrt{\frac{5}{2}}}{2} \frac{A_2}{x} + \&c. \right\}$

$$A_1 = \frac{\sqrt{1}}{\sqrt{\frac{3}{2}}} \frac{A_0}{m\sqrt{-1}}; \quad A_2 = \frac{\sqrt{2}}{\sqrt{\frac{5}{2}}} \frac{A_1}{m\sqrt{-1}}; \quad A_3 = \frac{\sqrt{3}}{\sqrt{\frac{7}{2}}} \frac{A_2}{m\sqrt{-1}} \&c.$$

or $A_1 = \frac{2}{1} \cdot \frac{A_0}{m\sqrt{-\pi}}; \quad A_2 = -\frac{2^2}{3 \cdot 1} \cdot \frac{2}{1} \frac{1 \cdot A_0}{m^2 \pi}$

$$A_3 = -\frac{2^3}{5 \cdot 3 \cdot 1} \cdot \frac{2^2}{3 \cdot 1 \cdot 1} \cdot \frac{2}{1} \frac{1 \cdot 2 \cdot A_0}{m^3 \pi \sqrt{-1}}; \quad A_4 = \frac{2^4}{7 \cdot 5 \cdot 3 \cdot 1} \cdot \frac{2^3}{5 \cdot 3 \cdot 1} \cdot \frac{2^2}{3 \cdot 1 \cdot 1} \cdot \frac{2}{1} \frac{1 \cdot 2 \cdot 3 \cdot A_0}{m^4 \pi^2}$$

&c. = &c.

and $y = A_0 \left\{ 1 + \frac{2}{1} \frac{1}{m x \sqrt{-\pi}} - \frac{2^2}{3 \cdot 1} \cdot \frac{2}{1} \cdot \frac{1}{m^2 x^2 \pi} - \frac{2^3}{5 \cdot 3 \cdot 1} \cdot \frac{2^2}{3 \cdot 1 \cdot 1} \cdot \frac{2}{1} \frac{1 \cdot 2}{m^3 x^3 \sqrt{-\pi}} \&c. \right\}$

Ex. 5. $y - m x^2 \frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} = 0.$

It is easily seen that the form of the series into which y may be expanded is this

$$y = A x + \frac{B}{\sqrt{x}} + \frac{C}{x^2} + \frac{D}{x^{\frac{5}{2}}} + \&c. \\ + a + \frac{\beta}{x^{\frac{3}{2}}} + \frac{\gamma}{x^3} + \frac{\delta}{x^{\frac{9}{2}}} + \&c.$$

and that the result of substitution is

$$\left. \begin{aligned} Ax + \frac{B}{\sqrt{x}} + \frac{C}{x^2} + \frac{D}{x^{\frac{3}{2}}} + \&c. \\ + a + \frac{\beta}{x^{\frac{3}{2}}} + \frac{\gamma}{x^3} + \frac{\delta}{x^{\frac{5}{2}}} + \&c. \end{aligned} \right\} = \begin{aligned} m\sqrt{-1} \left(\frac{1}{\sqrt{\frac{1}{2}}} Bx + \frac{\sqrt{\frac{5}{2}}}{2} \frac{C}{\sqrt{x}} + \frac{\sqrt{4}}{\sqrt{\frac{1}{2}}} \frac{D}{x^2} + \&c. \right) \\ + m\sqrt{-1} \left(\frac{2}{\sqrt{\frac{1}{2}}} \beta + \frac{\sqrt{\frac{7}{2}}}{3} \frac{\gamma}{x^{\frac{3}{2}}} + \frac{\sqrt{5}}{\sqrt{\frac{1}{2}}} \frac{\delta}{x^3} + \&c. \right) \end{aligned}$$

so that

$$B = \frac{\sqrt{\frac{1}{2}}}{1} \frac{A}{m\sqrt{-1}}; \quad C = \frac{\sqrt{2}}{\sqrt{\frac{5}{2}}} \frac{B}{m\sqrt{-1}} = -\frac{\sqrt{\frac{1}{2}}}{1} \frac{\sqrt{2}}{\sqrt{\frac{5}{2}}} \frac{1}{m^2}$$

$$D = \frac{\sqrt{\frac{7}{2}}}{4} \frac{C}{m\sqrt{-1}} = -\frac{\sqrt{\frac{1}{2}}}{1} \frac{\sqrt{2}}{\sqrt{\frac{5}{2}}} \frac{\sqrt{\frac{7}{2}}}{4} \frac{1}{m^3\sqrt{-1}}$$

$$E = \frac{\sqrt{\frac{1}{2}}}{1} \frac{\sqrt{2}}{\sqrt{\frac{5}{2}}} \frac{\sqrt{\frac{7}{2}}}{4} \frac{\sqrt{5}}{\sqrt{\frac{1}{2}}} \frac{1}{m^4} \&c.$$

and also

$$\beta = \frac{\sqrt{\frac{5}{2}}}{2} \frac{a}{m\sqrt{-1}}, \quad \gamma = -\frac{\sqrt{\frac{3}{2}}}{2} \frac{\sqrt{3}}{\sqrt{\frac{1}{2}}} \frac{a}{m^2}, \quad \delta = -\frac{\sqrt{\frac{3}{2}}}{2} \frac{\sqrt{3}}{\sqrt{\frac{1}{2}}} \frac{\sqrt{5}}{\sqrt{5}} \frac{a}{m^3\sqrt{-1}}$$

$$\epsilon = \frac{\sqrt{\frac{3}{2}}}{2} \frac{\sqrt{3}}{\sqrt{\frac{1}{2}}} \frac{\sqrt{5}}{\sqrt{\frac{1}{2}}} \frac{\sqrt{6}}{\sqrt{\frac{1}{2}}} \frac{a}{m^4} \&c.$$

$$\begin{aligned} \therefore y = A \left\{ x - \frac{1}{\frac{1}{2} \cdot \frac{3}{2} m^2 x^2} + \frac{1 \cdot 4}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} m^4 x^5} - \frac{1 \cdot 4 \cdot 7}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} \cdot \frac{13}{2} \cdot \frac{15}{2} m^6 x^8} + \&c. \right. \\ \left. + \frac{\sqrt{\pi}}{m\sqrt{-1}} \left(\frac{1}{\sqrt{x}} - \frac{\sqrt{\frac{5}{2}}}{2 \cdot 3 m^2 x^{\frac{3}{2}}} + \frac{\sqrt{\frac{5}{2}} \cdot \sqrt{\frac{1}{2}}}{2 \cdot 3 \cdot 5 \cdot 6 m^4 x^{\frac{5}{2}}} - \frac{\sqrt{\frac{5}{2}} \cdot \sqrt{\frac{1}{2}} \cdot \sqrt{\frac{7}{2}}}{2 \cdot 3 \cdot 5 \cdot 6 \cdot 8 \cdot 9 m^6 x^{\frac{7}{2}}} + \&c. \right) \right\} \\ + a \left\{ 1 - \frac{2}{\frac{3}{2} \cdot \frac{5}{2} m^2 x^3} + \frac{2 \cdot 5}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} m^4 x^6} - \frac{2 \cdot 5 \cdot 8}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} \cdot \frac{11}{2} \cdot \frac{13}{2} m^6 x^9} + \&c. \right. \\ \left. + \frac{\sqrt{\pi}}{m\sqrt{-1}} \left(\frac{\sqrt{\frac{1}{2}}}{x^{\frac{3}{2}}} - \frac{\sqrt{\frac{1}{2}} \cdot \sqrt{\frac{7}{2}}}{3 \cdot 4 m^2 x^{\frac{5}{2}}} + \frac{\sqrt{\frac{1}{2}} \cdot \sqrt{\frac{7}{2}} \cdot \sqrt{\frac{13}{2}}}{3 \cdot 4 \cdot 6 \cdot 7 m^4 x^{\frac{7}{2}}} - \&c. \right) \right\} \end{aligned}$$

Each of these four series is the integral of a differential equation of the second order.

$$\text{Let} \quad \frac{dy_1}{dx} = x - \frac{1}{\frac{1}{2} \cdot \frac{3}{2} m^2 x^2} + \frac{1 \cdot 4}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} m^4 x^5} - \&c.$$

$$\text{then} \quad y_1 = \frac{x^2}{2} + \frac{1}{\frac{1}{2} \cdot \frac{3}{2} m^2 x} - \&c.$$

$$\text{and} \quad \frac{d^2 \sqrt{x} y_1}{dx^2} = \frac{5 \cdot 3}{2 \cdot 2} \frac{x^{\frac{1}{2}}}{2} + \frac{1}{m^2 x^{\frac{5}{2}}} - \frac{1}{\frac{1}{2} \cdot \frac{3}{2} m^4 x^{\frac{7}{2}}} + \&c.$$

$$= \frac{15}{8} \sqrt{x} + \frac{1}{m^2 x^{\frac{5}{2}}} \frac{dy_1}{dx}$$

$$\text{or} \quad \frac{d^2 y_1}{dx^2} + \left(\frac{1}{x} - \frac{1}{m^2 x^4} \right) \frac{dy_1}{dx} - \frac{y_1}{4 x^2} = \frac{15}{8}$$

Again, let
$$\frac{dy_2}{dx} = \frac{1}{\sqrt{x}} - \frac{\frac{5}{2}}{2 \cdot 3 m^2 x^{\frac{7}{2}}} + \frac{\frac{5}{2} \cdot \frac{11}{2}}{2 \cdot 3 \cdot 5 \cdot 6 m^4 x^{\frac{9}{2}}} - \&c.$$

then
$$y_2 = 2\sqrt{x} + \frac{1}{2 \cdot 3 m^2 x^{\frac{5}{2}}} - \frac{\frac{5}{2}}{2 \cdot 3 \cdot 5 \cdot 6 m^4 x^{\frac{7}{2}}} + \&c.$$

and
$$\begin{aligned} \frac{d^2 \sqrt{x} y_2}{dx^2} &= \frac{1}{m^2 x^4} - \frac{\frac{5}{2}}{2 \cdot 3 m^4 x^7} + \&c. \\ &= \frac{1}{m^2 x^{\frac{7}{2}}} \frac{dy_2}{dx} \end{aligned}$$

$$\therefore \frac{d^2 y_2}{dx^2} + \left(\frac{1}{x} - \frac{1}{m^2 x^4} \right) \frac{dy_2}{dx} - \frac{y_2}{4x^2} = 0$$

Also let
$$\frac{dy_3}{dx} = 1 - \frac{2}{\frac{3}{2} \cdot \frac{5}{2} m^2 x^3} + \frac{2 \cdot 5}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} m^4 x^5} - \&c.$$

then
$$y_3 = x + \frac{1}{\frac{3}{2} \cdot \frac{5}{2} m^2 x^2} - \frac{2}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} m^4 x^4} + \&c.$$

and
$$\begin{aligned} \frac{d^2 \sqrt{x} y_3}{dx^2} &= \frac{3}{2} \frac{1}{2\sqrt{x}} + \frac{1}{m^2 x^{\frac{7}{2}}} - \frac{2}{\frac{3}{2} \cdot \frac{5}{2} m^4 x^{\frac{9}{2}}} + \&c. \\ &= \frac{3}{4\sqrt{x}} + \frac{1}{m^2 x^{\frac{7}{2}}} \frac{dy_3}{dx} \end{aligned}$$

or
$$\frac{d^2 y_3}{dx^2} + \left(\frac{1}{x} - \frac{1}{m^2 x^4} \right) \frac{dy_3}{dx} - \frac{y_3}{4x^2} = \frac{3}{4x}$$

Lastly, let
$$\frac{dy_4}{dx} = \frac{\frac{1}{2}}{x^{\frac{3}{2}}} - \frac{\frac{1}{2} \cdot \frac{7}{2}}{3 \cdot 4 m^2 x^{\frac{5}{2}}} + \frac{\frac{1}{2} \cdot \frac{7}{2} \cdot \frac{13}{2}}{3 \cdot 4 \cdot 6 \cdot 7 m^4 x^{\frac{7}{2}}} + \&c.$$

then
$$y_4 = -\frac{1}{x^{\frac{1}{2}}} + \frac{\frac{1}{2}}{3 \cdot 4 m^2 x^{\frac{3}{2}}} - \frac{\frac{1}{2} \cdot \frac{7}{2}}{3 \cdot 4 \cdot 6 \cdot 7 m^4 x^{\frac{5}{2}}} + \&c.$$

and
$$\begin{aligned} \frac{d^2 \sqrt{x} y_4}{dx^2} &= \frac{\frac{1}{2}}{m^2 x^5} - \frac{\frac{1}{2} \cdot \frac{7}{2}}{3 \cdot 4 m^4 x^8} + \&c. \\ &= \frac{1}{m^2 x^{\frac{7}{2}}} \frac{dy_4}{dx} \end{aligned}$$

or
$$\frac{d^2 y_4}{dx^2} + \left(\frac{1}{x} - \frac{1}{m^2 x^4} \right) \frac{dy_4}{dx} - \frac{y_4}{4x^2} = 0$$

Having found y_1, y_2, y_3, y_4 from these equations, we obtain

$$y = A \left(\frac{dy_1}{dx} + \frac{\sqrt{\pi}}{m\sqrt{-1}} \frac{dy_2}{dx} \right) + \alpha \left(\frac{dy_3}{dx} + \frac{\sqrt{\pi}}{m\sqrt{-1}} \frac{dy_4}{dx} \right)$$

The remarkable similarity between the equations which determine y_1, y_2, y_3, y_4 leads us to conclude that the form of this function is common to all similar equations. It may be seen that the equations for y_2 and y_4 are identical: the arbitrary constants must, however, be determined differently in the two: the one function vanishes when $x=\infty$, the other does not. By solving the equations in a more general form, and by a more purely symbolical method, we shall be able to

see the reason of this analogy. We shall, in Example 7, exhibit a complete and general solution of all equations of this form.

Ex. 6. $y - m x^3 \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} = 0$

Let $y = A x^2 + B x + C + \frac{A_1}{x^{\frac{1}{2}}} + \frac{B_1}{x^{\frac{3}{2}}} + \frac{C_1}{x^{\frac{5}{2}}} + \frac{A_2}{x^3} + \frac{B_2}{x^{\frac{7}{2}}} + \frac{C_2}{x^{\frac{9}{2}}} + \frac{A_3}{x^{\frac{13}{2}}} + \&c.$

then $A x^2 + B x + C + \frac{A_1}{x^{\frac{1}{2}}} + \&c. = m \sqrt{-1} \left(\frac{1}{\frac{1}{2}} x^2 A_1 + \&c. \right)$

$$A_1 = \frac{A}{m \sqrt{-1}} \frac{1}{1^{\frac{1}{2}}}, A_2 = \frac{A_1}{m \sqrt{-1}} \frac{1}{3^{\frac{1}{2}}}, A_3 = \frac{A_2}{m \sqrt{-1}} \frac{1}{5^{\frac{1}{2}}} \&c.$$

$$B_1 = \frac{B}{m \sqrt{-1}} \frac{1}{2^{\frac{1}{2}}}, B_2 = \frac{B_1}{m \sqrt{-1}} \frac{1}{4^{\frac{1}{2}}}, B_3 = \frac{B_2}{m \sqrt{-1}} \frac{1}{6^{\frac{1}{2}}} \&c.$$

$$C_1 = \frac{C}{m \sqrt{-1}} \frac{1}{3^{\frac{1}{2}}}, C_2 = \frac{C_1}{m \sqrt{-1}} \frac{1}{5^{\frac{1}{2}}}, C_3 = \frac{C_2}{m \sqrt{-1}} \frac{1}{7^{\frac{1}{2}}} \&c.$$

This gives us six separate series.

1°. $A x^2 + \frac{A_2}{x^3} + \frac{A_4}{x^8} + \&c. = A \left(x^2 - \frac{1 \cdot 2}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} m^2 x^3} + \frac{1 \cdot 2 \cdot 6 \cdot 7}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} \cdot \frac{11}{2} \cdot \frac{13}{2} \cdot \frac{15}{2} m^4 x^8} + \&c. \right)$

Let $\frac{d^2 y_1}{d x^2} = x^2 - \frac{1 \cdot 2}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} m^2 x^3} + \&c.$

then $y_1 = \frac{x^4}{3 \cdot 4} - \frac{1}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} m^2 x} + \frac{1 \cdot 2}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} \cdot \frac{11}{2} \cdot \frac{13}{2} \cdot \frac{15}{2} m^4 x^6} - \&c.$

$$\frac{d^3 \sqrt{x} \cdot y_1}{d x^3} = \frac{\frac{3}{2} \cdot \frac{7}{2} \cdot \frac{5}{2} x^{\frac{3}{2}}}{3 \cdot 4} + \frac{1}{m^2 x^{\frac{7}{2}}} - \frac{1 \cdot 2}{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} m^4 x^{\frac{13}{2}}} + \&c.$$

$$= \frac{105}{32} x^{\frac{3}{2}} + \frac{1}{m^2 x^{\frac{7}{2}}} \frac{d^2 y_1}{d x^2}$$

or $\frac{d^3 y_1}{d x^3} + \frac{3}{2 x} \frac{d^2 y_1}{d x^2} - \frac{3}{4 x^2} \frac{d y_1}{d x} + \frac{3}{8 x^3} y_1 = \frac{105}{32} x + \frac{1}{m^2 x^6} \frac{d^2 y_1}{d x^2}$

or $\frac{d^3 y_1}{d x^3} + \left(\frac{3}{2 x} - \frac{1}{m^2 x^6} \right) \frac{d^2 y_1}{d x^2} - \frac{3}{4 x^2} \frac{d y_1}{d x} + \frac{3}{8 x^3} y_1 = \frac{105}{32} x$

2°. $\frac{A_1}{x^{\frac{1}{2}}} + \frac{A_3}{x^{\frac{13}{2}}} + \&c.,$ gives $\frac{\sqrt{\pi}}{m \sqrt{-1}} A \left(\frac{1}{x^{\frac{1}{2}}} - \frac{\frac{7}{2} \cdot \frac{9}{2}}{3 \cdot 4 \cdot 5 m^2 x^{\frac{13}{2}}} + \frac{\frac{7}{2} \cdot \frac{9}{2} \cdot \frac{11}{2} \cdot \frac{13}{2}}{3 \cdot 4 \cdot 5 \cdot 8 \cdot 9 \cdot 10 m^2 x^{\frac{21}{2}}} + \&c. \right)$

Let $\frac{d^2 y_2}{d x^2} = \frac{1}{x^{\frac{1}{2}}} - \frac{\frac{7}{2} \cdot \frac{9}{2}}{3 \cdot 4 \cdot 5 m^2 x^{\frac{13}{2}}} + \&c.$

$$\text{then } y_2 = 2 \cdot \frac{2}{3} x^{\frac{3}{2}} - \frac{1}{3 \cdot 4 \cdot 5 m^2 x^{\frac{1}{2}}} + \frac{\frac{7}{2} \cdot \frac{3}{2}}{3 \cdot 4 \cdot 5 \cdot 8 \cdot 9 \cdot 10 m^4 x^{\frac{1}{2}}} - \&c.$$

$$\begin{aligned} \frac{d^3 y_2 \sqrt{x}}{d x^3} &= \frac{1}{m^2 x^{\frac{5}{2}}} - \frac{\frac{7}{2} \cdot \frac{3}{2}}{3 \cdot 4 \cdot 5 m^4 x^{\frac{11}{2}}} + \&c. \\ &= \frac{1}{m^2 x^{\frac{1}{2}}} \frac{d^2 y_2}{d x^2} \end{aligned}$$

$$\therefore \frac{d^3 y_2}{d x^3} + \left(\frac{3}{2x} - \frac{1}{m^2 x^6} \right) \frac{d^2 y_2}{d x^2} - \frac{3}{4 x^2} \frac{d y_2}{d x} + \frac{3}{8 x^3} y_2 = 0$$

$$\begin{aligned} 3^\circ. \quad Bx + \frac{B_2}{x^4} + \frac{B_3}{x^9} + \&c. &= B \left(x - \frac{2 \cdot 3}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} m^2 x^4} + \frac{2 \cdot 3 \cdot 7 \cdot 8}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} \cdot \frac{1}{2} \cdot \frac{5}{2} \cdot \frac{1}{2} \cdot \frac{1}{2} m^4 x^9} + \&c. \right) \\ &= B \frac{d^2 y_3}{d x^2} \text{ suppose} \end{aligned}$$

$$\text{then } y_3 = \frac{x^3}{2 \cdot 3} - \frac{1}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} m^2 x^2} + \frac{2 \cdot 3}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} \cdot \frac{1}{2} \cdot \frac{5}{2} \cdot \frac{1}{2} \cdot \frac{1}{2} m^4 x^7} - \&c.$$

$$\begin{aligned} \frac{d^3 \sqrt{x} y_3}{d x^3} &= \frac{\frac{7}{2} \cdot \frac{5}{2} \cdot \frac{3}{2} x^{\frac{1}{2}}}{2 \cdot 3} + \frac{1}{m^2 x^{\frac{9}{2}}} - \frac{2 \cdot 3}{\frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2} m^4 x^{\frac{19}{2}}} + \&c. \\ &= \frac{35}{16} x^{\frac{1}{2}} + \frac{1}{m^2 x^{\frac{1}{2}}} \frac{d^2 y_3}{d x^2} \end{aligned}$$

$$\therefore \frac{d^3 y_3}{d x^3} + \left(\frac{3}{2x} - \frac{1}{m^2 x^6} \right) \frac{d^2 y_3}{d x^2} - \frac{3}{4 x^2} \frac{d y_3}{d x} + \frac{3}{8 x^3} y_3 = \frac{35}{16}$$

$$\begin{aligned} 4^\circ. \quad \frac{B_1}{x^{\frac{1}{2}}} + \frac{B_3}{x^{\frac{9}{2}}} + \frac{B_5}{x^{\frac{25}{2}}} + \&c. &= \frac{B \sqrt{\pi}}{m \sqrt{-1}} \left(\frac{1}{x^{\frac{1}{2}}} - \frac{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2}}{4 \cdot 5 \cdot 6 m^2 x^{\frac{13}{2}}} + \&c. \right) \\ &= \frac{\sqrt{\pi}}{m \sqrt{-1}} B_1 \frac{d^2 y_4}{d x^2} \text{ suppose} \end{aligned}$$

$$\text{then } y_4 = -2 x^{\frac{1}{2}} - \frac{\frac{1}{2}}{4 \cdot 5 \cdot 6 m^2 x^{\frac{9}{2}}} + \&c.$$

$$\frac{d^3 \sqrt{x} y_4}{d x^3} = \frac{1}{m^2 x^{\frac{1}{2}}} - \&c. = \frac{1}{m^2 x^{\frac{1}{2}}} y_4 \text{ the same equation as for } y_2.$$

$$\begin{aligned} 5^\circ. \quad C + \frac{C_2}{x^6} + \frac{C_4}{x^{10}} + \&c. &= C \left(1 - \frac{3 \cdot 4}{\frac{5}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} m^2 x^6} + \&c. \right) \\ &= C \frac{d^2 y_5}{d x^2} \end{aligned}$$

$$\therefore y_5 = \frac{x^2}{1 \cdot 2} - \frac{1}{\frac{5}{2} \cdot \frac{7}{2} \cdot \frac{9}{2} m^2 x^3} + \&c.$$

$$\begin{aligned} \frac{d^3 \sqrt{x} y_5}{d x^3} &= \frac{5}{2} \cdot \frac{3}{2} \cdot \frac{1}{2 \sqrt{x}} + \frac{1}{m^2 x^{\frac{11}{2}}} - \&c. \\ &= \frac{15}{8 \sqrt{x}} + \frac{1}{m^2 x^{\frac{1}{2}}} \frac{d^2 y_5}{d x^2} \end{aligned}$$

$$\frac{d^3 y_5}{d x^3} + \left(\frac{3}{2x} - \frac{1}{m^2 x^6} \right) \frac{d^2 y_5}{d x^2} - \frac{3}{4 x^2} \frac{d y_5}{d x} + \frac{3}{8 x^3} y_5 = \frac{15}{8x}$$

$$6^{\circ}. \quad \frac{C_1}{x^{\frac{1}{2}}} + \frac{C_3}{x^{\frac{3}{2}}} + \&c. = \frac{\sqrt{\pi} C}{m\sqrt{-1}} \left(\frac{\frac{1}{2} \cdot \frac{3}{2}}{2 x^{\frac{5}{2}}} - \frac{\frac{1}{2} \cdot \frac{3}{2} \cdot \frac{5}{2} \cdot \frac{7}{2}}{2 \cdot 5 \cdot 6 \cdot 7} \frac{1}{m^2 x^{\frac{9}{2}}} + \&c. \right) \\ = \frac{\sqrt{\pi} C}{m\sqrt{-1}} \frac{d^2 y_6}{d x^2}$$

$$\therefore y_6 = \frac{1}{2\sqrt{x}} - \frac{\frac{1}{2} \cdot \frac{3}{2}}{2 \cdot 5 \cdot 6 \cdot 7} \frac{1}{m^2 x^{\frac{1}{2}}} + \&c.$$

$$\frac{d^3 \sqrt{x} y_6}{d x^3} = \frac{\frac{1}{2} \cdot \frac{3}{2}}{2 m^2 x^{\frac{1}{2}}} - \&c.$$

$$= \frac{1}{m^2 x^{\frac{1}{2}}} \frac{d^2 y_6}{d x^2} \text{ the same as } y_2 ;$$

$$\text{and } y = A \left(\frac{d^2 y_1}{d x^2} + \frac{\sqrt{\pi}}{m\sqrt{-1}} \frac{d^2 y_2}{d x^2} \right) + B \left(\frac{d^2 y_3}{d x^2} + \frac{\sqrt{\pi}}{m\sqrt{-1}} \frac{d^2 y_4}{d x^2} \right) \\ + C \left(\frac{d^2 y_5}{d x^2} + \frac{\sqrt{\pi}}{m\sqrt{-1}} \frac{d^2 y_6}{d x^2} \right)$$

It is scarcely necessary to point out the analogy which exists between the differential equations which determine the value of the transcendentals in this and in the preceding examples.

14. We proceed now to exhibit a general solution of equations of this kind.

$$\text{Ex. 7. } y - m x^n \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} = 0 ; n \text{ being any integer.}$$

The symbolical form of this equation is

$$y = \frac{1}{1 - m x^n d^{\frac{1}{2}}} \cdot 0 = \frac{1 + m x^n d^{\frac{1}{2}}}{1 - m^2 x^n d^{\frac{1}{2}} x^n d^{\frac{1}{2}}} 0 \\ = (1 + m x^n d^{\frac{1}{2}}) v = v + m x^n \frac{d^{\frac{1}{2}} v}{d x^{\frac{1}{2}}} \quad (1)$$

where v is determined by the equation

$$\frac{1}{1 - m^2 x^n d^{\frac{1}{2}} x^n d^{\frac{1}{2}}} 0 = v ; \text{ or}$$

$$v - m^2 x^n d^{\frac{1}{2}} x^n d^{\frac{1}{2}} v = 0 \text{ or } v - m^2 x^n \frac{d^{\frac{1}{2}}}{d x^{\frac{1}{2}}} \left(x^n \frac{d^{\frac{1}{2}} v}{d x^{\frac{1}{2}}} \right) = 0 \quad (2)$$

$$\text{Let } v = \frac{d^{n-1} z}{d x^{n-1}} ; \text{ then } \frac{d^{\frac{1}{2}} v}{d x^{\frac{1}{2}}} = \frac{d^{n-\frac{1}{2}} z}{d x^{n-\frac{1}{2}}}.$$

$$\therefore \frac{d^{\frac{1}{2}}}{d x^{\frac{1}{2}}} \left(x^n \frac{d^{\frac{1}{2}} v}{d x^{\frac{1}{2}}} \right) = x^n \frac{d^n z}{d x^n} + \frac{1}{2} n x^{n-1} \frac{d^{n-1} z}{d x^{n-1}}$$

$$- \frac{1 \cdot 1}{2 \cdot 4} n(n-1) x^{n-2} \frac{d^{n-2} z}{d x^{n-2}} + \&c. + (-1)^{n-1} \frac{1 \cdot 1 \cdot 3 \dots (2n-3)}{2 \cdot 4 \dots 2n} n(n-1) \dots 1 \cdot z$$

(Part I. Art. 11.)

By substituting this in equation (2) we obtain

$$\frac{d^{n-1}z}{dx^{n-1}} - m^2 x^n \left(x^n \frac{d^n z}{dx^n} + \frac{1}{2} n x^{n-1} \frac{d^{n-1}z}{dx^{n-1}} + \&c. \right) = 0$$

or

$$\begin{aligned} \frac{d^n z}{dx^n} + \left(\frac{n}{2x} - \frac{1}{m^2 x^{2n}} \right) \frac{d^{n-1}z}{dx^{n-1}} - \frac{1 \cdot 1}{2 \cdot 4} \frac{n(n-1)}{x^2} \frac{d^{n-2}z}{dx^{n-2}} \\ + \frac{1 \cdot 1 \cdot 3}{2 \cdot 4 \cdot 6} \frac{n(n-1)(n-2)}{x^3} \frac{d^{n-3}z}{dx^{n-3}} - \&c. + (-1)^{n-1} \frac{1 \cdot 1 \cdot 3 \dots (2n-3)}{2 \cdot 4 \cdot 6 \dots 2n} \cdot \\ \cdot \frac{n(n-1) \dots 1}{x^n} z = 0 \end{aligned}$$

When z has been determined from this equation, we shall have the complete value of y by means of Equation (1.), viz.

$$y = \frac{d^{n-1}z}{dx^{n-1}} + m x^n \frac{d^{n-\frac{1}{2}}z}{dx^{n-\frac{1}{2}}}$$

COR. If $n=3$;

$$\frac{d^3 z}{dx^3} + \left(\frac{3}{2x} - \frac{1}{m^2 x^6} \right) \frac{d^2 z}{dx^2} - \frac{3}{4x^2} \frac{dz}{dx} + \frac{3}{8x^3} z = 0;$$

which is the same equation as that which we obtained by a totally different process for determining y_2 and y_4 in Ex. 6.

Ex. 8.

$$y - m x^n \frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} = X$$

The solution is

$$\begin{aligned} y = \frac{1}{1 - m x^n d^{\frac{1}{2}}} (X + 0) &= \frac{1 + m x^n d^{\frac{1}{2}}}{1 - m^2 x^n d^{\frac{1}{2}} x^n d^{\frac{1}{2}}} (X + 0) \\ &= (1 + m x^n d^{\frac{1}{2}}) (v + w) \\ &= v + m x^n \frac{d^{\frac{1}{2}}v}{dx^{\frac{1}{2}}} + w + m x^n \frac{d^{\frac{1}{2}}w}{dx^{\frac{1}{2}}} \end{aligned}$$

where r is the same as in the last Example, and w is determined from the equation

$$w - m^2 x^n \frac{d^{\frac{1}{2}}}{dx^{\frac{1}{2}}} \left(x^n \frac{d^{\frac{1}{2}}w}{dx^{\frac{1}{2}}} \right) = X$$

or by writing $\frac{d^{n-1}u}{dx^{n-1}}$ for w , and proceeding as in the last Example,

$$\frac{d^{n-1}u}{dx^{n-1}} - m^2 x^n \left(x^n \frac{d^n u}{dx^n} + \frac{n x^{n-1}}{2} \frac{d^{n-1}u}{dx^{n-1}} + \&c. \right) = X, \text{ or}$$

$$\frac{d^n u}{d x^n} + \left(\frac{n}{2x} - \frac{1}{m^2 x^2} \right) \frac{d^{n-1} u}{d x^{n-1}} - \frac{1 \cdot 1}{2 \cdot 4} \frac{n(n-1)}{x^2} \frac{d^{n-2} u}{d x^{n-2}} + \&c.$$

$$+ (-1)^{n-1} \frac{1 \cdot 1 \cdot 3 \dots (2n-3)}{2 \cdot 4 \cdot 6 \dots 2n} \cdot \frac{n(n-1) \dots 1}{x^n} u = -\frac{X}{m^2 x^{2n}}$$

COR. 1. If $n=1$, the equation for determining u is

$$\frac{du}{dx} + \left(\frac{1}{2x} - \frac{1}{m^2 x^2} \right) u = -\frac{X}{m^2 x^2},$$

of which the solution is

$$u = -\frac{e^{-\frac{1}{m^2 x}}}{\sqrt{x}} \int \frac{1}{m^2 x^{\frac{3}{2}}} X dx = v$$

$$\therefore v = \frac{A e^{-\frac{1}{m^2 x}}}{\sqrt{x}}$$

and

$$y = \left(1 + m x \frac{d^{\frac{1}{2}}}{d x^{\frac{1}{2}}} \right) \left(\frac{A e^{-\frac{1}{m^2 x}}}{\sqrt{x}} - \frac{e^{-\frac{1}{m^2 x}}}{\sqrt{x}} \int \frac{1}{m^2 x^{\frac{3}{2}}} X dx \right)$$

COR. 2. If $n=1$, $X = \frac{b}{\sqrt{x}}$, it is evident that

$$u = \frac{b}{\sqrt{x}} \quad \therefore y = y_0 + \frac{b}{\sqrt{x}} + \frac{m b \sqrt{-1}}{\sqrt{\pi}}$$

where y_0 is the solution of the equation without X (Ex. 3.)

It appears, therefore, that the complete solution of equations of this form is reduced to the solution of ordinary linear equations, and the determination of the half differential coefficient of the results.

Ex. 9. $y - m x^n \frac{d^{r+\frac{1}{2}} y}{d x^{r+\frac{1}{2}}} = X$, where n and r are any whole numbers.

We have
$$y = \frac{X+0}{1-m x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}}} = \frac{1+m x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}}}{1-m^2 x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}} x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}}} \cdot (X+0)$$

$$= \left(1 + m x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}} \right) (v+w) \dots \dots (1)$$

where $v+w$ is the solution of the equation

$$\left(1 - m^2 x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}} x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}} \right) (v+w) = X+0.$$

Now

$$\frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}} x^n \frac{d^{r+\frac{1}{2}}}{d x^{r+\frac{1}{2}}} v = x^n \frac{d^{2r+1}}{d x^{2r+1}} v + (r+\frac{1}{2}) n x^{n+1} \frac{d^{2r}}{d x^{2r}} v$$

$$+ \frac{(r+\frac{1}{2})(r-\frac{1}{2})}{1 \cdot 2} n(n-1) x^{n-2} \frac{d^{2r-1}}{d x^{2r-1}} v - \&c.$$

$$\begin{aligned}
& + \frac{(r+\frac{1}{2})(r-\frac{1}{2}) \dots n(n-1) \dots 1}{1 \cdot 2 \dots n} \cdot \frac{d^{2r-n+1} v}{d x^{2r-n+1}} \\
& = x^n \frac{d^n z}{d x^n} + (r+\frac{1}{2}) n x^{n-1} \frac{d^{n-1} z}{d x^{n-1}} + \dots \\
& + \frac{(r+\frac{1}{2})(r-\frac{1}{2}) \dots (r-2n+\frac{5}{2})}{1 \cdot 2 \dots n} \cdot n(n-1) \dots 1 \cdot z
\end{aligned}$$

where

$$v = \frac{d^{n-(2r+1)} z}{d x^{n-(2r+1)}}$$

∴ the equation for determining v is

$$\begin{aligned}
& \frac{d^n z}{d x^n} + \frac{(r+\frac{1}{2}) n}{x} \frac{d^{n-1} z}{d x^{n-1}} + \frac{(r+\frac{1}{2})(r-\frac{1}{2})}{1 \cdot 2} \cdot \frac{n(n-1)}{x^2} \frac{d^{n-2} z}{d x^{n-2}} + \dots \\
& + \frac{(r+\frac{1}{2})(r-\frac{1}{2}) \dots (r-2n+\frac{5}{2})}{1 \cdot 2 \dots n} \cdot n(n-1) \dots 1 \cdot z \\
& - \frac{1}{m^2 x^{2n}} \frac{d^{n-(2r+1)} v}{d x^{n-(2r+1)}} = - \frac{1}{m^2 x^{2n}} X \dots \dots (2)
\end{aligned}$$

n is the particular value of v corresponding with $X=0$. Having thus obtained v and w , equation (1) gives the complete value of y . It must be observed, that the transformation from v to z is only to be made when n is greater than $2r+1$.

CLASS III. *Equations which are capable of solution by transformation, without division of operations.*

15. Ex. 1. $y - m x^{\frac{1}{2}} \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} = 0$

By (C) this equation is transformed into

$$\begin{aligned}
& y - m(-1)^{\frac{1}{2}} \frac{\sqrt{-D + \frac{3}{2}}}{\sqrt{-D}} y = 0, \text{ or} \\
& y = \left(1 + m\sqrt{-1} \frac{\sqrt{-D + \frac{3}{2}}}{\sqrt{-D}}\right)^{-1} \cdot 0.
\end{aligned}$$

Hence, as in Ex. 1, Class 2, the value of y is $y = \frac{A}{x^n}$, where n is determined

by the equation $1 + m\sqrt{-1} \frac{\sqrt{n + \frac{3}{2}}}{\sqrt{n}} = 0$.

COR. If $m = -\frac{1}{3\sqrt{\pi}\sqrt{-1}}$, $\sqrt{n + \frac{3}{2}} = \frac{3}{2} \cdot \frac{1}{2}\sqrt{\pi}\sqrt{n} = \sqrt{\frac{5}{2}}\sqrt{n}$

∴ $n=1$; and $y = \frac{A}{x}$.

Ex. 2. $y - m x^{\frac{3}{2}} \frac{d^{\frac{3}{2}} y}{d x^{\frac{3}{2}}} = X.$

This gives
$$y = \frac{A}{x^n} + \left(1 + m \sqrt{-1} \frac{\sqrt{-D + \frac{3}{2}}}{\sqrt{-D}}\right)^{-1} \cdot X$$
$$= \frac{A}{x^n} + \Sigma b_r \left(1 + m \sqrt{-1} \frac{\sqrt{r + \frac{3}{2}}}{\sqrt{r}}\right)^{-1} e^{-r\theta}$$

COR. If $r=n$, this expression must be reduced, as in Ex. 2, Class 2, to

$$y = \frac{A}{x^n} + \frac{b_r}{m \sqrt{-1}} \frac{\log x}{x^n} \frac{1}{\frac{d}{dn} \frac{\sqrt{n + \frac{3}{2}}}{\sqrt{n}}} + \Sigma b_s \left(1 + m \sqrt{-1} \frac{\sqrt{s + \frac{3}{2}}}{\sqrt{s}}\right)^{-1} \cdot \frac{1}{x^s}$$

COR. 2. As a particular case, the solution of

$$\frac{d^{\frac{3}{2}} y}{d x^{\frac{3}{2}}} + \frac{3}{4} \sqrt{-1} \sqrt{\pi} \frac{y}{x^{\frac{3}{2}}} = \frac{b}{x^{\frac{7}{2}}}$$
$$y = \frac{A}{x} - \frac{8b}{9 \sqrt{-1} \sqrt{\pi}} \cdot \frac{1}{x^2}$$

These equations might have been included in the preceding Class, to which, both in their form and in the mode of their solution, they are very analogous. They are, however, particular cases of Example 5, below, which does not belong to that Class.

Ex. 3. $y + a \sqrt{x} \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} + b x \frac{dy}{dx} = 0.$

The equation in θ is (by C),

$$y + a \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}} y - b \frac{\sqrt{-D + 1}}{\sqrt{-D}} \cdot y = 0$$

or
$$\left\{1 + a \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}} - b \frac{\sqrt{-D + 1}}{\sqrt{-D}}\right\} \cdot y = 0$$

Suppose $y = \Sigma a_n e^{-n\theta}$; then

$$\Sigma a_n \left\{1 + a \sqrt{-1} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}} - b \frac{\sqrt{n + 1}}{\sqrt{n}}\right\} \cdot e^{-n\theta} = 0 \text{ by (A)}$$

Hence any value of n which will satisfy the equation

$$1 + a \sqrt{-1} \frac{\sqrt{n + \frac{1}{2}}}{\sqrt{n}} - b \frac{\sqrt{n + 1}}{\sqrt{n}} = 0$$

will give a term in the solution.

COR. 1. If $a \sqrt{-1} = -\frac{2\sqrt{\pi}}{4-\pi}$, $b = \frac{4-2\pi}{4-\pi}$ we have

$$4 - \pi - n(4 - 2\pi) - 2\sqrt{\pi} \frac{n + \frac{1}{2}}{\sqrt{n}} = 0$$

which is satisfied by $n = \frac{1}{2}$ and $n = 1$.

Hence
$$y = \frac{A}{\sqrt{x}} + \frac{B}{x}.$$

COR. 2. If n be a whole number r ; $\sqrt{n} = 1.2 \dots (r-1)$

$$\text{and } \sqrt{n + \frac{1}{2}} = \frac{1}{2} \cdot \frac{3}{2} \dots (r - \frac{1}{2}) \sqrt{\pi}$$

$$\therefore 1.2 \dots (r-1) + a\sqrt{-1} \sqrt{\pi} \frac{1.3 \dots (2r-1)}{2^r} - b.1.2 \dots r = 0;$$

will determine the integral values of n .

If $n = r + \frac{1}{2}$, $\sqrt{n} = \frac{1.3 \dots (2r-1)}{2^r} \sqrt{\pi}$, $\sqrt{n + \frac{1}{2}} = 1.2 \dots r$

and
$$\frac{1.3 \dots (2r-1)}{2^r} \sqrt{\pi} + a\sqrt{-1}.1.2 \dots r - b \frac{1.3 \dots (2r+1)}{2^{r+1}} \sqrt{\pi} = 0,$$

which determines the fractional values of n which have 2 as their denominator.

Now it is evident that these are the only forms which n can assume; therefore the determination of the values of n is reduced to the solution of these two equations.

EX. 4.
$$y + a\sqrt{x} \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} + b x \frac{dy}{dx} = X.$$

Let $X = \sum b_r e^{-r\theta}$, then

$$\begin{aligned} y &= \sum a_n e^{-n\theta} + \sum b_r \left(1 + a\sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{-D} - b \frac{\sqrt{-D + 1}}{-D} \right)^{-1} \cdot e^{-r\theta} \\ &= \sum a_n e^{-n\theta} + \sum \frac{b_r x^{-r}}{1 + a\sqrt{-1} \frac{\sqrt{r + \frac{1}{2}}}{r} - b \frac{\sqrt{r + 1}}{r}} \text{ by (A)} \end{aligned}$$

the values of n being determined as in Example 3.

COR. If $r = p$, $n = p$, we obtain, as in other instances,

$$y = \sum \frac{a_n}{x^n} + C \frac{\log x}{x^p} + \sum \frac{b_s x^{-s}}{1 - a\sqrt{-1} \frac{\sqrt{s + \frac{1}{2}}}{\sqrt{s}} - b \frac{\sqrt{s + 1}}{\sqrt{s}}}$$

where

$$C = - \frac{1}{a\sqrt{-1} \frac{d}{d_p} \frac{\sqrt{p + \frac{1}{2}}}{\sqrt{p}} - b}$$

EX. 5.
$$x^m \frac{d^m y}{d x^m} + a x^{m-\frac{1}{2}} \frac{d^{m-\frac{1}{2}} y}{d x^{m-\frac{1}{2}}} + \&c. = X.$$

The equation in θ is

$$\left\{ (-1)^m \frac{\sqrt{-D+m}}{\sqrt{-D}} + a (-1)^{m-\frac{1}{2}} \frac{\sqrt{-D+m-\frac{1}{2}}}{\sqrt{-D}} + \&c. \right\} y = X$$

which may be written $f(-D)y = X$;

and

$$y = \{f(-D)\}^{-1} \cdot 0 + \{f(-D)\}^{-1} \cdot X$$

$$= \sum a_n x^{-n} + \sum \frac{b_r x^{-r}}{f(r)}$$

the values of n being determined by the equation $f(n) = 0$.

$$\text{Ex. 6.} \quad (ax + \beta)^m \frac{d^m y}{dx^m} + a(ax + \beta)^{m-\frac{1}{2}} \frac{d^{m-\frac{1}{2}} y}{dx^{m-\frac{1}{2}}} + \&c. = X.$$

$$\text{Let } x' = ax + \beta, \text{ then } \frac{d^m y}{dx^m} = a^m \frac{d^m y}{dx'^m} \quad (\text{Part 1, Art. 27.})$$

$$\&c. = \&c.$$

$$\therefore a^m x'^m \frac{d^m y}{dx'^m} + a a^{m-\frac{1}{2}} x'^{m-\frac{1}{2}} \frac{d^{m-\frac{1}{2}} y}{dx'^{m-\frac{1}{2}}} + \&c. = X'$$

which coincides with Example 5.

$$\text{Ex. 7.} \quad \frac{dy}{dx} - a \cdot \frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} - \frac{1}{2} \frac{y}{x} = 0$$

By multiplying by x and reducing to differentials in θ , we get

$$\frac{\sqrt{-D+1}}{\sqrt{-D}} y + a \sqrt{x} (-1)^{\frac{1}{2}} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}} y + \frac{1}{2} y = 0$$

$$\left(\frac{\sqrt{-D+1}}{\sqrt{-D}} + \frac{1}{2} \right) y + a (-1)^{\frac{1}{2}} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{x}} y = 0$$

$$\text{or} \quad (-D + \frac{1}{2}) y + a (-1)^{\frac{1}{2}} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{x}} y = 0$$

$$\text{or} \quad y + a (-1)^{\frac{1}{2}} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{3}{2}}} e^{\frac{\theta}{x}} y = 0$$

$$\text{or} \quad y + a (-1)^{\frac{1}{2}} e^{\frac{3\theta}{x}} \frac{\sqrt{-D-\frac{1}{2}}}{\sqrt{-D}} \frac{y}{e^{\frac{\theta}{x}}} = 0$$

$$\text{or} \quad \frac{y}{x} - a (-1)^{-\frac{1}{2}} x^{\frac{1}{2}} \frac{\sqrt{-D-\frac{1}{2}}}{\sqrt{-D}} \frac{y}{x} = 0$$

$$\text{or} \quad \frac{y}{x} - a \cdot \frac{d^{-\frac{1}{2}} y}{dx^{-\frac{1}{2}}} = 0.$$

If $\frac{y}{x} = v$; this gives

$$v - a \frac{d^{-\frac{1}{2}} v}{d x^{-\frac{1}{2}}} = 0,$$

whence

$$v = A e^{a^2 x}$$

and

$$y = v x = A x e^{a^2 x}.$$

This equation may be integrated in the following manner. The equation

$$y + a \sqrt{-1} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{3}{2}}} e^{\frac{\theta}{x}} y = 0,$$

may be made to depend on the equation

$$v + a \sqrt{-1} \frac{\sqrt{-D}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{x}} v = 0$$

by the relation

$$y = P_{\frac{1}{2}} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{3}{2}}} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}} v, \text{ where}$$

$$P_{\frac{1}{2}} f(D) = f(D) f(D - \frac{1}{2}) f(D - 1) \&c. \dots \dots$$

$$\begin{aligned} \therefore y &= P_{\frac{1}{2}} \frac{D}{D - \frac{1}{2}} v \\ &= \frac{D(D - \frac{1}{2})(D - 1) \dots \dots}{(D - \frac{1}{2})(D - 1) \dots \dots} v \\ &= D v \\ &= x \frac{d v}{d x} \end{aligned}$$

Now $v + a \sqrt{-1} \frac{\sqrt{-D}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{x}} v = 0$ is equivalent, by (D), to

$$v + a \sqrt{-1} e^{\frac{\theta}{x}} \frac{\sqrt{-D - \frac{1}{2}}}{\sqrt{-D}} v = 0$$

or

$$v - a \frac{d^{-\frac{1}{2}} v}{d x^{-\frac{1}{2}}} = 0$$

whence

$$v = A_1 e^{a^2 x}$$

\therefore

$$y = A x e^{a^2 x} \text{ the same result as before.}$$

This process, which is due to Mr BOOLE, is of great importance in the solution of certain classes of ordinary linear equations, but I have not, as yet, found it very extensively applicable to equations with fractional indices.

Ex. 8. *More generally, to investigate the conditions of integrability of the equation*

$$x \frac{d y}{d x} - c y + a x^{n+\frac{1}{2}} \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} = 0$$

The symbolical form is

$$\left(-\frac{\sqrt{-D+1}}{\sqrt{-D}}-c\right)y+a\sqrt{-1}e^{n\theta}\frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}}\cdot y=0,$$

or
$$-(-D+c)y+a\sqrt{-1}\frac{\sqrt{-D+n+\frac{1}{2}}}{\sqrt{-D+n}}\cdot e^{n\theta}y=0, \text{ by (D).}$$

This is reducible,

1. When $c=n-\frac{1}{2}$; and it becomes, by dividing, by $-D+n-\frac{1}{2}$,

$$y-a\sqrt{-1}\frac{\sqrt{-D+n-\frac{1}{2}}}{\sqrt{-D+n}}\cdot e^{n\theta}y=0$$

or
$$a\sqrt{-1}e^{n\theta}y-\frac{\sqrt{-D+n}}{\sqrt{-D+n-\frac{1}{2}}}\cdot y=0$$

or
$$a\sqrt{-1}y-e^{\frac{\theta}{2}}\frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}}\cdot e^{-(n-\frac{1}{2})\theta}y=0$$

or
$$ay+\frac{d^{\frac{1}{2}}yx^{-(n-\frac{1}{2})}}{dx^{\frac{1}{2}}}=0$$

If $yx^{-(n-\frac{1}{2})}=v$, this equation becomes

$$avx^{n-\frac{1}{2}}+\frac{d^{\frac{1}{2}}v}{dx^{\frac{1}{2}}}=0, \text{ or}$$

$$av+x^{-n+\frac{1}{2}}\frac{dv}{dx}=0$$

which is integrable when $n=\frac{1}{2}, 0, -\frac{1}{2}, -1$, &c. (Class. 2.)

2. When $c=n$, the equation becomes

$$-y+a\sqrt{-1}\frac{\sqrt{-D+n+\frac{1}{2}}}{\sqrt{-D+n+1}}\cdot e^{n\theta}y=0$$

or
$$a\sqrt{-1}e^{n\theta}y-\frac{\sqrt{-D+n+1}}{\sqrt{-D+n+\frac{1}{2}}}\cdot y=0$$

or
$$a\sqrt{-1}y-e^{\frac{\theta}{2}}\frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}}\cdot e^{-(n+\frac{1}{2})\theta}y=0$$

or
$$ay+x\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}x^{n+\frac{1}{2}}}=0.$$

If $\frac{y}{x^{n+\frac{1}{2}}}=v$, this equation becomes

$$av+x^{-n+\frac{1}{2}}\frac{dv}{dx}=0; \text{ the same as before.}$$

Ex. 9.
$$x\frac{dy}{dx}-ax^3\frac{d^{\frac{3}{2}}y}{dx^{\frac{3}{2}}}-\frac{3}{2}y=0.$$

The symbolical form is

$$\frac{-D+1}{-D} y + a (-1)^{\frac{1}{2}} x^{\frac{1}{2}} \frac{-D+\frac{3}{2}}{-D} y + \frac{3}{2} y = 0.$$

$$\text{or} \quad (-D + \frac{3}{2}) y + a (-1)^{\frac{3}{2}} \frac{-D+\frac{3}{2}}{-D+\frac{3}{2}} e^{\frac{3}{2}\theta} y = 0,$$

$$\text{or} \quad y + a (-1)^{\frac{3}{2}} \frac{-D+\frac{3}{2}}{-D+\frac{5}{2}} e^{\frac{3}{2}\theta} y = 0,$$

$$\text{or} \quad y + a (-1)^{\frac{3}{2}} e^{\frac{5}{2}\theta} \frac{-D+\frac{1}{2}}{-D} \frac{y}{e^{\theta}} = 0$$

$$\text{or} \quad y - a e^{2\theta} \frac{d^{\frac{1}{2}} \frac{y}{x}}{d x^{\frac{1}{2}}} = 0$$

$$\text{or} \quad \frac{y}{x} - a x \frac{d^{\frac{1}{2}} \frac{y}{x}}{d x^{\frac{1}{2}}} = 0$$

$$y = A \sqrt{x} e^{-\frac{1}{a^2 x}} \left\{ \frac{1}{2} \int \frac{e^{\frac{a^2 x}{2}}}{\sqrt{x}} dx - \frac{\sqrt{\pi} \sqrt{-1}}{a} \right\} \quad (\text{Ex. 3, Class 2.})$$

$$\text{Ex. 10.} \quad y + a x \frac{dy}{dx} + b x^n \left(\frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} + c x \frac{d^{\frac{3}{2}} y}{d x^{\frac{3}{2}}} \right) + 0.$$

The symbolical form of the equation is

$$y - a \frac{-D+1}{-D} y + b e^{(n-\frac{1}{2})\theta} \sqrt{-1} \left(\frac{-D+\frac{1}{2}}{-D} - c \frac{-D+\frac{3}{2}}{-D} \right) \cdot y = 0$$

$$\text{or} \quad (1+aD)y - b \sqrt{-1} e^{(n-\frac{1}{2})\theta} \frac{-D+\frac{1}{2}}{-D} \left(1+cD-\frac{c}{2} \right) \cdot y = 0 \quad (1).$$

This equation may be reduced in several instances :

A. If $c = \frac{2}{3}$ the equation becomes

$$(1+aD)y - b \sqrt{-1} \frac{2}{3} e^{(n-\frac{1}{2})\theta} \frac{-D+\frac{1}{2}}{-D} \cdot y = 0,$$

$$\text{or} \quad (1+aD)y - \frac{2}{3} b \sqrt{-1} \frac{-D+n}{-D+n-\frac{3}{2}} e^{(n-\frac{1}{2})\theta} y = 0 \quad \text{by (D.)}$$

$$\text{or} \quad y + \frac{2b}{3a} \sqrt{-1} \frac{-D+n}{(-D-\frac{1}{a})(-D+n-\frac{3}{2})} \cdot e^{(n-\frac{1}{2})\theta} y = 0. \quad (2.)$$

1. If $\frac{1}{a} = 1-n$, equation (2) is reduced to

$$y + \frac{2b}{3a} \sqrt{-1} \frac{-D+n-1}{-D+n-\frac{3}{2}} \cdot e^{(n-\frac{1}{2})\theta} y = 0;$$

which is equivalent to $y + \frac{2b}{3a} \sqrt{-1} e^{(n-\frac{3}{2})\theta} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}} \cdot xy = 0$

or $xy + \frac{2b}{3a} x^n \frac{d^{\frac{1}{2}} xy}{dx^{\frac{1}{2}}} = 0$, or, if $v = xy$,

$v + \frac{2b}{3a} x^n \frac{dv}{dx^{\frac{1}{2}}} = 0$, which is the form integrated in Class 1.

2. If $\frac{1}{a} = \frac{3}{2} - n$, equation (2) becomes

$$y + \frac{2b}{3a} \sqrt{-1} \frac{\sqrt{-D+n}}{\sqrt{-D+n-\frac{1}{2}}} \cdot e^{(n-\frac{1}{2})\theta} y = 0$$

which is equivalent to $y + \frac{2b}{3a} \sqrt{-1} e^{(n-\frac{1}{2})\theta} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}} \cdot y = 0$

or $y + \frac{2b}{3a} x^n \frac{dy}{dx^{\frac{1}{2}}} = 0$

which is of the same form as in the last case.

B. If c is not equal to $\frac{2}{3}$, we have from equation (1) by (D.)

$$(1+aD)y + b \sqrt{-1} \frac{\sqrt{-D+n} (1+cD-cn)}{\sqrt{-D+n-\frac{1}{2}}} \cdot e^{(n-\frac{1}{2})\theta} y = 0$$

$$y + b \sqrt{-1} \frac{1+cD-cn}{1+aD} \frac{\sqrt{-D+n}}{\sqrt{-D+n-\frac{1}{2}}} e^{(n-\frac{1}{2})\theta} y = 0$$

3. If $\frac{c}{1-cn} = a$ this gives

$$y + b \sqrt{-1} (1-cn) e^{(n-\frac{1}{2})\theta} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}} \cdot y = 0$$

or $y + b(1-cn) x^n \frac{dy}{dx^{\frac{1}{2}}} = 0$, the same form as before.

16. It would be improper to dismiss this equation without remarking the fact that it would appear to have been solved by M. BESGE in LIOUVILLE'S *Journal* 1844, ix., 294. The solution is, however, given without any demonstration, and is, if I mistake not, rather a differential equation *formed* than a differential equation *solved*. The *whole* which appears is as follows :

“ Let m, n, p, q be functions of x , and $\frac{d^{\frac{3}{2}} y}{dx^{\frac{3}{2}}} + m \frac{dy}{dx} + n \frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} + p y = q$, the proposed equation.

“ If we have $\frac{dm}{dx} + m n - p = 0$, the given equation can be reduced to the following, $\frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} + m y = z$, where z is obtained from the equation $\frac{dz}{dx} + n z = q$.”

Now, on examination, it appears that the proposed equation is nothing more than the differential coefficient of the quantity $\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} + my - z = 0$ added to n times the quantity itself: Thus,

$$\frac{d}{dx} \left(\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} + my - z \right) + n \left(\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} + my - z \right) = 0$$

gives

$$\frac{d^{\frac{3}{2}}y}{dx^{\frac{3}{2}}} + m \frac{dy}{dx} + y \frac{dm}{dx} - \frac{dz}{dx} + n \frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} + mny - nz = 0$$

$$\text{or} \quad \frac{d^{\frac{3}{2}}y}{dx^{\frac{3}{2}}} + m \frac{dy}{dx} + n \frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} + py = \frac{dz}{dx} + nz - \left(\frac{dm}{dx} + mn - p \right) y = q$$

$$\text{provided} \quad \frac{dz}{dx} + nz = q \quad \text{and} \quad \frac{dm}{dx} + mn - p = 0.$$

Thus it appears that the equation is not *solved* but *formed*: and this is probably all M. BESGE intends. How he can justify his additional remark, that $\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} + my = z$ can be solved if m is a constant, or a linear function of x , I am unable to conjecture.

CLASS 4. *Equations which are capable of solution by the division of operations.*

17. We have already met with several equations in Class 1, where the total operation was found to be equivalent to the product of two or more partial operations: and in Art. 9 we have pointed out the manner in which the partial operations are applied, viz., by decomposing the total operation in exactly the same way as an ordinary fraction is decomposed into partial fractions.

$$\text{Ex. 1.} \quad y + ax + by + bx \frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} + 2ax^2 \frac{dy}{dx} = 0.$$

This equation, when reduced to the symbolical form, is

$$y + ae^{\theta}y + be^{\frac{\theta}{2}}\sqrt{-1} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}}y - 2ae^{\theta} \frac{\sqrt{-D+1}}{\sqrt{-D}}y = 0$$

$$\text{or} \quad y + b\sqrt{-1} \frac{\sqrt{-D+1}}{-D+\frac{1}{2}} \cdot e^{\frac{\theta}{2}}y - 2a \left(\frac{\sqrt{-D+2}}{-D+1} - \frac{1}{2} \right) \cdot e^{\theta}y = 0 \text{ by (D).}$$

$$\text{Now} \quad \frac{\sqrt{-D+2}}{\sqrt{-D+1}} - \frac{1}{2} = -D + \frac{1}{2} = \frac{\sqrt{-D+\frac{3}{2}}}{\sqrt{-D+\frac{1}{2}}} = \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} \cdot \frac{\sqrt{-D+\frac{3}{2}}}{\sqrt{-D+1}}$$

$$\therefore \quad \left(\frac{\sqrt{-D+2}}{\sqrt{-D+1}} - \frac{1}{2} \right) e^{\theta}y = \frac{\sqrt{-D+1}}{-D+\frac{1}{2}} e^{\frac{\theta}{2}} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} \cdot e^{\theta}y \text{ by (D)}$$

and the equation is reduced to

$$y + b \sqrt{-1} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} \cdot e^{\frac{\theta}{2}} y - 2a \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{2}} \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{2}} \cdot y = 0.$$

Let us abbreviate the operation $\frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{2}}$ by ϕ , and the equation becomes

$$(1 + b \sqrt{-1} \phi - 2a \phi^2) \cdot y = 0.$$

If $1 + b \sqrt{-1} z - 2a z^2 = (1 + \alpha z)(1 + \beta z)$; this equation is equivalent to

$$(1 + \alpha \phi)(1 + \beta \phi)y = 0$$

or
$$y = \frac{1}{(1 + \alpha \phi)(1 + \beta \phi)} \cdot 0 = \frac{\alpha}{\alpha - \beta} \cdot \frac{1}{1 + \alpha \phi} 0 - \frac{\beta}{\alpha - \beta} \frac{1}{1 + \beta \phi} \cdot 0$$

Now
$$1 + \alpha \phi = 1 - a \frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}} e^{\frac{\theta}{2}} = 1 + \alpha e^{\frac{\theta}{2}} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}}$$

$$= 1 - \alpha \sqrt{-1} x \frac{d^{\frac{1}{2}}}{dx^{\frac{1}{2}}}$$

Hence the solution of the given equation is reduced to the solution of the two equations

$$y_1 + \alpha e^{\frac{\theta}{2}} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}} y_1 = 0, \quad y_2 + \beta e^{\frac{\theta}{2}} \frac{\sqrt{-D+\frac{1}{2}}}{\sqrt{-D}} y_2 = 0$$

or
$$y_1 - \alpha \sqrt{-1} x \frac{d^{\frac{1}{2}} y_1}{dx^{\frac{1}{2}}} = 0, \quad y_2 - \beta \sqrt{-1} x \frac{d^{\frac{1}{2}} y_2}{dx^{\frac{1}{2}}} = 0$$

Now these equations have been solved in Class 2, Ex. 3, and they give

$$y_1 = \frac{A e^{\frac{\alpha^2 x}{2}}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\alpha^2 x}}}{\sqrt{x}} dx - \frac{\sqrt{\pi}}{\alpha} \right\}$$

$$y_2 = \frac{B e^{\frac{\beta^2 x}{2}}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\beta^2 x}}}{\sqrt{x}} dx - \frac{\sqrt{\pi}}{\beta} \right\}$$

and
$$y = \frac{1}{(1 - \alpha \phi)(1 - \beta \phi)} \cdot 0 = \frac{\alpha}{\alpha - \beta} \frac{1}{1 - \alpha \phi} \cdot 0 - \frac{\beta}{\alpha - \beta} \frac{1}{1 - \beta \phi} \cdot 0$$

$$= \frac{A \alpha}{\alpha - \beta} \frac{e^{\frac{\alpha^2 x}{2}}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\alpha^2 x}}}{\sqrt{x}} dx - \frac{\sqrt{\pi}}{\alpha} \right\}$$

$$- \frac{B \beta}{\alpha - \beta} \frac{e^{\frac{\beta^2 x}{2}}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\beta^2 x}}}{\sqrt{x}} dx - \frac{\sqrt{\pi}}{\beta} \right\}$$

It will be readily seen that B is not an arbitrary constant, independent of A.

See Art. 18.) For when $b=0$, the equation becomes an ordinary linear equation

of the first degree, of which the solution is $y = C \frac{e^{\frac{1}{\alpha} x}}{\sqrt{x}}$.

In this case $\alpha = \beta$ and $A = -B$:

we may therefore write $B = -A$ generally, and we obtain as the complete solution

$$y = \frac{A e^{\frac{1}{\alpha} x}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\alpha} x}}{\sqrt{x}} dx - \frac{\sqrt{\pi}}{\alpha} \right\} - \frac{A e^{\frac{1}{\beta} x}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\beta} x}}{\sqrt{x}} dx - \frac{\sqrt{\pi}}{\beta} \right\}$$

The above equation may be reduced differently, thus. The symbolical form

$$y + a e^{\theta} y + b e^{\frac{\theta}{2}} \sqrt{-1} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}} y - 2 a e^{\theta} \frac{\sqrt{-D + 1}}{\sqrt{-D}} y = 0,$$

may be written

$$2 a (-D - \frac{1}{2}) y - b \sqrt{-1} e^{-\frac{\theta}{2}} \frac{\sqrt{-D + \frac{1}{2}}}{\sqrt{-D}} y - \frac{1}{2 a} e^{-\theta} y = 0,$$

or

$$y - \frac{b \sqrt{-1}}{2 a} \frac{\sqrt{-D}}{\sqrt{-D + \frac{1}{2}}} e^{-\frac{\theta}{2}} y - \frac{1}{2 a} \frac{1}{-D - \frac{1}{2}} e^{-\theta} y = 0,$$

or

$$y - \frac{b \sqrt{-1}}{2 a} \frac{\sqrt{-D}}{-D + \frac{1}{2}} e^{-\frac{\theta}{2}} y - \frac{1}{2 a} \frac{\sqrt{-D}}{\sqrt{-D + \frac{1}{2}}} \frac{\sqrt{-D - \frac{1}{2}}}{\sqrt{-D}} e^{-\theta} y = 0,$$

or

$$y - \frac{b \sqrt{-1}}{2 a} \frac{\sqrt{-D}}{\sqrt{-D + \frac{1}{2}}} e^{-\frac{\theta}{2}} y - \frac{1}{2 a} \frac{\sqrt{-D}}{\sqrt{-D + \frac{1}{2}}} e^{-\frac{\theta}{2}} \frac{\sqrt{-D}}{\sqrt{-D + \frac{1}{2}}} e^{-\theta} y = 0,$$

which is of the form $(1 - \frac{b \sqrt{-1}}{2 a} \phi_1 - \frac{1}{2 a} \phi_1^2) y = 0$;

of which the solutions are

$$(1 + \frac{1}{\alpha} \phi_1) y = 0, \text{ and } (1 + \frac{1}{\beta} \phi_1) y = 0, \text{ or}$$

$$y + \frac{1}{\alpha} e^{\frac{\theta}{2}} \frac{\sqrt{-D - \frac{1}{2}}}{\sqrt{-D}} e^{-\theta} y = 0, \text{ and } y + \frac{1}{\beta} e^{\frac{\theta}{2}} \frac{\sqrt{-D - \frac{1}{2}}}{\sqrt{-D}} e^{-\theta} y = 0,$$

or

$$y - \frac{1}{\alpha \sqrt{-1}} \frac{d^{-\frac{1}{2}}}{dx^{-\frac{1}{2}}} \frac{y}{x} = 0, \text{ and } y - \frac{1}{\beta \sqrt{-1}} \frac{d^{-\frac{1}{2}}}{dx^{-\frac{1}{2}}} \frac{y}{x} = 0;$$

which, on differentiation to the index $\frac{1}{2}$, give the same results as before.

Ex. 2. $y + a x y + b x \frac{d^{\frac{1}{2}} y}{dx^{\frac{1}{2}}} + 2 a x^2 \frac{d y}{d x} = X.$

The solution is, as in Example 1,

$$y = \frac{\alpha}{\alpha - \beta} \frac{1}{1 + \alpha \phi} (X + 0) - \frac{\beta}{\alpha - \beta} \frac{1}{1 + \beta \phi} (X + 0)$$

Now $\frac{X+0}{1+\alpha\phi}$ is the solution of the equation $y_1 - \alpha\sqrt{-1}x \frac{d^{\frac{1}{2}}y_1}{dx^{\frac{1}{2}}} = X + 0$; which (Class II, Ex. 8, Cor. 1) is

$$\left(1 + \alpha\sqrt{-1}x \frac{d^{\frac{1}{2}}}{dx^{\frac{1}{2}}}\right) \left(\frac{1}{\sqrt{x}} e^{\alpha^2 x} + \frac{1}{\sqrt{x}} \int \frac{e^{-\alpha^2 x}}{\alpha^2 x^{\frac{3}{2}}} X dx\right)$$

and a similar equation results for β . Hence the solution of the given equation is known.

18. It must be remarked of this solution, that it is not in all cases complete without the introduction of the complementary (or arbitrary) function. This arises from the circumstance that when y contains positive integral powers of x , $\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} = 0$, whereas $x \frac{d^{\frac{1}{2}}}{dx^{\frac{1}{2}}} x \frac{d^{\frac{1}{2}}}{dx^{\frac{1}{2}}}$ is not equal to 0.

Hence $x^2 \frac{dy}{dx} + \frac{1}{2}xy$ can be replaced by the latter function only by the convention that $\frac{d^{\frac{1}{2}}x^n}{dx^{\frac{1}{2}}}$ is not to be written 0 when n is a positive integer.

On account of this convention, the solution of the equation $\frac{1}{1+\alpha\phi}y = X$ must contain, besides the expression given for it above, a series of positive integral powers of x ; and hence y , the solution of Equation (2), is incomplete without the addition of such a function. It is probable, however, that the determination of a relation between the arbitrary constants may give a solution possessing all the generality which the science is capable of. We have already given an example of the mode of avoiding arbitrary functions by introducing such a relation in Example 1. We shall offer another as a corollary.

Cor. If $X = \frac{e}{\sqrt{x}}$, the solution is (Class 2, Ex. 8, Cor 2.)

$$\begin{aligned} y &= y_0 + \frac{\alpha}{\alpha - \beta} \left(\frac{e}{\sqrt{x}} - \frac{\alpha e}{\sqrt{x}} \right) - \frac{\beta}{\alpha - \beta} \left(\frac{e}{\sqrt{x}} - \frac{\beta e}{\sqrt{x}} \right) + \text{arbitrary function} \\ &= y_0 + \frac{e}{\sqrt{x}} - (\alpha + \beta) \frac{e}{\sqrt{\pi}} + \text{arbitrary function} \\ &= y_0 + \frac{e}{\sqrt{x}} - (\alpha + \beta) \frac{e}{\sqrt{\pi}} + px + qx^2 + \&c. \end{aligned}$$

Now if we examine the equation which connects together p , q , &c., we shall find that it is the same as that which determines y_1 in Class 2, Ex. 3, having only 2α in place of α^2 . Hence it is contained in the solution of the given equation when b and X are omitted. It is, therefore, itself only a supplementary term in the solution of the given equation, and its place may be supplied, appa-

rently without any sacrifice of generality, by the introduction of a relation between A and B. The relation is, $A + B = \frac{b e \sqrt{-1}}{\sqrt{\pi}}$.

Hence the complete solution of the equation

$$y + a x y + b x \frac{d^{\frac{1}{2}} y}{d x^{\frac{1}{2}}} + 2 a x^2 \frac{d y}{d x} = \frac{e}{\sqrt{x}}, \text{ is}$$

$$y = \frac{A e^{\frac{1}{\beta^2 x}}}{\sqrt{x}} \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\alpha^2 x}}}{\sqrt{x}} d x - \frac{\sqrt{\pi}}{a} \right\} - \left(A - \frac{b e \sqrt{-1}}{\sqrt{\pi}} \right) \left\{ \frac{1}{2} \int \frac{e^{-\frac{1}{\beta^2 x}}}{\sqrt{x}} d x - \frac{\sqrt{\pi}}{\beta} \right\} + \frac{e}{\sqrt{x}} - \frac{b e \sqrt{-1}}{\sqrt{\pi}}$$

$$19. \text{ Ex. 3. } x y + a x \frac{d^{-1} y}{d x^{-1}} - (a + b x) \frac{d^{-2} y}{d x^{-2}} + 2 b \frac{d^{-3} y}{d x^{-3}} = 0.$$

Multiply by x^{-3} , and the result will be

$$x^{-2} y + a x^{-2} \frac{d^{-1} y}{d x^{-1}} - (a x^{-3} + b x^{-2}) \frac{d^{-2} y}{d x^{-2}} + 2 b x^{-3} \frac{d^{-3} y}{d x^{-3}} = 0$$

of which the symbolical form is

$$y e^{-2\theta} - a e^{-\theta} \frac{\sqrt{-D-1}}{\sqrt{-D}} y - (a e^{-\theta} + b) \frac{\sqrt{-D-2}}{\sqrt{-D}} y - 2 b \frac{\sqrt{-D-3}}{\sqrt{-D}} y = 0$$

which is equivalent to

$$y e^{-2\theta} + a e^{-\theta} \left(\frac{1}{D+1} - \frac{1}{(D+1)(D+2)} \right) y - b \left(\frac{1}{(D+1)(D+2)} - \frac{2}{(D+1)(D+2)(D+3)} \right) y = 0$$

$$\text{or } y e^{-2\theta} + a e^{-\theta} \frac{1}{D+2} y - b \frac{1}{(D+2)(D+3)} y = 0$$

Hence, by multiplication,

$$y - \frac{a}{b} (D+2)(D+3) e^{-\theta} \frac{1}{D+2} y - \frac{1}{b} (D+2)(D+3) e^{-2\theta} y = 0$$

$$\text{or } y - \frac{a}{b} (D+2) e^{-\theta} y - \frac{1}{b} (D+2) e^{-\theta} (D+2) e^{-\theta} y = 0 \text{ by (B)}$$

$$\text{which is of the form } \left(1 - \frac{a}{b} \phi - \frac{1}{b} \phi^2 \right) y = 0;$$

which, being put under the form

$$(1 + \alpha \phi) (1 + \beta \phi) y = 0 \text{ gives}$$

$$y = A (1 + \alpha \phi)^{-1} . 0 + B (1 + \beta \phi)^{-1} . 0$$

Now $(1 + \alpha \phi)^{-1} . 0$ is the solution of the equation

$$y_1 + \alpha (D+2) y_1 e^{-\theta} = 0 \text{ or } y_1 + \frac{\alpha}{x} \frac{d x y_1}{d x} = 0$$

of which the result is $y_1 = \frac{A_1}{x} e^{-\frac{x}{\alpha}}$.

Hence $y = \frac{A}{x} e^{-\frac{x}{a}} + \frac{B}{x} e^{-\frac{x}{\beta}}$ is the complete solution of the given equation.

In my second Memoir on this subject, I exemplified the use of a theorem in general differentiation, by solving the problem of determining the law of force by which the particles of a sphere must act on a point, so that the whole attraction may be the same as if the sphere were collected at its centre of gravity. The solution of this problem led to a differential equation which was shewn, by an indirect process, to be satisfied by the law of force varying as the distance, or inversely as its square. I propose, at present, to solve this differential equation.

Ex. 4. The equation is (vol. xiv., p. 608).

$$\frac{\pi}{4a^2} \left\{ 8aR(a+R) \frac{d^{-2}y}{dz^{-2}} - 8(a^2 + 3aR + R^2) \frac{d^{-3}y}{dz^{-3}} \right. \\ \left. + 24(a+R) \frac{d^{-4}y}{dz^{-4}} - 24 \frac{d^{-5}y}{dz^{-5}} \right\} = \frac{4\pi}{3} R^3 f(a)$$

where $y = f(z+a)$, $z = 2R$, $a = a - R$.

This becomes, by substitution,

$$4az \left(a + \frac{z}{2} \right) \frac{d^{-2}y}{dz^{-2}} - 8 \left(a^2 + \frac{3az}{2} + \frac{z^2}{4} \right) \frac{d^{-3}y}{dz^{-3}} \\ + 24 \left(a + \frac{z}{2} \right) \frac{d^{-4}y}{dz^{-4}} - 24 \frac{d^{-5}y}{dz^{-5}} = \frac{2}{3} a^2 f a z^3.$$

Dividing by z^5 , we get

$$\frac{4a^2}{z^4} \frac{d^{-2}y}{dz^{-2}} + \frac{2a}{z^3} \frac{d^{-2}y}{dz^{-2}} - \frac{8a^2}{z^5} \frac{d^{-3}y}{dz^{-3}} - \frac{12a}{z^4} \frac{d^{-3}y}{dz^{-3}} - \frac{2}{z^3} \frac{d^{-3}y}{dz^{-3}} \\ + \frac{24a}{z^5} \frac{d^{-4}y}{dz^{-4}} + \frac{12}{z^4} \frac{d^{-4}y}{dz^{-4}} - \frac{24}{z^5} \frac{d^{-5}y}{dz^{-5}} = \frac{2}{3} \frac{a^2 f(a)}{z^2}$$

Writing e^θ for z , and $(-1)^{-\mu} \frac{\sqrt{-D-\mu}}{\sqrt{-D}} y$ for $\frac{1}{z^\mu} \frac{d^{-\mu}y}{dz^{-\mu}}$, there results the symbolical form

$$4a^2 e^{-2\theta} \frac{\sqrt{-D-2}}{\sqrt{-D}} y + 2a e^{-\theta} \frac{\sqrt{-D-2}}{\sqrt{-D}} y + 8a^2 e^{-2\theta} \frac{\sqrt{-D-3}}{\sqrt{-D}} y \\ + 12a e^{-\theta} \frac{\sqrt{-D-3}}{\sqrt{-D}} y + 2 \frac{\sqrt{-D-3}}{\sqrt{-D}} y + 24a e^{-\theta} \frac{\sqrt{-D-4}}{\sqrt{-D}} y \\ + 12 \frac{\sqrt{-D-4}}{\sqrt{-D}} y + 24 \frac{\sqrt{-D-5}}{\sqrt{-D}} y = \frac{2}{3} a^2 f(a) e^{-2\theta};$$

or, collecting the terms,

$$\left\{ 24 \frac{\sqrt{-D-5}}{\sqrt{-D}} + 12 \frac{\sqrt{-D-4}}{\sqrt{-D}} + \frac{\sqrt{-D-3}}{\sqrt{-D}} \right\} y +$$

$$+ a e^{-\theta} \left\{ 24 \frac{\sqrt{-D-4}}{\sqrt{-D}} + 12 \frac{\sqrt{-D-3}}{\sqrt{-D}} + 2 \frac{\sqrt{-D-2}}{\sqrt{-D}} \right\} y$$

$$+ a^2 e^{-2\theta} \left\{ 8 \frac{\sqrt{-D-3}}{\sqrt{-D}} + 4 \frac{\sqrt{-D-2}}{\sqrt{-D}} \right\} y = \frac{2}{3} a^2 f(a) e^{-2\theta}$$

which being reduced gives

$$2(D+1)(D+2) \frac{\sqrt{-D-5}}{\sqrt{-D}} y + 2a e^{-\theta} D(D+1) \frac{\sqrt{-D-4}}{\sqrt{-D}} y$$

$$- 4a^2 e^{-2\theta} (D+1) \frac{\sqrt{-D-3}}{\sqrt{-D}} y = \frac{2}{3} a^2 f(a) e^{-2\theta};$$

or

$$- \frac{1}{(D+3)(D+4)(D+5)} y + a e^{-\theta} \frac{D}{(D+2)(D+3)(D+4)} y$$

$$+ 2a^2 e^{-2\theta} \frac{1}{(D+2)(D+3)} y = \frac{1}{3} a^2 f(a) e^{-2\theta}$$

Now $y = f(a+z)$, but since a is itself a function of z , we cannot proceed further with the reduction of this equation by division, but must proceed to obtain a relation between a and R or a and z .

To do this we shall expand $f(a+z)$ by TAYLOR'S Theorem.

The result is

$$y = \sum \frac{d^n f(a)}{d a^n} \frac{z^n}{n+1} \text{ which being substituted in the reduced equation, gives by (A)}$$

$$\sum \left(\frac{d^n f(a)}{d a^n} \frac{e^{n\theta}}{n+1} \right) \left\{ \frac{-1}{(n+3)(n+4)(n+5)} \right.$$

$$\left. + a \frac{n}{(n+2)(n+3)(n+4)} e^{-\theta} + \frac{2a^2 e^{-2\theta}}{(n+2)(n+3)} \right\} = \frac{a^2}{3} f(a) e^{-2\theta}$$

But $a = a - R = a - \frac{z}{2}$: hence

$$\frac{d^n f(a)}{d a^n} = \frac{d^n f a}{d a^n} - \frac{d^{n+1} f a}{d a^{n+1}} \frac{z}{2} + \&c.$$

which being substituted for $\frac{d^n f(a)}{d a^n}$, the sum being taken for n and p , we get

$$\sum_{n,p} \frac{(-1)^p}{2^p} \frac{d^{n+p} f a}{d a^{n+p}} \frac{1}{n+1} \frac{1}{p+1} \left\{ \frac{-z^{n+p}}{(n+3)(n+4)(n+5)} \right.$$

$$\left. + \frac{a n z^{n+p-1}}{(n+2)(n+3)(n+4)} + \frac{2a^2 z^{n+p-2}}{(n+2)(n+3)} \right\} = \frac{a^2}{3} \frac{f a}{z^2}$$

every integer value of n and p being taken from 0 to ∞ .

When $n=0$, $p=0$, the left-hand side gives

$$\frac{-f a}{60} + \frac{a^2 f a}{3 z^2}$$

$$n=0, p=1, \dots \frac{z}{120} \frac{d f a}{d a} - \frac{a^2}{6z} \frac{d f a}{d a}$$

$$n=0, p=2, \dots \frac{-z^2}{480} \frac{d^2 f a}{d a^2} + \frac{a^2}{24} \frac{d^2 f a}{d a^2}$$

When $n=1, p=0, \dots \frac{-z}{120} \frac{d f a}{d a} + \frac{a}{60} \frac{d f a}{d a}$
 $+ \frac{a^2}{6z} \frac{d f a}{d a}$

$$n=1, p=1, \frac{z^2}{240} \frac{d^2 f a}{d a^2} - \frac{a z}{120} \frac{d^2 f a}{d a^2} - \frac{a^2}{12} \frac{d^2 f a}{d a^2}$$

$$n=2, p=0, \frac{-z^2}{420} \frac{d^2 f a}{d a^2} + \frac{a z}{120} \frac{d^2 f a}{d a^2} + \frac{a^2}{20} \frac{d^2 f a}{d a^2}, \&c., \&c.$$

Hence we obtain, by collecting the terms and equating their sum to $\frac{a^2 f a}{3 z^2}$,

$$\frac{a^2 f a}{3 z^2} - \frac{f a}{60} + \frac{a}{60} \frac{d f a}{d a} + \frac{a^2}{120} \frac{d^2 f a}{d a^2} + P z + Q z^2 + \&c.$$

$$= \frac{a^2 f a}{3 z^2}$$

Equating coefficients of like powers of z , we obtain

$$-\frac{f a}{60} + \frac{a}{60} \frac{d f a}{d a} + \frac{a^2}{120} \frac{d^2 f a}{d a^2} = 0.$$

This equation will determine $f(a)$, the only law of force by which a sphere can attract an external particle exactly as much as if it were all collected at its centre of gravity.

The symbolical form of the equation is

$$\{D(D-1) + 2D-2\} f a = 0$$

or $(D^2 + D - 2) f a = 0$, or $(D-1)(D+2) f a = 0$.

Hence $(D-1) f a = 0$, $(D+2) f a = 0$,

or $\frac{d f a}{d a} = f a$, and $\frac{d f a}{d a} = -2 f a$

that is $f a = A a$, and $f a = \frac{B}{a^2}$ are particular integrals, and the complete integral is

$y = A a + \frac{B}{a^2}$; which is the law required.

SECTION II. SIMULTANEOUS EQUATIONS.

20. To effect the solution of simultaneous equations, we must eliminate one of the quantities differentiated. This is best effected by treating both the differ-

entiation and the multiplication by a constant in the same manner, regarding both the one and the other as an operation. A similar process has been employed for the solution of ordinary simultaneous equations by Mr GREGORY in the *Cambridge Mathematical Journal*, i. 173.

$$\text{Ex. 1.} \quad \frac{d^{\frac{1}{2}}x}{dt^{\frac{1}{2}}} + ay = 0, \quad \frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} + bx = 0$$

By taking the $\frac{1}{2}$ differential of the first equation, we get $\frac{dx}{dt} + a \frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} = 0$; which, by virtue of the second, gives

$$\frac{dx}{dt} - abx = 0; \text{ or } x = A e^{ab t} \quad \therefore y = -A \sqrt{\frac{b}{a}} e^{ab t}.$$

$$\text{Ex. 2.} \quad \frac{d^{\frac{1}{2}}x}{dt^{\frac{1}{2}}} + ay + bx = 0, \quad \frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} + a'y + b'x = 0.$$

These equations may be written

$$\left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + b\right)x + ay = 0, \quad \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a'\right)y + b'x = 0;$$

whence, by eliminating y , we obtain

$$\left\{ \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a'\right) \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + b\right) - ab' \right\} x = 0.$$

Let $\alpha^{\frac{1}{2}}, \beta^{\frac{1}{2}}$, be the roots of the equation

$$(z + a')(z + b) - ab' = 0$$

then $\left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} - \alpha^{\frac{1}{2}}\right) \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} - \beta^{\frac{1}{2}}\right) x = 0$; which gives

$$x = A e^{\alpha t} + B e^{\beta t}$$

and

$$y = -\frac{1}{a} \frac{d^{\frac{1}{2}}x}{dt^{\frac{1}{2}}} - \frac{b}{a} x \\ = -\left(\frac{\alpha^{\frac{1}{2}}}{a} + \frac{b}{a}\right) A e^{\alpha t} - \left(\frac{\beta^{\frac{1}{2}}}{a} + \frac{b}{a}\right) B e^{\beta t}.$$

$$\text{Ex. 3.} \quad \frac{d^{\frac{1}{2}}x}{dt^{\frac{1}{2}}} + ay + bx = f(t)$$

$$\frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} + a'y + b'x = \phi(t)$$

$$\left\{ \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a'\right) \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + b\right) - ab' \right\} x = \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a'\right) f(t) - a\phi(t) \\ = \psi(t)$$

This coincides with Ex. 4, Class 1, and the solution is

$$x = A e^{\alpha t} + B e^{\beta t} + \frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} - \alpha^{\frac{1}{2}}\right)^{-1} \psi(t) - \frac{1}{\alpha^{\frac{1}{2}} - \beta^{\frac{1}{2}}} \left(\frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} - \beta^{\frac{1}{2}}\right)^{-1} \psi(t)$$

Ex. 4.
$$\frac{d^{\frac{1}{2}}x}{dt^{\frac{1}{2}}} + ax + by + cz = 0$$
$$\frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} + a'x + b'y + c'z = 0$$
$$\frac{d^{\frac{1}{2}}z}{dt^{\frac{1}{2}}} + a''x + b''y + c''z = 0$$

These equations can be written in the form

$$\begin{aligned} Ax + by + cz &= 0 \\ a'x + B'y + c'z &= 0 \\ a''x + b''y + C''z &= 0 \end{aligned}$$

By eliminating y and z we obtain

$$\{A B' C'' - b'' c' A - a'' c B' - a' b C'' + a' b'' c + a'' b c'\} x = 0$$

or
$$\left\{ \frac{d^{\frac{3}{2}}}{dt^{\frac{3}{2}}} + (a + b' + c) \frac{d}{dt} + (a b' + a c'' + b c'') \frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a b' c'' - b'' c' \frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a b'' c' - a'' c \frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a'' b' c - a' b \frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a' b c' \right\} x = 0$$

or
$$\left\{ \frac{d^{\frac{3}{2}}}{dt^{\frac{3}{2}}} + (a + b + c) \frac{d}{dt} + (a b' + a c'' + b'' c' - b c'' - a'' c' - a' b) \frac{d^{\frac{1}{2}}}{dt^{\frac{1}{2}}} + a b' c'' + a b'' c' + a'' b' c + a' b c'' + a' b'' c + a'' b c' \right\} x = 0$$

which is of the same form as Cor. 2, Ex. 3, Class 1, and its integral is therefore known.

Knowing x , we have $by + cz = f(t)$ by the first of the three equations, and $\frac{b d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} + c \frac{d^{\frac{1}{2}}z}{dt^{\frac{1}{2}}} = \phi(t)$ by differentiation, whence, by substituting the values of

$\frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}}$ and $\frac{d^{\frac{1}{2}}z}{dt^{\frac{1}{2}}}$ from the second and third equations, there results a second equation between y , z , and t . From these two equations y and z are determined in terms of t .

Ex. 5. Given
$$\frac{dx}{dt} + a \frac{d^{\frac{1}{2}}x}{dt^{\frac{1}{2}}} + bx = p \frac{dy}{dt} + q \frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} + ry$$
$$\frac{dx}{dt} + a' \frac{d^{\frac{1}{2}}x}{dt^{\frac{1}{2}}} + b'x = p' \frac{dy}{dt} + q' \frac{d^{\frac{1}{2}}y}{dt^{\frac{1}{2}}} + r'y$$

These equations may be written

$$\begin{aligned} (d + a d^{\frac{1}{2}} + b) x &= (p d + q d^{\frac{1}{2}} + r) y \\ (d + a' d^{\frac{1}{2}} + b') x &= (p' d + q' d^{\frac{1}{2}} + r') y \end{aligned}$$

$$\begin{aligned} \therefore (p'd + q'd^{\frac{1}{2}} + r')(d + a d^{\frac{1}{2}} + b)x &= (p d + q d^{\frac{1}{2}} + r)(d + a' d^{\frac{1}{2}} + b')x \\ \text{or } (p-p') \frac{d^2 x}{d t^2} + (p a' + q - p' a - q') \frac{d^{\frac{3}{2}} x}{d t^{\frac{3}{2}}} &+ (p b' + q a' + r \\ &- p' b - q' a - r') \frac{d x}{d t} + (q b' + r a' - q' b - r' a) \frac{d^{\frac{1}{2}} x}{d t^{\frac{1}{2}}} \\ &+ r b' - r' b)x = 0 \end{aligned}$$

which coincides with the general form Cor. 2, Ex. 3, Class 1.

SECTION III. PARTIAL DIFFERENTIAL EQUATIONS.

21. In order to effect the solution of partial differential equations in which the operation with respect to x is totally independent of the operation with respect to y , we must distinguish the operation of differentiation in the two cases by different symbols. Let d stand for $\frac{d}{dx}$, δ for $\frac{d}{dy}$: then in solving the equation with respect to d , we may treat δ as a constant, and *vice versa*.

$$\text{Ex. 1.} \quad \frac{d^{\frac{1}{2}} z}{d x^{\frac{1}{2}}} - b \frac{d^{\frac{1}{2}} z}{d y^{\frac{1}{2}}} = 0.$$

Write this equation $(d^{\frac{1}{2}} - b \delta^{\frac{1}{2}})z = 0$: In this form it coincides with Ex. 1, Class 1, and its solution is $z = A e^{b^{\frac{1}{2}} \delta x}$.

Now A is an arbitrary function of y ; call it $f(y)$: then $z = e^{b^{\frac{1}{2}} x \delta} f(y)$, where $e^{b^{\frac{1}{2}} x \delta}$ represents an operation on $f(y)$.

$$\begin{aligned} \text{But since } f(y+k) &= f y + \frac{d f y}{d y} k + \\ &= (1 + k \delta + \&c.) f(y) \\ &= e^{k \delta} f(y), \end{aligned}$$

it is evident that

$$z = f(y + b^{\frac{1}{2}} x).$$

$$\text{Ex. 2.} \quad \frac{d^{\frac{1}{2}} z}{d x^{\frac{1}{2}}} = a \frac{d z}{d y}.$$

This equation may be written $(d^{\frac{1}{2}} - a \delta)z = 0$.

$$\therefore z = e^{a^2 x \delta^2} f(y).$$

$$\text{Now } \int_{-\infty}^{\infty} d\omega e^{-(\omega-b)^2} = \sqrt{\pi} \quad (\text{GREGORY'S Examples, p. 499.})$$

$$\therefore z \sqrt{\pi} = \int_{-\infty}^{\infty} d\omega e^{-\omega^2 + 2 b \omega - b^2} \cdot e^{a^2 x \delta^2} f(y)$$

$$= \int_{-\infty}^{\infty} d\omega e^{-\omega^2} e^{2 \omega a \sqrt{x} \delta} f(y) \quad (\text{if } b = a \sqrt{x} \delta)$$

$$= \int_{-\infty}^{\infty} d\omega e^{-\omega^2} f(y + 2\omega a \sqrt{x})$$

which is the solution of the equation in the form of a definite integral.

Ex. 3.
$$\frac{d^{\frac{1}{2}} z}{d x^{\frac{1}{2}}} = a \frac{d z}{d y} + c z$$

The first form of the solution is evidently

$$z = e^{(a\delta + c)^2 x} f(y)$$

which is reduced to

$$\begin{aligned} z &= e^{c^2 x} e^{a^2 \delta^2 x} f(y + 2acx) \\ &= \frac{e^{c^2 x}}{\sqrt{\pi}} \int_{-\infty}^{\infty} d\omega e^{-\omega^2} f(y + 2acx + 2\omega a \sqrt{x}) \end{aligned}$$

as in Example 2.

Ex. 4.
$$\frac{d z}{d x} - 2a \frac{d^{\frac{1}{2}} z}{d x^{\frac{1}{2}}} \cdot \frac{d}{d y} z + a^2 \frac{d^2 z}{d y^2} = c^2 z$$

This equation may be written $(d - 2a\delta^{\frac{1}{2}}\delta + a^2\delta^2 - c^2)z = 0$, which is of the form of Ex. 3, Class 1, and the solution is

$$\begin{aligned} z &= e^{c^2 x} e^{a^2 \delta^2 x} \{e^{2acx\delta} f(y) + e^{-2acx\delta} \phi(y)\} \\ &= e^{c^2 x} e^{a^2 \delta^2 x} \{f(y + 2acx) + \phi(y - 2acx)\} \\ &= \frac{e^{c^2 x}}{\sqrt{\pi}} \int_{-\infty}^{\infty} d\omega e^{-\omega^2} \{f(y + 2acx + 2\omega a \sqrt{x}) + \phi(y - 2acx + 2\omega a \sqrt{x})\} \end{aligned}$$

Ex. 5.
$$\frac{d z}{d x} - 2a \frac{d^{\frac{1}{2}} z}{d x^{\frac{1}{2}}} \cdot \frac{d^{\frac{1}{2}} z}{d y^{\frac{1}{2}}} + a^2 \frac{d z}{d y} = 0.$$

This equation gives $(d - 2a\delta^{\frac{1}{2}}\delta^{\frac{1}{2}} + a^2\delta)z = 0$

or

$$\begin{aligned} z &= e^{a^2 x \delta} f(y) + x e^{a^2 x \delta} \phi(y) \quad (\text{Class 1, Ex. 3. Cor. 1.}) \\ &= f(y + a^2 x) + x \phi(y + a^2 x) \end{aligned}$$

Ex. 6.
$$\frac{d z}{d x} + a \frac{d^{\frac{1}{2}} z}{d x^{\frac{1}{2}}} \cdot \frac{d^{\frac{1}{2}} z}{d y^{\frac{1}{2}}} + b \frac{d z}{d y} = c z.$$

This equation may be written $(d + a\delta^{\frac{1}{2}}\delta^{\frac{1}{2}} + b\delta - c)z = 0$ which coincides with Ex. 3, Class 1; and the solution is

$$z = e^{ax} f(y) + e^{\beta x} \phi(y)$$

where $\alpha^{\frac{1}{2}}, \beta^{\frac{1}{2}}$ are the roots of the equation in v

$$v + a\delta^{\frac{1}{2}}v^{\frac{1}{2}} + b\delta - c = 0:$$

or

$$\begin{aligned} \alpha^{\frac{1}{2}} &= -\frac{a\delta^{\frac{1}{2}}}{2} + \sqrt{\left(\frac{a^2}{4} - b\right)\delta + c} \\ \beta^{\frac{1}{2}} &= -\frac{a\delta^{\frac{1}{2}}}{2} - \sqrt{\left(\frac{a^2}{4} - b\right)\delta + c} \end{aligned}$$

or

$$\alpha = \left(\frac{a^2}{2} - b\right) \delta + c - a \delta^{\frac{1}{2}} \sqrt{\left(\frac{a^2}{4} - b\right) \delta + c}$$

$$\beta = \left(\frac{a^2}{2} - b\right) \delta + c + a \delta^{\frac{1}{2}} \sqrt{\left(\frac{a^2}{4} - b\right) \delta + c}$$

$$z = e^{c x} e^{\left(\frac{a^2}{2} - b\right) \delta x} \left\{ e^{-a \delta^{\frac{1}{2}} x \sqrt{\left(\frac{a^2}{4} - b\right) \delta + c}} f(y) + e^{a \delta^{\frac{1}{2}} x \sqrt{\left(\frac{a^2}{4} - b\right) \delta + c}} \phi(y) \right\}$$

$$= e^{c x} \left\{ e^{-a \delta^{\frac{1}{2}} x \sqrt{\left(\frac{a^2}{4} - b\right) \delta + c}} f\left(y + \left(\frac{a^2}{2} - b\right)x\right) + e^{a \delta^{\frac{1}{2}} x \sqrt{\left(\frac{a^2}{4} - b\right) \delta + c}} \phi\left(y + \left(\frac{a^2}{2} - b\right)x\right) \right\}$$

These expressions do not appear to be susceptible of further reduction, except in particular cases.

COR. Let $b = \frac{a^2}{4}$; then

$$z = e^{c x} \left\{ e^{-a x \delta^{\frac{1}{2}} \sqrt{c}} f\left(y + \frac{a^2}{4} x\right) + e^{a x \delta^{\frac{1}{2}} \sqrt{c}} \phi\left(y + \frac{a^2}{4} x\right) \right\}$$

To reduce this expression, it may perhaps be sufficiently general to suppose the symbol $\delta^{\frac{1}{2}}$ to include both the positive and negative signs, in which case we may write only one of the functions $e^{-a \sqrt{c} x \delta^{\frac{1}{2}}} f\left(y + \frac{a^2}{4} x\right)$.

Now $\frac{\sqrt{\pi}}{2} e^{-2v} = \int_0^\infty d\omega e^{-\left(\omega^2 + \frac{v^2}{\omega^2}\right)}$. (See GREGORY'S *Examples*, p. 499.)

Let $v = \frac{a x \sqrt{c} \delta^{\frac{1}{2}}}{2}$; then

$$\frac{\sqrt{\pi}}{2} e^{-a \sqrt{c} x \delta^{\frac{1}{2}}} = \int_0^\infty d\omega e^{-\left(\omega^2 + \frac{a^2 c x^2 \delta}{4 \omega^2}\right)}$$

and

$$z = e^{c x} e^{-a \sqrt{c} x \delta^{\frac{1}{2}}} f\left(y + \frac{a^2}{4} x\right)$$

$$= e^{c x} \frac{2}{\sqrt{\pi}} \int_0^\infty d\omega e^{-\left(\omega^2 + \frac{a^2 c x^2 \delta}{4 \omega^2}\right)} f\left(y + \frac{a^2}{4} x\right)$$

$$= \frac{2}{\sqrt{\pi}} e^{c x} \int_0^\infty d\omega e^{-\omega^2} f\left(y + \frac{a^2}{4} x - \frac{a^2 c x^2}{4 \omega^2}\right).$$

SECTION IV. DIFFERENCES.

22. The definition of the difference of u_x , as it is commonly written by English authors is $u_{x+1} - u_x$. We shall retain this definition, and generalize it by writing $e^{\frac{d}{dx}} u_x$ for u_{x+1} , and consequently $(e^{\frac{d}{dx}} - 1) u_x$ for Δu_x .

The results which we shall produce from this definition, as applied to frac-

tional values of the index of difference will, in most cases, differ not at all from the results obtained in the ordinary calculus of differences. We offer them only for the purpose of exhibiting those formulæ which possess all the generality which can be desired, at a single glance.

Suppose, then, $\Delta u_x = f(u_x) = v_x$

$$\therefore \Delta^2 u_x = \Delta v_x = (e^{\frac{d}{dx}} - 1) v_x = (e^{\frac{d}{dx}} - 1)^2 u_x, \text{ \&c.}$$

and, according to the axiom of the calculus of operations that the repetitions of equivalent operations are equivalent, we shall have generally $\Delta^n u_x = (e^{\frac{d}{dx}} - 1)^n u_x$: whatever n may be. This, then, may be said to be the *definition* of $\Delta^n u_x$.

Also, since $u_{x+n} = e^{\frac{n}{dx}} u_x$ by TAYLOR'S Theorem, and $\Delta u_x = (e^{\frac{d}{dx}} - 1) u_x$; it follows that $u_{x+n} = (1 + \Delta)^n u_x$.

We proceed now to apply it to the demonstration of the theorems which connect together $\Delta^r u_{x+s}$, and u_{x+p} , &c.

$$(1). \quad \Delta^n u_x = (e^{\frac{d}{dx}} - 1)^n u_x = \left(e^{\frac{n}{dx}} - n e^{\frac{(n-1)}{dx}} + \frac{n(n-1)}{1 \cdot 2} e^{\frac{(n-2)}{dx}} - \&c. \right) u_x \\ = u_{x+n} - n u_{x+n-1} + \frac{n(n-1)}{1 \cdot 2} u_{x+n-2} - \&c.$$

COR. 1. If $n = -1$; $\Delta^{-1} u_x = u_{x-1} + u_{x-2} + u_{x-3} + \&c.$

or $\Sigma u_x = u_{x-1} + u_{x-2} + u_{x-3} + \&c.$ together with an arbitrary constant;

or $u_x = \Delta u_{x-1} + \Delta u_{x-2} + \Delta u_{x-3} + \&c.$

COR. 2. If $n = -2$; $\Sigma^2 u_x = u_{x-2} + 2 u_{x-3} + 3 u_{x-4} + \&c.$ together with $A + Bx$.

$$(2). \quad \Delta^n u_x = (-1)^n (1 - e^{\frac{d}{dx}})^n u_x = (-1)^n \left(1 - n e^{\frac{d}{dx}} + \frac{n(n-1)}{1 \cdot 2} e^{\frac{2}{dx}} - \&c. \right) u_x \\ = (-1)^n (u_x - n u_{x+1} + \frac{n(n-1)}{1 \cdot 2} u_{x+2} - \&c.)$$

COR. 1. If $n = -1$, $\Delta^{-1} u_x = \Sigma u_x = -(u_x + u_{x+1} + u_{x+2} + \&c.)$; to which we may add an arbitrary constant.

COR. 2. If $n = -2$, $\Sigma^2 u_x = u_x + 2 u_{x+1} + 3 u_{x+2} + \&c.$, together with $A + Bx$.

$$(3). \quad \Delta^n u_x = e^{\frac{n}{dx}} \left(\frac{e^{\frac{d}{dx}} - 1}{e^{\frac{d}{dx}}} \right)^n u_x = e^{\frac{n}{dx}} \left(\frac{e^{\frac{d}{dx}}}{e^{\frac{d}{dx}} - 1} \right)^{-n} u_x \\ = e^{\frac{n}{dx}} \left(1 + (e^{\frac{d}{dx}} - 1)^{-1} \right)^{-n} u_x$$

$$\begin{aligned}
&= e^{\frac{n}{d}x} \left\{ 1 - n(e^{\frac{d}{d}x} - 1)^{-1} + \frac{n(n+1)}{1 \cdot 2} (e^{\frac{d}{d}x} - 1)^{-2} - \&c. \right\} \\
&= e^{\frac{n}{d}x} \left\{ u_x - n \Delta^{-1} u_x + \frac{n(n+1)}{1 \cdot 2} \Delta^{-2} u_x - \&c. \right\} \\
&= u_{x+n} - n \Delta^{-1} u_{x+n} + \frac{n(n+1)}{1 \cdot 2} \Delta^{-2} u_{x+n} - \&c.
\end{aligned}$$

or

$$= u_{x+n} - n \Sigma u_{x+n} + \frac{n(n+1)}{1 \cdot 2} \Sigma^2 u_{x+n} - \&c.$$

$$\begin{aligned}
(4). \quad \Delta^n u_x &= (-1)^{-n} \left(1 - \frac{e^{\frac{d}{d}x}}{e^{\frac{d}{d}x} - 1} \right)^{-n} u_x \\
&= (-1)^{-n} \left\{ 1 + n e^{\frac{d}{d}x} (e^{\frac{d}{d}x} - 1)^{-1} + \frac{n(n+1)}{1 \cdot 2} e^{\frac{2d}{d}x} (e^{\frac{d}{d}x} - 1)^{-2} + \&c. \right\} u_x \\
&= (-1)^{-n} \left\{ u_x + n \Sigma u_{x+1} + \frac{n(n+1)}{1 \cdot 2} \Sigma^2 u_{x+2} + \&c. \right\}
\end{aligned}$$

$$\begin{aligned}
(5). \quad \Delta^n u_x &= \left(\frac{e^{\frac{d}{d}x}}{e^{\frac{d}{d}x} - 1} - 1 \right)^{-n} u_x \\
&= \left\{ e^{-\frac{n}{d}x} (e^{\frac{d}{d}x} - 1)^n + n e^{-(n+1)\frac{d}{d}x} (e^{\frac{d}{d}x} - 1)^{n+1} + \&c. \right\} u_x \\
&= \Delta^n u_{x-n} + n \Delta^{n+1} u_{x-n-1} + \frac{n(n+1)}{1 \cdot 2} \Delta^{n+2} u_{x-n-2} + \&c.
\end{aligned}$$

These formulæ are all quite independent of the value of n , and serve to connect the n th difference of a function of x with differences of functions of $x+n$, &c., $x+1$, &c.

We shall now obtain the converse series of connections, those of u_{x+n} with u_x , &c.

$$\begin{aligned}
(6). \quad u_{x+n} &= (1 + \Delta)^n u_x = \left(1 + n \Delta + \frac{n(n-1)}{1 \cdot 2} \Delta^2 + \&c. \right) u_x \\
&= u_x + n \Delta u_x + \frac{n(n-1)}{1 \cdot 2} \Delta^2 u_x + \&c.
\end{aligned}$$

$$\begin{aligned}
(7). \quad u_{x+n} &= (\Delta + 1)^n u_x = \left(\Delta^n + n \Delta^{n-1} + \frac{n(n-1)}{1 \cdot 2} \Delta^{n-2} + \&c. \right) u_x \\
&= \Delta^n u_x + n \Delta^{n-1} u_x + \frac{n(n-1)}{1 \cdot 2} \Delta^{n-2} u_x + \&c.
\end{aligned}$$

If n were a positive integer, formula (1) would coincide with formula (2); and formula (6) with formula (7); but in our present calculus they are by no means the same thing.

$$\begin{aligned}
 (8.) \quad u_{x+n} &= \left(\frac{1}{1+\Delta}\right)^{-n} u_x = \left(1 - \frac{\Delta}{1+\Delta}\right)^{-n} u_x \\
 &= \left(1 + n \frac{\Delta}{1+\Delta} + \frac{n(n+1)}{1 \cdot 2} \frac{\Delta^2}{(1+\Delta)^2} + \&c.\right) u_x \\
 &= u_x + n \Delta u_{x-1} + \frac{n(n+1)}{1 \cdot 2} \Delta^2 u_{x-2} + \&c.
 \end{aligned}$$

COR. If $n=1$, $u_{x+1} = u_x + \Delta u_{x-1} + \Delta^2 u_{x-2} + \&c.$

$$\begin{aligned}
 (9.) \quad u_{x+n} &= (-1)^{-n} \left(\frac{\Delta}{1+\Delta} - 1\right)^{-n} u_x \\
 &= (-1)^{-n} (\Delta^{-n} (1+\Delta)^n + n \Delta^{-(n+1)} (1+\Delta)^{n+1} + \&c.) u_x \\
 &= (-1)^{-n} \left\{ \Sigma^n u_{x+n} + n \Sigma^{n+1} u_{x+n+1} + \frac{n(n+1)}{1 \cdot 2} \Sigma^{n+2} u_{x+n+2} + \&c. \right\}
 \end{aligned}$$

In strictness we ought to write Δ^{-n} for Σ^n , but the latter notation is more familiar to the eye.

$$\begin{aligned}
 (10.) \quad u_{x+n} &= \Delta^n \left(\frac{1+\Delta}{\Delta}\right)^n u_x = \Delta^n \left(1 - \frac{1}{1+\Delta}\right)^{-n} u_x \\
 &= \Delta^n \left\{ 1 + n (1+\Delta)^{-1} + \frac{n(n+1)}{1 \cdot 2} (1+\Delta)^{-2} + \&c. \right\} u_x \\
 &= \Delta^n u_x + n \Delta^n u_{x-1} + \frac{n(n+1)}{1 \cdot 2} \Delta^n u_{x-2} + \&c.
 \end{aligned}$$

Formula (10) is a particular form of formula (1), for by formula (1), $\Delta^{-m} u_{x+m} = u_x + m u_{x-1} + \&c.$, which is reduced to (10) by multiplying by Δ^m . In the same manner we may reproduce formula (2.)

The last class of relations which we shall produce are such as do not depend on the general expansion of the binomial.

$$\begin{aligned}
 (11.) \quad u_{x+n} &= e^{n \frac{d}{dx}} u_x = e^{n \frac{d}{dx}} \frac{e^{\frac{d}{dx}} - 1}{\frac{d}{dx} - 1} u_x \\
 &= (e^{\frac{d}{dx}} - 1) (e^{(n-1) \frac{d}{dx}} + e^{(n-2) \frac{d}{dx}} + e^{(n-3) \frac{d}{dx}} + \&c.) u_x \\
 &= (e^{\frac{d}{dx}} - 1) (u_{x+n-1} + u_{x+n-2} + u_{x+n-3} + \&c.) \\
 &= \Delta u_{x+n-1} + \Delta u_{x+n-2} + \Delta u_{x+n-3} + \&c.
 \end{aligned}$$

COR. 1. If $n=0$; $u_x = \Delta u_{x-1} + \Delta u_{x-2} + \Delta u_{x-3} + \&c.$,

which coincides with Cor. 1., formula (1.)

COR. 2. If n be a positive integer

$$\begin{aligned}
 u_{x+n} &= \Delta u_{x+n-1} + \Delta u_{x+n-2} + \&c. + \Delta u_{x+1} + \Delta u_x + \Delta u_{x-1} + \&c. \\
 &= \Delta u_{x+n-1} + \Delta u_{x+n-2} + \&c. + \Delta u_{x+1} + \Delta u_x + u_x; \text{ by Cor. 1.}
 \end{aligned}$$

$$\begin{aligned}
 (12.) \quad u_{x+n} &= -e^{\frac{n}{d}x} \frac{e^{\frac{d}{d}x} - 1}{1 - e^{\frac{d}{d}x}} u_x \\
 &= -(e^{\frac{d}{d}x} - 1) \left(e^{\frac{n}{d}x} + e^{\frac{(n+1)}{d}x} + e^{\frac{(n+2)}{d}x} + \&c. \right) u_x \\
 &= -(\Delta u_{x+n} + \Delta u_{x+n+1} + \Delta u_{x+n+2} + \&c.)
 \end{aligned}$$

COR. If $n=0$, $\Sigma u_x = -(u_x + u_{x+1} + u_{x+2} + \&c.)$,

which coincides with Cor. 1, formula (2.)

$$\begin{aligned}
 (13.) \quad u_{x+n} &= e^{\frac{(n-1)}{d}x} \frac{d}{d} e^{\frac{d}{d}x} u_x = e^{\frac{(n-1)}{d}x} \frac{1}{1 - \frac{e^{\frac{d}{d}x} - 1}{e^{\frac{d}{d}x}}} u_x \\
 &= e^{\frac{(n-1)}{d}x} \cdot \left(1 + \frac{e^{\frac{d}{d}x} - 1}{\frac{d}{d}} + \left(\frac{e^{\frac{d}{d}x} - 1}{\frac{d}{d}} \right)^2 + \&c. \right) u_x \\
 &= u_{x+n-1} + \Delta u_{x+n-2} + \Delta^2 u_{x+n-3} + \&c.
 \end{aligned}$$

which coincides with the Cor. to formula (8).

Thus formulæ (1), (2), and (8), include formulæ (11), (12), and (13).

It is evident that by the same process all the ordinary formulæ in finite differences, which are usually obtained by the aid of generating functions, may be easily obtained.

For example the following:

$$\begin{aligned}
 (14.) \quad u_{x+n} &= (n+1) \left\{ u_x + \frac{(n+1)^2 - 1^2}{1 \cdot 2 \cdot 3} \Delta^2 u_{x-1} + \frac{(n+1)^2 - 1^2 \cdot (n+1)^2 - 2^2}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} \Delta^4 u_{x-2} + \&c. \right\} \\
 &\quad - n \left\{ u_{x-1} + \frac{n^2 - 1^2}{1 \cdot 2 \cdot 3} \Delta^2 u_{x-2} + \&c. \right\}
 \end{aligned}$$

We have

$$\begin{aligned}
 \frac{1}{1-a(1+\Delta)} &= \frac{1 - \frac{a}{1+\Delta}}{\left\{ 1 - a(1+\Delta) \right\} \left\{ 1 - \frac{a}{1+\Delta} \right\}} = \\
 &= \frac{1}{(1-a)^2 - \frac{a \Delta^2}{1+\Delta}} - \frac{\frac{a}{1+\Delta}}{(1-a)^2 - \frac{a \Delta^2}{1+\Delta}}
 \end{aligned}$$

Now $\frac{1}{(1-a)^2 - az}$ when expanded in terms of a , gives as the coefficient of a^n ,

$$(n+1) \left\{ 1 + \frac{(n+1)^2 - 1^2}{1 \cdot 2 \cdot 3} z + \&c. \right\}$$

Hence, if we equate the coefficients of α^n in the two equivalent expressions

$$\frac{1}{1-\alpha(1+\Delta)} u_x \text{ and } \left(\frac{1}{(1-\alpha)^2 - \frac{\alpha \Delta^2}{1+\Delta}} - \frac{\frac{\alpha}{1+\Delta}}{(1-\alpha)^2 - \frac{\alpha \Delta^2}{1+\Delta}} \right) u_x$$

the result will be

$$\begin{aligned} (1+\Delta)^n u_x &= (n+1) \left\{ 1 + \frac{(n+1)^2 - 1^2}{1 \cdot 2 \cdot 3} \frac{\Delta^2}{1+\Delta} + \&c. \right\} u_x \\ &\quad - n \left\{ 1 + \frac{n^2 - 1^2}{1 \cdot 2 \cdot 3} \frac{\Delta^2}{1+\Delta} + \&c. \right\} \frac{1}{1+\Delta} u_x \\ \text{or} \quad u_{x+n} &= (n+1) \left\{ u_x + \frac{(n+1)^2 - 1^2}{1 \cdot 2 \cdot 3} \Delta^2 u_{x-1} + \&c. \right\} \\ &\quad - n \left\{ u_{x-1} + \frac{n^2 - 1^2}{1 \cdot 2 \cdot 3} \Delta^2 u_{x-2} + \&c. \right\} \end{aligned}$$

24. Let us apply these formulæ to examples.

Ex. 1. Let $u_x = e^{\alpha x}$, then

$$\Delta^n u_x = (e^{\frac{\Delta}{1+\Delta}} - 1)^n e^{\alpha x} = (e^\alpha - 1)^n e^{\alpha x} \text{ (by A.)}$$

Ex. 2. Let $u_x = x$, $n = \frac{1}{2}$, then, formula (2),

$$\begin{aligned} \Delta^{\frac{1}{2}} x &= \sqrt{-1} \left\{ x - \frac{1}{2}(x+1) + \frac{\frac{1}{2}(\frac{1}{2}-1)}{1 \cdot 2} (x+2) - \frac{\frac{1}{2}(\frac{1}{2}-1)(\frac{1}{2}-2)}{1 \cdot 2 \cdot 3} (x+3) + \&c. \right\} \\ &= \sqrt{-1} \left\{ x - \frac{1}{2}x + \frac{\frac{1}{2}(\frac{1}{2}-1)}{1 \cdot 2} x - \frac{\frac{1}{2}(\frac{1}{2}-1)(\frac{1}{2}-2)}{1 \cdot 2 \cdot 3} x + \&c. \right. \\ &\quad \left. - \frac{1}{2} \left(1 + \frac{\frac{1}{2}}{1} + \frac{\frac{1}{2} \cdot \frac{3}{2}}{1 \cdot 2} + \&c. \right) \right\} \\ &= \sqrt{-1} x (1-1)^{\frac{1}{2}} - \frac{\sqrt{-1}}{2} (1-1)^{-\frac{1}{2}} \\ &= \infty \end{aligned}$$

$$\begin{aligned} \text{Ex. 3.} \quad \Delta^n x &= (-1)^n \left(x - n(x+1) + \frac{n(n-1)}{1 \cdot 2} (x+2) - \&c. \right) \\ &= (-1)^n x (1-1)^n - n(-1)^n \left(1 - \frac{n-1}{1} + \frac{(n-1)(n-2)}{1 \cdot 2} - \&c. \right) \\ &= (-1)^n x (1-1)^n - n(-1)^n (1-1)^{n-1} \end{aligned}$$

which is zero when n is greater than 1, finite only when $n=1$, in which case it is 1; and infinite when n is less than 1.

It is evident that this introduction of ∞ may indicate simply that the form of the expansion is incorrect: for $\Delta x = \infty(x+1) - \infty x = x + \text{const.}$ is the analytical result of the equation $\Delta x = (x+1) - x = 0x + \text{const.}$, by dividing both sides by the symbol 0.

When n is less than 1, therefore, it is necessary to seek some other method of obtaining the n th difference. The following method, analogous to that by which we obtained the n th differential coefficient of a logarithm in Art. 2 appears to be the most simple.

Let x be represented by $\frac{e^{p \cdot x} - e^{q \cdot x}}{p}$, where q is of a higher order than p , and both are 0.

Then

$$\begin{aligned} \Delta^n x &= \frac{(e^p - 1)^n e^{p \cdot x} - (e^q - 1)^n e^{q \cdot x}}{p} \\ &= \frac{(p + \frac{p^2}{1 \cdot 2} + \frac{p^3}{1 \cdot 2 \cdot 3} + \&c.)^n e^{p \cdot x} - (q + \&c.)^n e^{q \cdot x}}{p} \\ &= \frac{(p + \frac{p^2}{1 \cdot 2} + \&c.)^n (1 + p x + \&c.) - (q + \frac{q^2}{1 \cdot 2} + \&c.)^n (1 + q x + \&c.)}{p} \\ &= \frac{p^n + \frac{n p^{n+1}}{2} + \frac{n(3n+1)}{24} p^{n+2} + \&c. - q^n - \frac{n q^{n+1}}{2} - \frac{n(3n+1)}{24} q^{n+2} + \&c.}{p} \\ &\quad + \frac{p + \frac{n+1}{2} n p^{n+2} + \&c. - q^{n+1} - \frac{n q^{n+2}}{2} - \&c.}{p} x \\ &\quad + \frac{\frac{p^{n+2}}{2} + \&c. - \frac{q^{n+2}}{2} + \&c.}{p} x^2 \\ &\quad + \&c. \end{aligned}$$

If $n > 0 < 1$, every part vanishes except the constant, which is infinite: if $n = 1$, $\Delta x = 1$; if $n > 1$, every term is zero.

If n is negative, there will still exist the infinite constant which may be regarded as part of the arbitrary constant; there will also exist in some instances infinite functions of x , which, as will easily be seen, may be considered in those cases as part of the arbitrary functions.

Let $n = -1$; then

$$\begin{aligned} \Delta^{-1} x &= \text{const.} - \frac{\frac{1}{2} p + \&c.}{p} x + \frac{1}{2} x^2 \\ &= \text{const.} + \frac{x(x-1)}{2} \end{aligned}$$

Let $n = -2$; and

$$\Delta^{-2} x = \text{const.} + \text{const.} x - \frac{x^2}{2} + \frac{x^3}{6}$$

and so on.

Ex. 4. $\Delta^n x^m = (x+n)_m - n(x+n-1)^m + \frac{n(n-1)}{1 \cdot 2} (x+n-2)^m - \&c.$

by the first formula.

Ex. 5.
$$\Delta^n \frac{1}{(a+x)^m} = (-1)^n \left\{ \frac{1}{(a+x)^m} - \frac{n}{(a+x+1)^m} + \&c. \right\}$$

$$= (-1)^n \Delta_{x'}^n \frac{1}{x'^m}; \text{ if } x' = a+x$$

Ex. 6. To find $\Delta^{\frac{1}{2}} \frac{1}{x}$.

By formula (2), $\Delta^{\frac{1}{2}} \frac{1}{x} = \sqrt{-1} \left\{ \frac{1}{x} - \frac{\frac{1}{2}}{x+1} - \frac{1 \cdot 1}{2 \cdot 4} \frac{1}{x+2} - \&c. \right\}$

Let
$$v = \frac{y^x}{x} - \frac{1}{2} \frac{y^{x+1}}{x+1} - \&c.$$

then
$$\frac{dv}{dy} = y^{x-1} \left(1 - \frac{1}{2} y - \frac{1 \cdot 1}{2 \cdot 4} \cdot y^2 - \&c. \right)$$

$$= y^{x-1} (1-y)^{\frac{1}{2}}$$

and
$$\Delta^{\frac{1}{2}} \frac{1}{x} = \sqrt{-1} \int_0^1 y^{x-1} (1-y)^{\frac{1}{2}} dy$$

$$= \sqrt{-1} \sqrt{-\left(x, \frac{3}{2}\right)} = \sqrt{-1} \frac{\sqrt{x} \sqrt{\frac{3}{2}}}{x + \frac{3}{2}}$$

Ex. 7. To find $\Delta^n \frac{1}{x}$.

$$\Delta^n \frac{1}{x} = (-1)^n \left\{ \frac{1}{x} - \frac{n}{x+1} + \frac{n(n-1)}{1 \cdot 2} \frac{1}{x+2} - \&c. \right\}$$

Let
$$v = \frac{y^x}{x} - \frac{n y^{x+1}}{x+1} + \&c.$$

then
$$\frac{dv}{dy} = y^{x-1} (1-y)^n$$

and
$$\Delta^n \frac{1}{x} = (-1)^n \int_0^1 y^{x-1} (1-y)^n dy$$

$$= (-1)^n \sqrt{-\left(x, n+1\right)} = (-1)^n \frac{\sqrt{x} \sqrt{n+1}}{x+n+1}$$

COR. 1. If n be a whole number, $\sqrt{x+n+1} = x(x+1) \dots (x+n) \sqrt{x}$

$\therefore \Delta^n \frac{1}{x} = (-1)^n \frac{1 \cdot 2 \dots n}{x(x+1) \dots (x+n)}$

COR. 2. If $n = \frac{1}{2}$, $\Delta^{\frac{1}{2}} \frac{1}{x} = \sqrt{-1} \frac{\sqrt{x} \sqrt{\frac{3}{2}}}{x - \frac{3}{2}} = \frac{1}{2} \sqrt{-1} \sqrt{\pi} \frac{\sqrt{x}}{x + \frac{3}{2}}$

Ex. 8. To find, $\Delta^n \frac{1}{x^m}$, m being any integer.

$$\Delta^n \frac{1}{x^m} = (-1)^n \left\{ \frac{1}{x^m} - n \frac{1}{(x+1)^m} + \frac{n(n-1)}{1 \cdot 2} \frac{1}{(x+2)^m} - \&c. \right\} \text{ by formula (2).}$$

Let
$$v = \frac{y^x}{x^m} - n \frac{y^{x+1}}{(x+1)^m} + \&c.$$

then
$$\left(y \frac{d}{dy}\right)^m v = y^x (1-y)^n$$

$$\therefore v = \int_0^1 \frac{dy}{y} \int_0^1 \frac{dy}{y} \dots (m \text{ times}) \dots y^x (1-y)^n.$$

and
$$\Delta^n \frac{1}{x^m} = (-1)^n \int_0^1 \frac{dy}{y} \int_0^1 \frac{dy}{y} \int_0^1 \frac{dy}{y} \dots y^x (1-y)^n.$$

Ex. 9. To find $\Delta^n \sin ax$.

$$\begin{aligned} \Delta^n \sin ax &= \frac{1}{2\sqrt{-1}} (e^{\frac{d}{dx}} - 1)^n (e^{ax\sqrt{-1}} - e^{-ax\sqrt{-1}}) \\ &= \frac{1}{2\sqrt{-1}} \left\{ (e^{a\sqrt{-1}} - 1)^n e^{ax\sqrt{-1}} - (e^{-a\sqrt{-1}} - 1)^n e^{-ax\sqrt{-1}} \right\} \\ &= \frac{1}{2\sqrt{-1}} \left\{ \left(e^{\frac{a}{2}\sqrt{-1}} - e^{-\frac{a}{2}\sqrt{-1}} \right)^n e^{\frac{na}{2}\sqrt{-1} + ax\sqrt{-1}} \right. \\ &\quad \left. - (-1)^n \left(e^{\frac{a}{2}\sqrt{-1}} - e^{-\frac{a}{2}\sqrt{-1}} \right)^n e^{-\frac{na}{2}\sqrt{-1} + ax\sqrt{-1}} \right\} \\ &= \frac{1}{2\sqrt{-1}} \left(2\sqrt{-1} \sin \frac{a}{2} \right)^n \left\{ \left(\cos ax + \frac{na}{2} + \sqrt{-1} \sin ax + \frac{na}{2} \right) \right. \\ &\quad \left. - (\cos 2\lambda + 1)n\pi - \sqrt{-1} \sin 2\lambda + 1n\pi \right\} \left(\cos ax + \frac{na}{2} - \sqrt{-1} \sin ax + \frac{na}{2} \right) \} \\ &= (2\sqrt{-1})^{n-1} \sin \frac{na}{2} \left\{ \cos \left(ax + \frac{na}{2} \right) + \sqrt{-1} \sin \left(ax + \frac{na}{2} \right) \right. \\ &\quad \left. - \cos \left(2\lambda + 1n\pi + ax + \frac{na}{2} \right) + \sqrt{-1} \sin \left(2\lambda + 1n\pi + ax + \frac{na}{2} \right) \right\} \\ &= 2^n (\sqrt{-1})^{n-1} \sin^n \frac{a}{2} \sin \left(ax + \frac{na}{2} + \frac{2\lambda+1}{2}n\pi \right) \left(\sin 2\lambda + 1 \frac{n\pi}{2} \&c. \right. \\ &\quad \left. = 2^n (\cos nr\pi - \sqrt{-1} \sin nr\pi) \sin^n \frac{a}{2} \sin \left(ax + \frac{na}{2} + \frac{(2\lambda+1)}{2}n\pi \right) \right); \end{aligned}$$

r and λ being any integers.

Ex. 10. To find $\Delta^n e^{mx} \sin ax$.

$$\begin{aligned} \Delta^n e^{mx} \sin ax &= \frac{1}{2\sqrt{-1}} (e^{\frac{d}{dx}} - 1)^n (e^{mx+ax\sqrt{-1}} - e^{mx-ax\sqrt{-1}}) \\ &= \frac{1}{2\sqrt{-1}} \left\{ (e^{m+a\sqrt{-1}} - 1)^n e^{mx+ax\sqrt{-1}} - (e^{m-a\sqrt{-1}} - 1)^n e^{mx-ax\sqrt{-1}} \right\} \\ &= \frac{e^{mx}}{2\sqrt{-1}} \left\{ (e^m \cos a + e^m \sqrt{-1} \sin a - 1)^n (\cos ax + \sqrt{-1} \sin ax) \right. \end{aligned}$$

$$-(e^m \cos a - e^m \sqrt{-1} \sin a - 1)^n (\cos ax - \sqrt{-1} \sin ax)\}$$

Let $e^m \cos a - 1 = P \cos \theta$, $e^m \sin a = P \sin \theta$;

then $P^2 = e^{2m} - 2e^m \cos a + 1$, and $\tan \theta = \frac{e^m \sin a}{e^m \cos a - 1}$

$$\begin{aligned} \therefore \Delta^n e^{mx} \sin ax &= \frac{e^{mx}}{2\sqrt{-1}} P^n \{(\cos n\sqrt{2\lambda\pi + \theta} + \sqrt{-1} \sin n\sqrt{2\lambda\pi + \theta}) (\cos ax + \sqrt{-1} \sin ax) \\ &\quad - (\cos 2\lambda'\pi + \theta - \sqrt{-1} \sin 2\lambda'\pi + \theta) (\cos ax - \sqrt{-1} \sin ax)\} \\ &= \frac{e^{mx} P^n}{\sqrt{-1}} \{\sqrt{-1} \cos n(\lambda' - \lambda)\pi + \sin n(\lambda' - \lambda)\pi\} \sin(ax + n\theta + n\sqrt{\lambda + \lambda'}\pi) \\ &= e^{mx} P^n (\cos r n \pi - \sqrt{-1} \sin r n \pi) \sin(ax + n\theta + n\lambda\pi) \end{aligned}$$

r and λ being any integers.

Similar expressions may be obtained for the n th differences of $\cos ax$ and of $e^{mx} \cos ax$.

25. We shall now proceed to the demonstration of certain theorems analogous to those in the ordinary calculus of differences.

PROP. 1.
$$\frac{1}{v_{x+n}} = \frac{1}{v_x} - \frac{nb}{v_x v_{x+1}} + \frac{n(n-1) \cdot b^2}{v_x v_{x+1} v_{x+2}} - \&c.$$

where $v_x = a + bx$ or $\Delta v_x = b$.

By formula (6); putting $\frac{1}{v_x}$ for u_x

$$\frac{1}{v_{x+n}} = \frac{1}{v_x} + n \Delta \frac{1}{v_x} + \frac{n(n-1)}{1 \cdot 2} \Delta^2 \frac{1}{v_x} + \&c.$$

Now $\Delta \frac{1}{v_x} = -\frac{\Delta v_x}{v_x v_{x+1}} = -\frac{b}{v_x v_{x+1}}$, $\Delta^2 \frac{1}{v_x} = \frac{1 \cdot 2 \cdot b^2}{v_x v_{x+1} v_{x+2}}$ &c, &c.

$$\therefore \frac{1}{v_{x+n}} = \frac{1}{v_x} - \frac{nb}{v_x v_{x+1}} + \frac{n(n-1)b^2}{v_x v_{x+1} v_{x+2}} \&c.$$

PROP. 2. A similar result may be obtained from formula (8).

For $\Delta \frac{1}{v_{x-1}} = -\frac{b}{v_x v_{x-1}}$, $\Delta^2 \frac{1}{v_{x-1}} = \frac{1 \cdot 2 \cdot b^2}{v_x v_{x-1} v_{x-2}}$ &c.

$$\therefore \frac{1}{v_{x+n}} = \frac{1}{v_x} - \frac{nb}{v_x v_{x-1}} + \frac{n(n+1)b^2}{v_x v_{x-1} v_{x-2}} - \&c.$$

PROP. 3. $\Delta^n u_x v_x = v_x \Delta^n u_x + n \Delta v_x \Delta^{n-1} u_{x+1} + \&c.$

For $\{(1 + \Delta)(1 + \Delta') - 1\} u_x v_x$ being an operation on $u_x v_x$ may be repeated according to any law, consequently

$\Delta^n u_x v_x = \{(1 + \Delta)(1 + \Delta') - 1\}^n u_x v_x$: and every step in the demonstration is the same as when n is a whole number.

COR. The same is true of the formula for the n th difference of $u_{x,y}$: for

$$\begin{aligned}\Delta u_{x,y} &= \{ (1 + \Delta_x) (1 + \Delta_y) - 1 \} u_{x,y} \\ \therefore \Delta^n u_{x,y} &= \{ \overline{1 + \Delta_x} \overline{1 + \Delta_y} - 1 \}^n u_{x,y} \\ \text{or} &= (e^{\frac{d}{dx} + \frac{d}{dy}} - 1)^n u_{x,y}\end{aligned}$$

and the same results are produced as when n is a whole number.

PROP. 4. $F(\Delta) e^{rx} f(x) = e^{rx} F(e^r \overline{1 + \Delta} - 1) f(x)$

Let $u_x = e^{rx}$; and $v_x = f(x)$ in Prop. 3.

$$\begin{aligned}\therefore \Delta^n e^{rx} f(x) &= f(x) \Delta^n e^{rx} + n \Delta f(x) \Delta^{n-1} e^{rx+r} + \&c. \\ &= f(x) (e^r - 1)^n e^{rx} + n \Delta f(x) (e^r - 1)^{n-1} e^{rx+r} + \&c. \\ &= e^{rx} (e^r - 1 + e^r \Delta)^n f(x) \\ &= e^{rx} (e^r \overline{1 + \Delta} - 1)^n f(x)\end{aligned}$$

which being true for all values of n , shews that the following theorem is also true:

$$F(\Delta) \cdot e^{rx} f(x) = e^{rx} F(e^r \overline{1 + \Delta} - 1) \cdot f(x)$$

$$\text{PROP. 5.} \quad \Delta u_x = \left(\left(1 + \frac{1}{x} \right)^D - 1 \right) u_\theta = \left\{ (1 + \Delta_\theta)^{\log \left(1 + \frac{1}{x} \right)} - 1 \right\} u_\theta$$

Let $x = e^\theta$, and let u_x be represented by u_θ when e^θ is written for x , let also Δ_θ be the symbol of difference $u_{\theta+1} - u_\theta$. Then by (C), when n is an integer,

$$\begin{aligned}x^n \frac{d^n u_x}{dx^n} &= D(D-1)(D-2) \dots (D-n+1) u_\theta \\ \therefore \Delta u_x &= (e^{\frac{d}{dx}} - 1) u_x = \left(\frac{d}{dx} + \frac{1}{1 \cdot 2} \left(\frac{d}{dx} \right)^2 + \frac{1}{1 \cdot 2 \cdot 3} \left(\frac{d}{dx} \right)^3 + \&c. \right) u_x \\ &= \left(\frac{1}{x} D + \frac{1}{1 \cdot 2} \frac{1}{x^2} D(D-1) + \frac{1}{1 \cdot 2 \cdot 3} \frac{1}{x^3} D(D-1)(D-2) + \&c. \right) u_\theta \\ &= \left(\left(1 + \frac{1}{x} \right)^D - 1 \right) u_\theta\end{aligned}$$

$$\text{But} \quad \Delta_\theta u = (e^D - 1) u_\theta \quad \therefore e^D u_\theta = (1 + \Delta_\theta) u_\theta$$

$$\text{or} \quad D = \log(1 + \Delta_\theta).$$

$$\text{Hence} \quad \left(1 + \frac{1}{x} \right)^D = \left(1 + \frac{1}{x} \right)^{\log(1 + \Delta_\theta)} = (1 + \Delta_\theta)^{\log \left(1 + \frac{1}{x} \right)}$$

$$\text{and} \quad \Delta u_x = \left(\left(1 + \frac{1}{x} \right)^D - 1 \right) u_\theta = \left\{ (1 + \Delta_\theta)^{\log \left(1 + \frac{1}{x} \right)} - 1 \right\} u_\theta$$

$$\text{COR.} \quad u_{x+n} = (1 + \Delta_\theta)^{\log \left(1 + \frac{1}{x} \right)} u_\theta$$

PROP. 6. $u_{x+n} = (1 + \Delta_\theta)^{\log \left(1 + \frac{n}{x}\right)} u_\theta$

For $u_{x+n} = e^{\frac{n}{d} \frac{d}{x} u_x} = \left(1 + n \frac{d}{dx} + \frac{n^2 \left(\frac{d}{dx}\right)^2}{1 \cdot 2} + \&c.\right) u_\theta$

$$= \left(1 + \frac{n}{x} D + \frac{1}{1 \cdot 2} \frac{n^2}{x^2} D(D-1) + \&c.\right) u_\theta$$

$$= \left(\left(1 + \frac{n}{x}\right)^D - 1\right) u_\theta$$

$$= (1 + \Delta_\theta)^{\log \left(1 + \frac{n}{x}\right)} u_\theta$$

It must be observed that x is considered constant with respect to Δ_θ in the formula $\left(1 + \frac{1}{x}\right)^{\log(1 + \Delta_\theta)}$. Had we supposed it otherwise, we must have taken account of the differential coefficients of $\frac{1}{x}$ itself. This would have given the following theorem.

PROP. 7. $\Delta u_x = \{(1 - e^{-\theta})^{-(D+1)} - 1\} u_\theta$

$$= \{(1 + \Delta_\theta)^{-\log(1 - e^{-\theta})} (1 - e^{-\theta})^{-1} - 1\} u_\theta$$

For $\Delta u_x = (e^{\frac{d}{dx}} - 1) u_x = \left(\frac{d}{dx} + \frac{1}{1 \cdot 2} \left(\frac{d}{dx}\right)^2 + \&c.\right) u_x$

$$= \left(\frac{1}{x} D + \frac{1}{1 \cdot 2} \frac{1}{x^2} D(D-1) + \&c.\right) u_\theta$$

$$= \left((D+1)e^{-\theta} + \frac{1}{1 \cdot 2} (D+2)(D+1)e^{-2\theta} + \&c.\right) u_\theta \quad (B)$$

$$= \{(1 - e^{-\theta})^{-(D+1)} - 1\} u_\theta$$

$$= \{(1 - e^{-\theta})^{-\log(1 + \Delta_\theta)} - 1\} u_\theta$$

$$= \{(1 + \Delta_\theta)^{-\log(1 - e^{-\theta})} (1 - e^{-\theta})^{-1} - 1\} u_\theta$$

PROP. 8. $u_{x+n} = e^{\frac{n}{d} \frac{d}{x} u_x} = \left(1 + n \frac{d}{dx} + \frac{n^2}{1 \cdot 2} \left(\frac{d}{dx}\right)^2 + \&c.\right) u_x$

$$= \left(1 + n(D+1)e^{-\theta} + \frac{n^2}{1 \cdot 2} (D+2)(D+1)e^{-2\theta} + \&c.\right) u_\theta$$

$$= (1 - n e^{-\theta})^{-(D+1)} u_\theta$$

$$= (1 + \Delta_\theta)^{-\log(1 - n e^{-\theta})} (1 - n e^{-\theta})^{-1} u_\theta$$

It is manifest that these formulæ do not follow the distributive law. They cannot, consequently, be applied with any great advantage to the solution of equations of differences. We shall exhibit their application only in one instance.

EQUATIONS OF DIFFERENCES.

26. As the method of solving equations of differences of the second and higher orders, by treating the symbols of operation as symbols of quantity, and reducing the resulting fraction by decomposing it into partial fractions, has been little, if at all, employed, we shall commence with an example or two in ordinary equations of differences.

Ex. 1. $u_{x+3} + a u_{x+2} + b u_{x+1} + c u_x = X.$

This may be written

$$\{(1+\Delta)^3 + a(1+\Delta)^2 + b(1+\Delta) + c\}u_x = X.$$

If we write Δ , for $1+\Delta$, and suppose a, β, γ the roots of the equation $\Delta^3 + a\Delta^2 + b\Delta + c = 0$; we get $(\Delta - a)(\Delta - \beta)(\Delta - \gamma)u_x = X$, or

$$u_x = \frac{1}{(\Delta - a)(\Delta - \beta)(\Delta - \gamma)} \cdot (X + 0).$$

This equation is reduced, by the decomposition of the fraction of operation into its equivalent partial fractions, to

$$u_x = \frac{1}{(a - \beta)(a - \gamma)} \frac{1}{(\Delta - a)} (X + 0) + \frac{1}{(\beta - a)(\beta - \gamma)} \frac{1}{(\Delta - \beta)} (X + 0) \\ + \frac{1}{(\gamma - a)(\gamma - \beta)} \frac{1}{(\Delta - \gamma)} (X + 0).$$

Now $\frac{1}{\Delta - a}(X + 0)$ is the solution of the equation $v_{x+1} - a v_x = X + 0$;

hence it is equal to $a^x \left(A + \Sigma \frac{X}{a^{x+1}} \right)$; and similarly of the others.

Hence the complete solution of the given equation is

$$u_x = \frac{1}{(a - \beta)(a - \gamma)} a^x \left(A + \Sigma \frac{X}{a^{x+1}} \right) + \frac{1}{(\beta - a)(\beta - \gamma)} \beta^x \left(B + \Sigma \frac{X}{\beta^{x+1}} \right) \\ + \frac{1}{(\gamma - a)(\gamma - \beta)} \gamma^x \left(C + \Sigma \frac{X}{\gamma^{x+1}} \right)$$

COR. If $a = \beta$, we must, as in similar cases, put $a + c$ for β , and expand in terms of c . The result is

$$u_x = -\frac{1}{c(a - \gamma)} a^x \left(A + \Sigma \frac{X}{a^{x+1}} \right) + \\ \frac{1}{a - \gamma} \left(\frac{a^x}{c} + x a^{x-1} - \frac{a^x}{a - \gamma} \right) \left(B + \Sigma \frac{X}{a^{x+1}} - c \Sigma \frac{X(x+1)}{a^{x+2}} \right) \\ + \frac{1}{(\gamma - a)^2} \gamma^x \left(C + \Sigma \frac{X}{\gamma^{x+1}} \right)$$

$$= \frac{A, \alpha^x}{\alpha - \gamma} + \frac{B x \alpha^{x-1}}{\alpha - \gamma} + \left(\frac{x \alpha^{x-1}}{\alpha - \gamma} - \frac{\alpha^x}{(\alpha - \gamma)^2} \right) \Sigma \frac{X}{\alpha^{x+1}} - \frac{\alpha^x}{\alpha - \gamma} \Sigma \frac{X(x+1)}{\alpha^{x+2}} \\ - \frac{1}{(\gamma - \alpha)^2} \gamma^x \left(C + \Sigma \frac{X}{\gamma^{x+1}} \right).$$

In precisely the same manner we may integrate the general equation with constant coefficients.

Let us apply the formula of Prop. 6 to the following example.

Ex. 2. $u_x - 3(x+1)u_{x+1} + 2(x+1)(x+2)u_{x+2} = 0.$

Calling $\left(1 + \frac{1}{x}\right)^l$ we get

$$u_\theta - 3(e^\theta + 1)(1 + \Delta_\theta)^l u_\theta + 2(e^\theta + 1)(e^\theta + 2)(1 + \Delta_\theta)^{\left(1 + \frac{2}{x}\right)} u_\theta = 0$$

Now since

$$e^{\tau^\theta} (1 + \Delta_\theta)^l u_\theta = e^{-\tau^l} (1 + \Delta_\theta)^l e^{\tau^\theta} u_\theta$$

we have

$$u_\theta - 3(1 + e^{-\theta})e^{-l}(1 + \Delta_\theta)^l e^\theta u_\theta + 2(e^\theta + 1)(1 + 2e^{-\theta})e^{-\left(1 + \frac{2}{x}\right)} (1 + \Delta_\theta)^{\left(1 + \frac{2}{x}\right)} e^\theta u_\theta = 0.$$

or $u_\theta - 3(1 + \Delta_\theta)^l e^\theta u_\theta + 2(e^\theta + 1)(1 + \Delta_\theta)^{\left(1 + \frac{2}{x}\right)} e^\theta u_\theta = 0.$

Put $(1 + \Delta_\theta)^l \cdot (1 + \Delta_\theta)^l$ for $(1 + \Delta_\theta)^{\left(1 + \frac{2}{x}\right)}$

where Δ_θ in the former operates on the x in the latter.

$$\therefore u_\theta - 3(1 + \Delta_\theta)^l e^\theta u_\theta + 2(1 + e^{-\theta})e^\theta (1 + \Delta_\theta)^l \cdot (1 + \Delta_\theta)^l e^\theta u_\theta = 0$$

or $u_\theta - 3(1 + \Delta_\theta)^l e^\theta u_\theta + 2(1 + \Delta_\theta)^l e^\theta \cdot (1 + \Delta_\theta)^l e^\theta u_\theta = 0$

or $u_\theta - 3(1 + \Delta_\theta)^l e^\theta u_\theta + 2\overline{(1 + \Delta_\theta)^l e^\theta}^2 u_\theta = 0$

which can be resolved into the two

$$\{1 - (1 + \Delta_\theta)^l e^\theta\} u_\theta = 0 \text{ and } \{1 - 2(1 + \Delta_\theta)^l e^\theta\} u_\theta = 0$$

or $u_x - (x+1)u_{x+1} = 0$ and $u_x - 2(x+1)u_{x+1} = 0$

where $u_x = \frac{A}{x+1}$ or $u_x = \frac{B}{2^x(x+1)}$

and therefore generally, $u_x = \frac{A + B 2^{-x}}{x+1}$, which is the complete solution of the equation.

It is evident that the process employed in Example 1, applies equally in this Example, when a function of x appears on the right-hand side of the equation. Hence

Ex. 3. $u_x - 3(x+1)u_{x+1} + 2(x+1)(x+2)u_{x+2} = X$ gives

$$u_\theta = -\frac{X+0}{1-(1+\Delta_\theta)^l e^\theta} + 2\frac{X+0}{1-2(1+\Delta_\theta)^l e^\theta}$$

$$= \frac{A+B \cdot 2^{-x}}{\sqrt{x+1}} + \frac{1}{\sqrt{x+1}} \Sigma \sqrt{x+1} X - \frac{2^{-x+1}}{\sqrt{x+1}} \Sigma 2^x \sqrt{x+1} X$$

COR. In the same manner may the more general equation

$$u_x - a(x+1)^r u_{x+1} + b(x+1)^r (x+2)^r u_{x+2} + \&c. = X, \text{ be solved.}$$

It is not necessary to solve such equations as $u_{x+\frac{1}{2}} + a u_x + \&c. = X$, since it is evident that, by putting $x = \frac{1}{2} x'$, this form of equation is reduced to $v_{x'} + a v_{x'+1} + \&c. = X$, which has been already solved.

We proceed then to the solution of equations involving fractional differences. Here we must, at present, confine ourselves to very simple examples.

Ex. 4. $\Delta^{\frac{1}{2}} u_x - a u_x = 0.$

Since $\Delta^{\frac{1}{2}} e^{m x} = (e^m - 1)^{\frac{1}{2}} e^{m x}$

It is evident that if $m = \log(a^2 + 1)$ the solution of the equation is $u_x = A e^{m x}$

$$= A e^{x \log(a^2 + 1)} = A (a^2 + 1)^x$$

Ex. 5. $\Delta^{\frac{1}{2}} u_x - a u_x = c e^{a x}$

$$u_x = A (a^2 + 1)^x + \frac{c}{(e^a - 1)^{\frac{1}{2}} - a} e^{a x}$$

or, if $e^a = 1 + b^2$, $u_x = A (a^2 + 1)^x + \frac{c}{b - a} (b^2 + 1)^x$

COR. If $b = a$, this solution fails. Put $b = a + \beta$ as in similar cases in Differential Equations:

then $\frac{c}{b-a} (b^2 + 1)^x = \frac{c}{\beta} (a^2 + 1 + 2a\beta)^x = \frac{c}{\beta} (a^2 + 1)^x + \frac{c}{\beta} 2a\beta x (a^2 + 1)^{x-1}$

$$u_x = A_1 (a^2 + 1)^x + 2a c x (a^2 + 1)^{x-1}$$

A_1 being an arbitrary constant.

It may be interesting to verify this solution.

$$\Delta^{\frac{1}{2}} (a^2 + 1)^x - a \cdot (a^2 + 1)^x = 0 \text{ evidently,}$$

and $\Delta^{\frac{1}{2}} 2a c x (a^2 + 1)^{x-1} = 2a c x a (a^2 + 1)^{x-1} + \frac{1}{2} 2a c (a^2 + 1)^x \frac{1}{a}$ by Prop. 3.

$$= 2a^2 c x (a^2 + 1)^{x-1} + c (a^2 + 1)^x$$

$\therefore \Delta^{\frac{1}{2}} 2a c x (a^2 + 1)^{x-1} - a \cdot 2a c x (a^2 + 1)^{x-1} = c (a^2 + 1)^x = c e^{a x}$ as it ought.

Ex. 6. Generally, let $\Delta^{\frac{1}{2}} u_x - a u_x = X$

then $u_x = A (a^2 + 1)^x + (\Delta^{\frac{1}{2}} - a)^{-1} X$

$$= A(a^2 + 1)^x + \frac{\Delta^{\frac{1}{2}} + a}{\Delta - a^2} X$$

$$= A(a^2 + 1)^x + (\Delta^{\frac{1}{2}} + a)(a^2 + 1)^x \Sigma \frac{X}{(a^2 + 1)^{x+1}}$$

COR. 1. Let $X = bx$; then the solution of the equation $\Delta^{\frac{1}{2}} u_x - a u_x = bx$ is

$$u_x = A(a^2 + 1)^x - (\Delta^{\frac{1}{2}} + a) \cdot \left(\frac{bx}{a^2} + \frac{b}{a^4} \right)$$

COR. 2. Let $X = b \frac{a^2 x + a^2 + 1}{x(x+1)}$

then the value of $(\Delta - a^2)^{-1} X$ is $-\frac{b}{x}$

therefore the solution of the equation $\Delta^{\frac{1}{2}} u_x - a u_x = b \frac{a^2 x + a^2 + 1}{x(x+1)}$

is

$$u_x = A(a^2 + 1)^x - (\Delta^{\frac{1}{2}} + a) \frac{b}{x}$$

$$= A(a^2 + 1)^x - \frac{ab}{x} - b \sqrt{-1} \frac{\sqrt{x} \sqrt{\frac{3}{2}}}{x + \frac{3}{2}} \quad (\text{by Ex. 6, Art. 24.})$$

EX. 7. $\Delta u_x + a \Delta^{\frac{1}{2}} u_x + b u_x = 0.$

This equation may be written

$$(\Delta + a \Delta^{\frac{1}{2}} + b) u_x = 0.$$

Let α, β be the roots of the equation $z^2 + az + b = 0$, then

$$(\Delta^{\frac{1}{2}} - \alpha)(\Delta^{\frac{1}{2}} - \beta) u_x = 0$$

$$u_x = A(\Delta^{\frac{1}{2}} - \alpha)^{-1} \cdot 0 + B(\Delta^{\frac{1}{2}} - \beta)^{-1} \cdot 0$$

$$= A(1 + \alpha^2)^x + B(1 + \beta^2)^x \quad (\text{Ex. 4.})$$

COR. If $\alpha = \beta$, we obtain, by the usual process, $u_x = (A + Bx)(1 + \alpha^2)^x.$

EX. 8. $\Delta u_x + a \Delta^{\frac{1}{2}} u_x + b u_x = c(1 + e^2)^x$

The solution is

$$u_x = \frac{1}{\alpha - \beta} (\Delta^{\frac{1}{2}} - \alpha)^{-1} (0 + c \sqrt{1 + e^2})^x - \frac{1}{\alpha - \beta} (\Delta^{\frac{1}{2}} - \beta)^{-1} (0 + c \sqrt{1 + e^2})^x$$

$$= A(1 + \alpha^2)^x + B(1 + \beta^2)^x + \frac{c(1 + e^2)^x}{(a - \beta)(e - \alpha)} - \frac{c(1 + e^2)^x}{(a - \beta)(e - \beta)}$$

$$= A(1 + \alpha^2)^x + B(1 + \beta^2)^x + \frac{c(1 + e^2)^x}{e^2 + ae + b}$$

COR. If $e = \alpha$, we must proceed as in Cor. 3, Ex. 4, Class 1, of Differential Equations, and we shall obtain

$$u_x = A(1 + \alpha^2)^x + B(1 + \beta^2)^x + \frac{cx(1 + \alpha^2)^{x-1}}{2\alpha + a}$$

Ex. 9. $\Delta u_x + a \Delta^{\frac{1}{2}} u_x + b u_x = X$

$$\begin{aligned} u_x &= \frac{1}{a-\beta} (\Delta^{\frac{1}{2}} - a)^{-1} (0+X) - \frac{1}{a-\beta} (\Delta^{\frac{1}{2}} - \beta)^{-1} (0+X) \\ &= A (1+a^2)^x + B (1+\beta^2)^x + \frac{1}{a-\beta} (\Delta^{\frac{1}{2}} - a)^{-1} X - \frac{1}{a-\beta} (\Delta^{\frac{1}{2}} - \beta)^{-1} X \\ &= A (1+a^2)^x + B (1+\beta^2)^x + \frac{1}{a-\beta} (\Delta^{\frac{1}{2}} + a) (1+a^2)^x \frac{X}{(1+a^2)^{x+1}} \\ &\quad - \frac{1}{a-\beta} (\Delta^{\frac{1}{2}} + \beta) (1+\beta^2)^x \frac{X}{(1+\beta^2)^{x+1}} \end{aligned}$$

Ex. 10. $u_x + a x \Delta^{\frac{1}{2}} u_x = X.$

This equation gives $u_x = \frac{1}{1+a x \Delta^{\frac{1}{2}}} X$

$$= \frac{1-a x \Delta^{\frac{1}{2}}}{1-a^2 x \Delta^{\frac{1}{2}} x \Delta^{\frac{1}{2}}} X$$

$= (1-a x \Delta^{\frac{1}{2}}) v_x$, where v_x is determined by the equation

$$v_x - a^2 x \Delta^{\frac{1}{2}} x \Delta^{\frac{1}{2}} v_x = X$$

Now $\Delta^{\frac{1}{2}} x \Delta^{\frac{1}{2}} v_x = x \Delta v_x + \frac{1}{2} v_{x+1}$ (Prop. 3.)

$\therefore v_x - a^2 x^2 \Delta v_x - \frac{a^2}{2} x v_{x+1} = X$

or $v_{x+1} - \frac{2}{a^2} \frac{1+a^2 x^2}{x+2x^2} v_x = -\frac{2X}{a^2(x+2x^2)}$

which being solved by the ordinary method, v_x and therefore u_x (provided $\Delta^{\frac{1}{2}} v_x$ can be found) is known.

Equations of Differences with two independent variables are not capable of solution to any great extent. An example or two will suffice to illustrate our process.

Ex. 11. $\Delta_x u_{x,y} - \Delta_y u_{x,y} = b.$

The solution is $u_{x,y} = \frac{1}{\Delta_x - \Delta_y} b$

Treat Δ_y as a constant c , then the solution of $(\Delta_x - c) u_{x,y} = b$ is

$$\begin{aligned} u_{x,y} &= (c+1)^x A - \frac{b}{c} \\ u_{x,y} &= (\Delta_y + 1)^x v_y - \Delta_y - 1 b \\ &= \left(e^{\frac{d}{dy}}\right)^x v_y - b y \\ &= v_{y+x} - b y \end{aligned}$$

v_{y+x} being an arbitrary function of $y+x$.

Ex. 12. $\Delta_x^{\frac{1}{2}} u_{x,y} - \Delta_y^{\frac{1}{2}} u_{x,y} = b$

This equation gives
$$u_{x,y} = \frac{1}{\Delta_x^{\frac{1}{2}} - \Delta_y^{\frac{1}{2}}} b$$
$$= \frac{\Delta_x^{\frac{1}{2}} + \Delta_y^{\frac{1}{2}}}{\Delta_x - \Delta_y} b$$
$$= (\Delta_x^{\frac{1}{2}} + \Delta_y^{\frac{1}{2}}) (v_{y+x} - b y) \quad (\text{Ex. 11.})$$

We have thus succeeded in solving equations with fractional indices of all forms corresponding with the ordinary forms of Linear Differential Equations, whether total or partial,—whether solitary or simultaneous. We have also placed the Calculus of Fractional Differences on the same footing with respect to the ordinary Calculus of Fractional Differences as that which the Calculus of General Differentiation occupies relatively to the ordinary Differential Calculus.

P. KELLAND.

EDINBURGH, *October 10, 1846.*

XXI.—*Observations on the Principle of Vital Affinity, as illustrated by recent discoveries in Organic Chemistry.* By WILLIAM PULTENEY ALISON, M.D., F.R.S.E., *Professor of the Practice of Medicine in the University of Edinburgh.*

(Read 1st and 15th February 1847.)

PART II.

It may be remembered that, in the paper formerly laid before this Society on this subject, I endeavoured to establish the principle still disputed by some physiologists, that the laws which regulate the chemical relations, as well as those which regulate the visible movements of the particles of matter, undergo a certain determinate modification or change in living bodies, which is essential to the commencement and to the maintenance of the organization of those bodies; and farther, that I undertook the task of attempting to deduce, from the numerous but somewhat discordant experiments and observations lately made on the subject, certain inferences which appear to be well ascertained, although not generally admitted, as to the essential nature of this change, *i. e.*, as to laws which regulate those chemical actions which are peculiar to the state of life, and essential to the maintenance of organization, both in vegetables and animals.

In confirmation of my statement of the general principle of Vital Affinity, as distinguished from simply chemical affinities, I have much satisfaction in quoting two sentences from the last edition of LIEBIG'S "Animal Chemistry." Some of the statements of general principles made by this author, seem to me open to objection, and some I do not profess to understand; but the following is simple and precise; and, considering the authority of LIEBIG as a chemist, may, I think, be held nearly decisive as to the soundness of the principle. "A *fundamental error*, committed by some physiologists is, that they suppose the chemical and physical forces alone, or in combination with anatomy, sufficient to explain the phenomena of vitality. It is, indeed, difficult to understand how the chemist, who is intimately acquainted with chemical forces, should recognise in the living body the existence of *new laws*, of new causes, while the physiologist, who is little or not at all familiar with the action and nature of chemical and physical forces, should think himself ready to explain the same processes with the aid of the laws of inorganic nature alone."—*Animal Chemistry* (third edition, p. 252.)

The first and most fundamental of these general principles (likewise considered in my former paper) is the power of vegetable life, under the influence of light, to decompose the carbonic acid existing in the atmosphere,—set the oxygen free, fix the carbon, and form with it and the elements of water, starch, sugar, gum, and the analogous compounds. Our knowledge of this power, of the effects

which result from it, and of the period when it must have been first exerted on the earth's surface, enables us to assert with confidence, that by means of it, the whole organised creation has been, as DUMAS expresses it, the offspring of the air; and that it was by enabling the rays of the sun to excite this action in certain particles of matter, existing in the atmosphere, but destined to be either the first specimens, or the first germs of vegetable life, that "a beneficent God," to use the striking expression of LAVOISIER, "has strewed the surface of the earth, first with organized structures, and then with sensation and thought."

In proceeding farther to inquire into the laws of Vital Affinity, we must always keep in mind the general arrangement or classification, long ago made by Dr PROUT, of all the organic compounds, of which any organized structures, vegetable or animal, are composed, into three groups or classes, the Saccharine or amylaceous, the Oily, and the Albuminous; and the important observation, I believe first made by him, that the food of most animals contains all these compounds, and that no complex animal structure can be maintained without the concurrence of at least two of these kinds of compounds in its food.

I do not think it is going too far to say that we have now a general knowledge of the laws or conditions under which all these compounds are formed in living bodies, taking the starch formed from carbonic acid and water as the foundation of all. But we perceive farther, that that these laws, *varying in different parts of the same structure*, and at *different times in the same parts*, and being of *transient duration* in all, are liable to an *influence of time and of place*, and in animals to a farther influence of mental changes, which is quite analogous to the vital actions, both of muscular and nervous organs, but is strongly contrasted with the uniformity of the laws that determine the changes of inorganic matter. And if this be so, we may assert that considerable progress has been made, both in establishing and in illustrating the doctrine of vital affinity, as a first principle in physiology.

I. The formation of Oil or Fat in living bodies is, perhaps, that part of the chemical processes there carried on, which is now the best understood, and the study of which gives us the clearest insight into the nature of vital affinities. We need not enter into any of the simply *chemical* questions as to the mode of combination of the fatty acids and bases in the different kinds of fat; it is sufficient for our purpose to observe that they are found very generally, though very variously disposed, in almost all vegetables and animals, and even in the earliest stages of their existence; the store of nourishment contained in the seed and in the egg, containing a proportion of fatty matter. And though there is considerable variety in the different kinds of fat or oil, they all differ from the varieties of starch, by having a much smaller proportion of oxygen, and, of course, a larger proportion

of carbon and hydrogen. The composition of most fats is stated by Liebig to be $C_{12} H_{10} O_1$; and we have thus, therefore, another compound formed apparently by vital affinity, indicating a peculiar attraction of the two first elements for one another, and a feeble attraction for oxygen. Indeed, in the composition of wax (one of this family of compounds), as stated by MULDER, the proportion of oxygen is only one equivalent to 24 of carbon; in cholestérine, the proportion of carbon to oxygen is stated as high as 36 to 1; and in many volatile oils, no oxygen has been detected.

Supposing such a peculiar affinity to act, there is obviously no difficulty (on looking at the numbers indicating the proportions of the elements) in understanding the formation of these compounds out of starch ($C_{12} H_{10} O_{10}$), just as there is none in understanding the formation of starch or sugar (although by an affinity occurring only in living bodies, and which we regard as vital) from carbonic acid and water ($CO_2 + HO$), in living vegetables, where a continual evolution of oxygen attends the growth; particularly if we suppose that the carbonic acid taken in by the leaves and roots, is carried to, and decomposed in, all parts of the plant: the formation of the fatty compounds, is, no doubt, one of the processes by which the oxygen is set free. But in the case of animals, where (with the exception of some of the infusory tribes) there is no evolution of oxygen, the formation of fat from starch presents a difficulty. Yet the numerous observations and experiments of LIEBIG and of CHEVREUL and MILNE-EDWARDS, leave no room for doubt that various animals, fed chiefly on varieties of starch, or bees fed on sugar, form a much larger quantity of fat, oil, or wax, than they have received mixed with their food, and this when they are exhaling no pure oxygen, but, on the contrary, compounds of hydrogen and carbon with oxygen, viz., water and carbonic acid. Indeed, Dr ROBERT THOMSON having ascertained by repeated experiments, that the quantity of butter yielded by cows bears no fixed proportion to the quantity of oleaginous matter contained in their food, varying indeed from one quarter to nearly the whole of the oleaginous ingesta, thinks himself justified in inferring that "the butter cannot be supplied from the oil of the food." (*On the Food of Animals*, p. 156.)

It is quite certain that in this action, in all animal bodies, the greater part of the oxygen of the starch employed must unite with a portion of its carbon and hydrogen, and pass off in the excretions just noticed, leaving the small remainder of the oxygen in combination with the predominant quantities of carbon and hydrogen.

It appears possible, indeed, that *all* the oxygen which must be separated from starch before it can be converted into fat, may be evolved in combination with part of the carbon and hydrogen of the starch, without any constituent of the air taking any part in the process; but the quantity of fat formed would then be small, and it is also possible that the oxygen of the air may be concerned

in the metamorphoses to which starch is liable in a living body ; and as we know the importance of oxygen in maintaining (in one way or other) all vital action, the latter supposition is more probable.

If, *e. g.*, we suppose 4 atoms of starch to yield 2 of fat, we must subtract from

48 C	40 H	40 O
24 C	20 H	2 O
leaving 24 C	20 H	38 O = 20 HO 9 CO ² + 15 C ;*

so that on this supposition 15 atoms of carbon are set free, and as these do not appear, they must unite with the oxygen of the air, and take the form of carbonic acid ; and then the fat which appears, together with the water and carbonic acid thrown off, will account for all the elements concerned in the action. In this process, therefore, supposing the quantities of starch taken in, and of fat formed to be as above, 30 equivalents of oxygen must be absorbed ; so that we perceive the use of oxygen in the change, and the necessity of its presence, although the fat formed contains so much less oxygen than the starch.

That this should be the real nature of the change is just what we ought to expect, if, agreeably to the supposition formerly made, the starch taken into the blood of a living animal, is acted on at certain parts of the body by two powers, and divides itself between them, *viz.*, a vital affinity, in which carbon is the chief agent, which leads to the formation of fat, and the simply chemical affinities, exerted chiefly by oxygen (continually taken into the blood), by which, if removed from the living body, we know that it would gradually be resolved into carbonic acid and water. And that this is the real state of the case we are fully assured by a simple but very important observation, *viz.*, attending to the effect of *exercise* on the formation or deposition of fat in the living animal body. As we see by the numbers given above, that a certain amount of oxygen must be absorbed, and a certain quantity of carbonic acid and water, formed by its help, must be excreted, to enable starch to yield oil or fat by the process there represented, we can understand that moderate exercise should favour the change ; but when exercise is carried beyond a very moderate extent, we know that the circulation and respiration being much accelerated, and the quantity of oxygen taken into the living blood being much increased, the effect is, to increase the exhalation of carbonic acid and water, and proportionally to diminish the deposition of fat ; *i. e.*, to give a preponderance to the simply chemical affinities exerted by the oxygen, over the vital affinity, which would tend to the formation of fat.

From this simple fact we may infer, 1. That the vital affinity by which oil is

* It need hardly be said, that all these numbers are given, not as indicating the exact changes which take place when the organic compounds are formed, but only as illustrating their general nature.

formed from starch, or by which its elements are held together, does not supersede its natural chemical relations, but only adds a new chemical power to those which can operate on it, and allows of a division of the starch between the result of a vital and a simply chemical affinity; and, 2. That the vital action by which fat is formed or maintained, is of no great strength, as compared with the simply chemical affinities to which the same matter is liable; being superseded simply by an increased supply of oxygen. And we cannot doubt that, in this as in other vital chemical processes, the oxygen, although not taken into the organic compound formed, aids its formation materially, by promoting, on the principle of divellent affinity, the other parts of the metamorphoses whereby it is produced. We shall see afterwards the importance of having it established by this simple example, that the oxygen of the air, when taken in full quantity into the blood, is capable of combining, somewhere in the course of the circulation with a part of that carbon and hydrogen, recently absorbed into the blood, which, under a smaller supply of oxygen, would form a living texture; and that the combination of these portions of the ingesta with oxygen, are one source of the excretions.

There are other facts which lead to the same conclusion, as to the affinity by which fat is formed, being more nearly akin than most vital actions to simply chemical affinities; particularly,—

1. The formation of Adipocere, not from starch, but from albumen, after vitality is over, when undergoing decomposition under ground, where there is a full supply of water and but little air, so that the supply of oxygen is less than in ordinary putrefaction, which may be understood thus:—

	C	N	H	O	
	48	6	36	14	= Albumen
Add			12	12	Water
				1	Oxygen
	48	6	48	27	
Subtract	36		30	3	Fat
	12	6	18	24	= Carbonic Acid and Ammonia

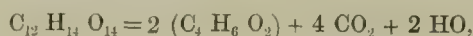
which escape, and the attraction of which for each other, no doubt in part determines the result.

2. Again, in the living body, but in a feeble constitution, along with great emaciation, and a deficient supply of oxygen, a morbid deposition of fat sometimes takes place, in circumstances where it could not have been anticipated, but only in particular parts. Some distinct cases of this kind have lately attracted attention, one in the kidneys, in one form of BRIGHT'S disease, another in the liver, as in many phthisical cases, and a third in the atheromatous exudations

so common on the arteries. It may be suspected that in these cases the formation of fat is by an affinity hardly more vital than the formation of adipocere,—in both cases the decomposition of albumen to form the fat, being aided by the simply chemical affinities, of carbon for oxygen, and of hydrogen for azote.

3. The same peculiarity of the attractions by which fat is formed in the animal economy may be admitted in explanation of the more general fact, that in a healthy constitution, when more, particularly of amylaceous, food is taken than is required for the nutrition of the more important textures, and when little oxygen is taken in, the excess always tends to the deposition of fat, which implies that a large portion of the oxygen of that food has gone off as carbonic acid and water.

The process of the formation of oil from starch in the animal body, admits of an instructive comparison with the simply chemical one of the formation of alcohol from the same matter,—at least, from a compound fluid of which starch (first converted into sugar by the kind of fermentation formerly mentioned) is the chief constituent, in fermentation; *e. g.*, the changes in the vinous fermentation of grape-sugar, are represented thus,—



that is, the elements of grape-sugar resolve themselves into two equivalents of alcohol, four of carbonic acid, and two of water. In this case, as in the formation of fat, the starch or sugar is divided into three parts, water, carbonic acid, and a peculiar compound fluid. In both cases, the oxygen of the air is necessary to the commencement, and probably to the continuance, of the process, although in both, the new compound formed contains less oxygen than the starch or sugar from which it is produced. In both cases, a third body is present, and its influence somehow promotes the process, besides the oxygen and the starch, *viz.*, in the one case, yeast, or some kind of ferment, itself in a state of decomposition, which it imparts, without giving up any part of its substance, to the starch or sugar; in the other case, a living cell, composed of gelatin, which is itself undergoing a simultaneous change, by a living process. In both cases, extension of the change takes place, as from a centre, from this third body, through the fluid in which the change commences. In both cases, the compound formed is not stable; and the portions of the starch which go to form it are destined ultimately to follow the same course as those portions which are resolved into carbonic acid and water. In the one case, the compound formed, $C_4 H_6 O_2$, contains a less proportion of carbon than any of those which we regard as endowed with strictly vital properties; while, in the other, the compound formed, $C_{12} H_{14} O_{14}$, has the characteristic predominance of carbon. But if we are asked, Why we regard the one as the result of a simply chemical process, and the other of a vital affinity? I apprehend the sufficient answer to be,

that the one is a change which uniformly results when the sugar is exposed to the influence of air, water, and a certain temperature; and is in contact with a substance undergoing some part of that decomposition and chemical change to which living bodies are liable after the phenomena of life are over; whereas the other is not seen, in the presence of those substances, and under those conditions as to air, water, and temperature, in which it here takes place, unless the starch is at the same time in contact with living cells,—*i. e.*, cells forming a part of a body in which the peculiar phenomena of life are then exhibited.*

II. The next question is as to the formation of the Albuminous, or what have lately been called the Protein, compounds in animal bodies. The late acrimonious dispute as to the existence of Protein, should rather be termed a dispute as to the exact composition of the compound to which MULDER gave that name, and which is thrown down from the solution of either albumen or fibrin, in potash, by acetic acid. Of the precipitate being the same in both cases there is no doubt; and we shall avoid the controversy entirely, by using the term Albuminous Compounds, as Dr PROUT did, instead of the term Protein.

Since it has been clearly ascertained, that the vegetable gluten is identical in composition with the albuminous compounds,—*i. e.*, fibrin, albumen, and casein of animals,—no doubt can exist that the formation of a great part of the albumen found in animal bodies must take place in vegetables; and, I presume, it is also generally agreed that the chief agents in this farther change, beyond the formation of starch and of fat, are sulphur, and ammonia or its elements, taken into the fluids of the vegetable, although it is still doubtful from what sources this ammonia or its elements may be originally derived, and particularly whether, in any circumstances, the azote of the atmosphere is concerned in producing it.

Some experiments recorded by DUMAS,† however, seem to leave no room for doubt, that certain families of plants, in one way or another, fix azote from the air, being found to add largely to that contained in their seeds, when germinating and growing merely in silica and water; and it is by no means ascertained, that this azote passes necessarily into the state of ammonia before it is applied to the nourishment of those vegetables. And the statements of MULDER seem equally conclusive as to the fact, that ammonia may be

* It is no objection to this statement, that oily matters may, in different cases besides that of adipocere already noticed, be formed from organic compounds in the dead state, *i. e.*, by simply chemical affinities. To establish that the affinity by which it is formed in a living structure is vital, it is not necessary to shew that oil cannot be formed, under any circumstances, by simply chemical laws, but only to assure ourselves, that it cannot be formed by those laws from the substances, and in the circumstances, in which it is continually formed in certain living cells.

† Balance of Organic Nature, p. 77.

formed by the union of azote from the atmosphere with hydrogen from water, whenever another substance, exerting an attraction for the oxygen of the water, is present.—(*Chemistry of Vegetable and Animal Physiology*, p. 149, *et seq.*) Now, as carbonic acid and water form starch, or its allied compounds, in the living vegetable, by the attraction of carbon for the elements of water, to the exclusion of oxygen; and as the starch then forms oil, by the attraction of the carbon to hydrogen, to the exclusion of great part of the remaining oxygen; so, on the introduction of ammonia, or its elements in a state fit for entering into new combinations, into the scene of those metamorphoses, it is only in accordance with what we know of the nature of these vital affinities, to suppose that the carbon may attach to itself the elements of this ammonia, to the exclusion of the elements of water and of oxygen, matters which are known to be continually thrown off by vegetables, during the continuance of these vital processes. Thus we have the elements of starch, 48 C, 40 H, 40 O *plus* the elements of ammonia, 6 N, 18 H, = 48 C, 6 N, 58 H, 40 O = 48 C, 6 N, 36 H, 14 O (the elements of albumen) *plus* 22 H O + 4 O, a considerable quantity of the water, and a small quantity of the oxygen, which are continually exhaled by the plant.

Thus, during the whole process of the formation of organic compounds in the vegetable, we see that the vital affinities shew themselves by the attractions of Carbon, first for the elements of water in preference to oxygen, then, either for the hydrogen of those elements, in preference to the oxygen, or for the elements of water, with an excess of hydrogen, along with those of ammonia; and thus, by these peculiarities of attraction of Carbon, for the elements of water, for hydrogen, and for azote,—to the more or less complete exclusion of oxygen,—we see that the essential materials of all organized matters may be easily formed, while water and oxygen, the known excretions of vegetables, only escape.

The point at this moment most disputed, and the settlement of which is most essential to the precise comprehension of the nature of vital affinities, is, Whether there is any formation of albuminous matter in *animal* bodies? and it is obvious, that there is a difficulty in regard to its formation from starch, just similar to that which was stated as to the formation of oil in the animal body, because we see no evolution of oxygen; but it is also certain that this may be got over, precisely in like manner as in the former case, by supposing—what is quite in accordance with known facts—that a considerable absorption of the oxygen of the air attends the process, and that, with its help, a large portion of the carbon

* This may be shortly stated thus

$\text{CO}_2 + \text{HO}$	= Carbonic acid and water. From this is formed,
$\text{C} + \text{H} + \text{O}$	= Sugar, oxygen going off. From this,
$\text{C}_{48} \text{H}_{40}^3 \text{O}_{40}$	= Starch, water going off. From this, either
$\text{C}_{48} \text{H}_{40} \text{O}_4$	= Fat, oxygen going off. Or,
$\text{C}_{48} \text{N}_6 \text{H}_8^6 \text{O}_{14}$	= Albumen, ammonia being added, and water and a little oxygen going off.

and hydrogen are thrown off in carbonic acid and water. Thus, supposing a large quantity of starch, 60 C, 50 H, 50 O, to unite with a small quantity of ammonia, we have

	C	N	H	O
	60	6	68	50 and adding 20 of oxygen,
we have	60	6	68	70=48 C, 6 N, 36 H, 14 O,

(the elements of albumen) + 32 HO + 12 CO₂, the water and carbonic acid which escape. Or, adding an equivalent of oil, we may have

	C	N	H	O	
	48	...	40	40	Starch.
Add	12	...	10	1	Oil.
	...	6	18	...	Ammonia.
<hr/>					
	60	6	68	41	
Subtract	48	6	36	14	Albumen.
<hr/>					
	12	...	32	27	Adding 29 oxygen,

we get 12 CO₂ + 32 HO carbonic acid and water.

It is certain, therefore, that if the elements of ammonia can be set free in the primæ viæ of an animal, starch absorbed from thence, with or without the addition of oil, may be converted into albumen in its blood, without any other matter being thrown off than the water and carbonic acid, which undoubtedly escape from every animal. If this be so, we have here another division of the elements of the ingesta, between substances exerting a vital and a simply chemical affinity for them, and another formation of part of the excretions, by the help of the oxygen of the air, from matters recently absorbed, and which aid in the nourishment of the animal. But whether this is a process that actually goes on in the animal economy, or whether all the albuminous compounds of animal bodies have passed into them, directly or indirectly (but ready formed), from vegetables, is the point at this moment the most important to be ascertained.

As it is obvious that the albuminous compounds, and the gelatinous compounds (which are closely related to them, and are generally thought to be formed from them), compose the greater part of the animal textures, and are equally the groundwork of all animal structure, as starch is of vegetables, this inquiry involves the essential point of distinction, so far as chemistry goes, between vegetables and animals. It is well known that both LIEBIG and DUMAS have expressed a decided opinion that no albumen is formed in animals; and the latter author has contrasted, in a lively manner, vegetable and animal life in this respect, representing the former as always a reducing or deoxidating apparatus, and the latter as an apparatus of oxidation or combustion, *i. e.*, of the destruction, never of the formation, of any organic compound. But he does not appear to have adverted particularly to the question which seems to me the most essential in a physiological view, viz., what are the chemical changes during the state of life, whether

in vegetables or animals, which are distinctly at variance with the ordinary laws of chemistry, and which we must therefore ascribe to vital affinities?

It is evident that what, in physiological language, is commonly called Assimilation, includes two distinct actions, both, in many cases, as I believe, strictly vital; *first*, the mere selection and attraction of a part of a compound fluid, to be added to a living body; and, *secondly*, the *transformation* of the elements of two or more compounds, so as to form a new compound, similar to one already existing in the living body wherein this change occurs. If DUMAS'S view of the subject were to be adopted, we should say that animals can exert only the first of these powers, the simple selection and attraction of one of the ingredients of a compound fluid by each organ or texture, without any power of *transformation*, or formation of new compounds; and accordingly, he says that "it is in plants that the true laboratory of organic chemistry resides."

But if we state the proposition thus generally, we may state various facts to shew, that it is incorrect. It is quite certain, as already stated, that oil or fat may be *formed* in animal bodies, by a new arrangement of the elements of starch, attended by an evolution of much of its oxygen, and of part of its carbon and hydrogen, effected by the aid of the oxygen of the air; and the influence (already noticed) of exercise, *i. e.*, of an increased application of oxygen, on this change, shews distinctly that the recent ingesta are liable to two influences in a living animal, one of which is an action of oxidation or combustion, throwing off water and carbonic acid, but the other is strictly an action of reduction, by which a quantity of oxygen is separated from its combinations in an organic compound, while a fresh compound, constituting part of the animal frame, is formed. And the fat of the animal body, which may be thus formed, is not to be considered as a merely unorganized appendage to the textures. It appears from some of LIEBIG'S observations, that the muscular flesh of all animals, after being cleared of all visible fat, still retains a considerable and variable quantity in its substance; and we know that in two of the most important textures of the body, nervous matter and bone, fat is an essential ingredient.

In like manner, the formation of the essential ingredient Gelatin in the animal body is the result of a new arrangement of elements, attended with evolution of carbon and hydrogen, by the aid of the oxygen of the air, but probably not with absorption of oxygen.

In the case of Inflammation, we see distinctly that, in connection with an increased action of nutrition or deposition of plastic lymph, there is a transformation of portions of the blood to form the compound, very similar to gelatin, termed, by MULDER, the Tritoxide of Protein, which is found there in very unusual quantity; and in other morbid actions, in certain chronic malignant diseases, we see compounds, altogether foreign to the natural organization, formed and even rapidly extended; the formation of which is certainly neither a simply chemical act

of oxidation, nor a mere selection and appropriation of compounds previously formed in vegetables.

On the other hand, it is known that there is an evolution of carbonic acid as well as water from vegetables,—from the parts of fructification during their development even in the day time, and from all parts during the night; and it appears quite possible that, in both cases, this may be by a process of slow combustion, similar to the process of oxidation which DUMAS considers as characteristic of animal life only. For, although it has been stated by DUMAS that the carbonic acid given up by vegetables during the night is only what has been absorbed by their roots, and passed unchanged through their substance, yet I do not find any distinct proof of this in his writings. It is certainly true, that the organic compounds formed by vegetables, and taken into animal bodies, ultimately undergo in them a chemical change nearly equivalent to slow combustion, and are thus returned to the inorganic world; but this is in the processes of absorption, decomposition, and excretion, of the animal textures, to be considered presently; and this fact affords of itself no proof, that in the previous growth and development of animal textures, there may not be an actual formation of albuminous compounds, as well as of gelatin and fat.

These facts appear sufficient to shew, that there is no such direct opposition between vegetables and animals, as to the chemical results of their vital action, as DUMAS has represented; and even to make it probable, that, during the organic or vegetative life of animals, there will be a formation of albuminous matter, equally as of gelatin and fat.

In fact, this question can be only finally decided by experiments to shew whether or not the whole quantity of albumen deposited in the textures of a growing animal may be greater than that contained in its food; or whether the azote excreted, during a pretty long period, from an animal, by the bowels, kidneys, skin,* and lungs† (for it appears to be well ascertained, that, from all these parts, there is a frequent, if not an habitual, excretion of azote), is greater, under any circumstances, than the quantity of that element contained in the albuminous portion of its food, which is the only ascertained channel of the introduction of azote into the animal system; and, although this is a difficult inquiry, we cannot suppose that the difficulties are insurmountable. If such an excess of excretion of azote shall be ascertained, it will be nearly enough to entitle us to conclude that albuminous matters can be formed in the animal body, and yield it during their decomposition there. It is not enough to say, that there is no occasion for more azote in the animal economy than is contained in the albuminous ingesta, because what is there contained is already in just the same proportion to

* See GOLDING BIRD on Urinary Deposits, p. 104.

† See DU LONG, quoted by DUMAS (*Organic Nature*, p. 106).

carbon and hydrogen, as that which exists in the blood, or in the textures of animals. As there is, in the whole of the ingesta of animals, a great excess of carbon and hydrogen over their proportion to azote in albumen, and as oxygen is always present in the blood, it is quite possible that a part of the azote of the albumen taken in, may be thrown off in combination with portions of those other elements, by the bowels and kidneys, without entering into the textures; and that the nourishment of the textures may be in part due to fresh albumen, formed in the animal body by help of oxygen from the lungs, and of azote taken in by another channel; just as we are nearly sure that part of the oil taken into an animal is often decomposed and thrown off, and that fresh fat is often formed from the starch or sugar of the ingesta.

There is one mode, pointed out by LIEBIG, in which we can have no doubt that azote must be introduced into the blood of animals, independently of the albuminous ingesta, viz., by the air which is contained in the water, and still more in the saliva, continually taken into the stomach. "During the mastication of the food, there is secreted into the mouth, from organs specially destined to this function, a fluid, the saliva, which possesses the remarkable property of inclosing air in the shape of froth, in a far higher degree than even soap-suds. This air, by means of the saliva, reaches the stomach with the food, and there its oxygen enters into combination, while its nitrogen is given out through the skin and lungs."*

Now, what proof is there that the azote, thus believed to be set free in the stomach, is excreted, unchanged, by the skin and lungs? Is it not much more probable that it enters into fresh combinations in the *primæ viæ* and in the blood, and is only separated from the blood, when, by the agency of the oxygen of the air, acting, under the circumstances to be afterwards stated, with peculiar energy on some of the constituents of the blood, it is disjoined from its union with carbon and hydrogen.

In fact, the azote thus set at liberty in the stomach, must be in circumstances almost exactly similar to those in which, according to the statements of MULDER and others, ammonia is formed from air, even by the help of inorganic matter; still more when organic matter, although non-azotised, is present in a state of decomposition, or an analogous condition.† "By all porous substances ammonia is produced,—provided they are moist, are filled with atmospheric air, and are exposed to a certain temperature."

"When reddened litmus paper is hung up in a bottle, filled with pure atmospheric air, and when pure iron-filings, moistened with pure water, are laid at the bottom, then the red litmus is quickly turned blue by the action of ammonia, formed from the nitrogen on the air, and the hydrogen of the decomposed water, the oxygen of which had combined with the iron.

* LIEBIG'S Animal Chemistry, pp. 113-4.

† MULDER, p. 149, *et seq.*

“Such a formation of ammonia continually takes place in the soil. There, atmospheric air is present, and consequently nitrogen; hydrogen is continually liberated, and thus the conditions necessary to the formation of ammonia are fulfilled as often as cellulose, ligneous matter, starch, &c., are changed either into humic acid, or into other constituents of the soil.”

That a partial decomposition of organized matter takes place in the stomach, and is, indeed, the first part of the changes occurring during digestion, seems to be sufficiently proved by some curious and important observations of LIEBIG himself.* “The fresh lining membrane of the stomach of a calf, digested with weak muriatic acid, gives to this fluid no power of dissolving boiled flesh or coagulated white of egg” (the supposed property of the Pepsin, or extract of the mucous membrane there.) “But if previously allowed to dry, or if left for a time in water, it then yields, to water acidulated with muriatic acid, a substance in minute quantity, the decomposition of which is already commenced, and is completed in the solution. If coagulated albumen be placed in this solution, the state of decomposition is communicated to it, first at the edges, which become translucent, pass into a mucilage, and finally dissolve. The same change gradually affects the whole mass, and, at last, it is entirely dissolved.”

I think we cannot doubt, therefore, that the air introduced into the stomach of animals, and decomposed there, as LIEBIG supposes, must be in circumstances peculiarly well adapted for the generation of ammonia, or the setting free of its elements; which, as we have seen, is all that appears necessary to explain the gradual formation in the matters absorbed from the stomach, of albumen out of non-azotised ingesta; under the influence of vital affinities, similar to those by which albumen is formed in vegetables.

I am aware that LIEBIG states with confidence that experiments prove that the whole of the azote excreted in a given time by an animal, is not more than that which is taken in by its albuminous ingesta; but in this he relies chiefly on the experiments of BOUSSINGAULT, and these experiments are not considered by the author himself as altogether satisfactory; nor can they be satisfactory without farther investigation of the quantity excreted by the skin and lungs, into which he did not inquire. (See *Dumas*, p. 106.)

I admit it to be certain, however, from a simple comparison of the quantities of albuminous ingesta and the azotised excretions, that the formation of albumen in animals can be to no great extent; and I am clearly of opinion that the distinction drawn by LIEBIG, of the azotised and non-azotised ingesta of animals, and the evidence he has given of the chief destination and use of each, constitute the most important improvement lately made, in this department of physiology. It appears now ascertained; 1st, That the latter class of aliments are incapable, in

* *Animal Chemistry*, pp. 110-1.

themselves, of adding to any of the animal textures except the fat ; but that they are the chief material on which the oxygen of the air acts to keep up the animal heat. 2*d*, That the main reliance of the animal body for the nourishment of all its parts must be on the former class of aliments ; their adequacy for that purpose being beautifully exemplified in the life of the chick *in ovo*, where all the textures are formed out of the albumen, partially converted into gelatin in the process, and with the addition of a small quantity of oil from the yolk ; the oxygen of the air being essential to the vital movement, but no farther concerned in the results, than as it carries off a certain portion of the carbon and hydrogen from the moving matter, and so occasions a loss of substance during the process of incubation. 3*d*, That the azotised ingesta, or the textures formed from them, are themselves liable to this action of the oxygen when the non-azotised ingesta are deficient ; and, therefore, that an important use of the non-azotised food is, to protect the albuminous constituents of the blood and the animal textures, from an influence of the oxygen of the air, which, but for that protection, would be injurious, and ultimately destructive. And I may perhaps be allowed to state what seem to me the most important results, both as to Physiology and Pathology, which are involved in these principles.

1. Our ideas of the use of the digestive apparatus of animals are rendered much more simple and precise. I have stated, indeed, that DUMAS appears to have erred in the way of extreme simplification, when he says that "an animal only assimilates" (*i. e.* selects and attracts) "organic structures already formed ; that he forms none ;" that "digestion is therefore a simple process of absorption, soluble substances passing directly into the blood (*i. e.* by the veins), for the most part without alteration, and insoluble substances making their way into the chyle after having been sufficiently comminuted, to be imbibed by the lacteals." But although we suppose that certain transformations, as well as simple absorption, must be commenced, at least, in the digestive organs, we are sure that no complication of apparatus is necessary for accomplishing them ; the most important of all transformations necessary to life taking place in vegetables, and in organs of extreme simplicity.

The following may be stated as the purposes which are served by the digestive apparatus of every kind of animal, whether carnivorous or herbivorous, and the greater complexity of the arrangements in the latter tribes must be considered as intended merely to present a larger surface, and afford a longer time, for the accomplishment of changes which are, in fact, identical in kind, and all of which may be effected in the simplest form of apparatus.

(1.) This apparatus is obviously necessary, as stated by CUVIER, for the support of textures whose vital action is dependent on a continuous supply of nourishment, to afford that continuous supply from aliments, the reception of which, in the case of animals, is only occasional, and sometimes long delayed.

(2.) It is useful, as providing for the separation and immediate expulsion

from the body of those parts of the ingesta which are wholly inapplicable to nutrition, and for which no part of the living structure has any vital attraction.

(3.) It is especially useful, as giving the necessary fluidity to aliments which must be moved to all parts of the animal frame, and applied to the nourishment of the organs in a state of minute subdivision, but which are often introduced into the system in a solid form, having been formed in one living structure, vegetable or animal, and applied to the purpose of nutrition in another, and often after a long interval of time. For this purpose, it appears certain, that various contrivances are employed: in many cases, the mechanical process of attrition is an essential preliminary; in all cases, water is employed; in most cases, it would appear, especially from the observations of LIEBIG, that a certain degree of incipient decomposition—speedily arrested by the action of vital affinities, but beginning on the mucous membrane, and extending to the mass of aliments—precedes and aids the action of the solvent; just in like manner as an incipient decomposition of starch, and formation of soluble sugar, precedes the development of vegetable shoots and flowers; but especially the requisite fluidity is given by solvents, applied at different spots, and which are prepared from the blood, under the influence of appropriate stimuli, by a vital attraction, or selecting power, existing at those parts. Thus, an acid liquor is prepared at the stomach and at the cæcum, and, with a similar intention, according to recent observations, it would appear, that an alkaline liquor is prepared in the salivary glands, liver, and pancreas.

(4.) The most soluble part of the ingesta, and especially the amylaceous portion, must necessarily be taken up by the veins, and carried directly to the liver to form bile; and as this portion, unless combined with azotised matter, is inapplicable to the nutrition of any texture except the fat, we see here one ground for the opinion to be afterwards stated, that the animal matter of the bile is chiefly useful as a part of the provision for the agency of oxygen, and the maintenance of animal heat.

(5.) Although we are uncertain how far transformations of the organic compounds are effected in the animal economy, as preliminary to nutrition, yet we have seen that some such transformations must be admitted as a part of the living power of animals, for the formation of fat, of gelatin, perhaps also of albumen; and this process is pretty certainly commenced in the chyme, in the primæ viæ, and particularly in the organized globules there formed, to be afterwards carried on in the course of the circulation.

2. In the next place, the principles laid down by LIEBIG as to the distinction between the azotised and non-azotised classes of aliments, enable us distinctly to understand the law of PROUT, as to the necessity of a mixture of at least two of the three kinds of aliment which he distinguished, the albuminous, oily, and saccharine, in order to maintain life. In fact, I have no doubt we may go farther (in

consequence of the discoveries made as to the existence of albuminous matter in vegetables since Dr PROUT wrote), and assert that more or less of albuminous matter is always necessary, because it alone, of all the solid or fluid ingesta, contains the azote which is a necessary constituent of animal textures; and that it must be combined either with starch or with oil, or both; partly because oil is an essential constituent of parts of the body, and must either be furnished ready made, or formed in the body from starch; and partly because the animal heat, the first requisite of vitality, can only be maintained by the oxygen of the air combining with carbon and hydrogen in the blood; and if it does not find these elements in sufficient quantity, and in a fit state for such union, in the other constituents of the blood or of the textures, it will attack the albuminous portions of the blood and textures, and so cause decomposition and wasting of the body.

We see likewise the importance of oily food, which, containing the largest proportion of carbon and hydrogen, will yield to the oxygen the largest quantity of carbonic acid and water, and therefore evolve the greatest quantity of caloric,—in cold climates; and of saccharine and amylaceous food which, containing more oxygen in itself, will furnish a smaller quantity of caloric compound with the oxygen of the air,—in warm climates; particularly as the supply of heat from this kind of ingesta is farther regulated and moderated by the action of the liver, in a way to be afterwards considered.

3. We understand the principle, on which the wasting of the body is effected, either in cases of denial of aliments, or of disease preventing their reception or digestion; *i. e.*, we understand that the oxygen of the air, introduced regularly and uniformly in the blood by respiration, but meeting there with very different compounds as the privation of ingesta continues, is the main agent in the process; acting first, as it must do in the healthy state, on the non-azotised compounds existing in the blood, oil, cholesterine, or other constituents of the bile, and starch, or matters recently formed from starch, and nearly destitute of azote, and which readily give up their carbon and hydrogen; next acting on the non-azotised portion of the solid textures, *i. e.*, the fat, and causing emaciation; afterwards acting on the albuminous portions of the blood itself, rendering it more serous; and then acting directly or indirectly on the solid textures, determining ultimately such absorption of the substance of the brain and nerves as causes delirium and insensibility, and such absorption of the muscular textures, as causes death by asthenia. It can only be by successively acting on these different matters, that the oxygen can find the quantity of carbon and hydrogen with which it must unite in the course of the circulation, to account for its own disappearance and for the quantity of carbonic acid which is known to be still thrown off, for days and weeks, while no carbonaceous matter is added to the blood; and the order in which the successive changes on the sensible qualities and functions of the body occur, corresponds perfectly with the belief that the oxygen, acting on the dif-

ferent parts more or less rapidly, as they give up their carbon more or less easily, is the immediate agent by which the extenuation of all is effected.

4. We understand, certainly not completely, but better now than formerly, the nature of the changes which take place in animals long fed on one kind, even of albuminous food, equally as when albumen is withheld; and which appear in both cases to indicate a deficiency of the albuminous constituents of the blood; and likewise, certain phenomena in disease, connected with deficiency of those albuminous constituents.

There are several facts connected with such diseases which we cannot understand, until we have some farther information as to the relation to each other in the living body, of the different constituents of the blood which are albuminous,—the red globules which contain the largest portion of that matter,—the white globules which seem to be more immediately concerned in nutrition,—the albumen of the serum,—and the fibrin, which is in the smallest quantity, and which differs from the albumen only in the peculiar (vital) attraction or aggregation among its particles; and which appears to exist in the living state partly, and, according to ANDRAL'S observations, entirely, in the white globules above noticed. Until the relations of these different matters are better understood, we cannot explain how some of the most striking symptoms of that disease which seems to be the most directly produced by inadequate nourishment, viz., the Scurvy, are produced. But in that disease we now know that there usually is a great deficiency in the quantity of red globules, as well as either in the quantity or in the vital power of the fibrin; and we can now distinctly understand how it should happen that scurvy should shew itself, both when there is a long-continued deficiency of sufficient albuminous nourishment, and likewise when the nourishment taken is too exclusively albuminous;—most frequently, in this last case, when it is at the same time salted and hardened, and difficult of solution in the gastric juice, but, likewise, as repeated experience has shewn, when it is fresh and nutritious, but uniform.* In the first case (exemplified in several prisons of late years), there is a simple deficiency of azotised nourishment; in the other, there is a deficiency of the non-azotised matter which should protect this nourishment; the oxygen of the air therefore acts upon it, and the chief result seems to be, that the formation of the globules, apparently both of red and white globules, is prevented. Both cases are illustrated by what happens in BRIGHT'S disease of the kidneys, where there is such a change in the vital action of these organs, that they throw off prematurely much of the albumen of the blood; the effect of which on the constitution of the blood is to diminish greatly all its azotised constituents, even although a full quantity of azotised food is taken; the specific gravity of the serum falling, and the proportion of the red globules to the other constituents of the blood becoming

* See BUDD on Scurvy, in the Library of Medicine.

as small as in the worst diseases of the stomach ; while at the same time there is a tendency to extravasation, not indeed of the red globules as in scurvy or purpura, but of the serous part of the blood,—equally dependent as the extravasations in scurvy, on the condition of the blood itself.

But not only do we understand that there should be this great deficiency of the albuminous contents of the blood in scurvy, resulting after a time from the use of exclusively albuminous food, equally as from the denial of such food, or the continued morbid discharge of albumen from the blood, or the deficiency of digestive or assimilating power, as in chlorosis ; but we understand, likewise, what appears at first sight paradoxical,—how the evils resulting from this state of the blood should be remedied by the use of food which is not albuminous, by succulent vegetables and vegetable acids. I do not say that we can understand exactly the efficacy of the small quantities of the vegetable acids in particular, which appear to be effectual in relieving the symptoms of scurvy ; but we can distinctly perceive the principle, that, when a quantity of non-azotised matter is taken into the blood, the oxygen of the air will have less power to act injuriously on the albuminous constituents of the blood.

But although the distinction of the azotised and non-azotised ingesta, and the view taken of the chief offices of the two, enable us to understand much that was formerly obscure in regard to these points, yet it is not necessary, in acquiescing in this doctrine, to deny the possibility of the formation of albumen in the animal body. We may state other facts, occurring both in health and in disease, which are hardly consistent with the belief, either that no albuminous matter can be formed there, or that none of the albuminous matter taken into the body is applied immediately to the formation of excretions.

1. When we attend to the invigorating effect of pure air and of exercise on all vital action, and to the evidence we have of the increase of the red globules of the blood (the chief part of its albuminous constituents), and of the muscular texture throughout the body under their influence, it seems hardly possible to doubt, that the effect of the increased introduction of oxygen into the system is a real increase of the deposition of albuminous matter. Now, if there be no formation of albumen in the animal body, the increased introduction of oxygen is the increased application of a cause only of degradation or destruction of such matter ; whereas, if albumen can be formed out of the non-azotised ingesta, as we have seen that there must be a considerable discharge of carbon and hydrogen, by help of the oxygen of the air, before the remaining elements can fall into the arrangement necessary for that purpose, we at once perceive that the effect of pure air and of muscular exertion must be, to increase the formation of that albuminous matter in the blood.

The effect of exercise in preventing or relieving the symptoms of Scurvy, ap-

pears to me peculiarly important in this inquiry. If we suppose that the immediate cause of the diminution of the albuminous matter in the blood, which takes place in that disease, is the action which the oxygen exerts on that matter,—in consequence usually of the small proportion of non-azotised matter which it finds in the blood,—and if the animal system has no power of forming albumen, we do not see how the increased introduction of oxygen should have any but an injurious effect; but if by means of it a part, even a small part, of the blood, consisting of amylaceous and oily matter, can be made to yield albumen, at the same time that it gives out carbonic acid and water, we can distinctly understand how the accession of scurvy should be retarded or prevented. And, in fact, we find that this effect is very generally observed, as the result of habitual and invigorating exercise.

It is stated by Sir E. PARRY, that in Greenland the scurvy seldom makes its appearance among the natives until they confine themselves in their close huts for the winter, although the diet which they use when thus confined is the same as when they are moving about.

In our own country we have had various examples, on a large scale, of scurvy affecting prisoners long confined, although the diet on which they lived would not appear to have been materially different from that on which many of the lower ranks, particularly in Scotland, when at large, preserve their health, and are fit for much muscular exertion. Thus the diet of the prisoners at the Millbank Penitentiary in 1822, on which more than half of them became scorbutic (indeed three-fourths of those above three years confined), consisted of $1\frac{1}{2}$ lb. of brown bread daily, with one quart of soup, which soup had been made with from 2 to 3 oz. of the meat of ox-heads, with 3 oz. of garden stuffs, and was farther thickened with peas or barley; and at Coldbathfield Prison, about the same time, scurvy appeared pretty extensively within a few weeks after the diet had been reduced to $1\frac{1}{2}$ lb. of white bread, with 1 pint either of soup or gruel in the day, and $\frac{1}{2}$ lb. of beef on Sunday.* Comparing this diet with that of many labouring men in Scotland, consuming about $1\frac{1}{2}$ lb. of oatmeal, and *perhaps* 1 pint of milk daily, we can hardly doubt that the air and exercise of the latter exert an influence to improve the condition of the blood; whereas, upon the supposition that the oxygen of the air can give no help in forming albumen, that influence, in so far as the production of scurvy is concerned, should be only injurious.

2. All the phenomena of Scrofulous disease appear clearly to indicate that what we call the scrofulous diathesis, is necessarily connected with a deficiency in the nutritious or albuminous constituents of the blood; and we can now put that proposition in a definite and tangible form, in consequence of the important observation of ANDRAL,—that in numerous trials made on the blood of persons

* See HOLFORD's Second Vindication, &c. &c., pp. 4, 5, 10.

affected with tubercular disease, even in its earliest stage, he had always found the proportion of the red globules, in which the largest part of the albuminous matter is contained, less than the lowest proportion which he had ever found in healthy persons (less than 100 in the 1000 parts, the average proportion being 127). Now there is no proposition, in regard to the external causes of the scrofulous diathesis, which has been more anxiously investigated of late years, or, on the whole, more fully established than this, that it is, *ceteris paribus*, increased by atmospheric impurity and by sedentary habits, and diminished by pure air and exercise. Yet, if the animal frame cannot form albuminous matter, the only effect on the albuminous portion of the blood, of the increased introduction of oxygen which is implied in these circumstances, must be, to hasten the decomposition and expulsion of the albuminous matter absorbed from the *primæ viæ*. I do not state this fact, as affording more than a presumption against that opinion, because I am aware it may be said that, under the influence of fresh air and exercise, a larger quantity of albumen is taken into, or is absorbed from, the stomach and bowels, than in sedentary persons breathing impure air; but in so far as we can judge from the quantities taken into the body, I am pretty certain that the experience of medical men goes to prove that, when the quantities and kind of ingesta are *the same*, the beneficial effects of air and exercise in counteracting the scrofulous tendency,—*i. e.*, as I believe, in increasing the proportion of albuminous matter in the blood, may be distinctly perceived.

Indeed, independently of disease, I am strongly inclined to believe, that the nourishment of the animal body, and especially of the muscular textures, by a given quantity of ingesta, may be distinctly observed to be promoted by exercise, which is hardly conceivable on the supposition, that the only truly chemical changes which take place in the body are of the nature of oxidation, or slow combustion, and consequent excretion, in which the oxygen of the air is the chief agent.

3. The phenomena of Diabetes seem to me very adverse to the idea of the amylaceous matter taken into the system, being wholly inapplicable to the formation of albumen. In that disease, the digestion and appropriation of albuminous matter appear to go on even with unusual rapidity; and the urea which is contained in the urine, often in increased quantity in the early stage, and which is always easily obtained from it in full quantity immediately before death, shews that this matter is ultimately disposed of in the usual way in the animal economy; the amylaceous matter taken in must be the source of all the sugar which is formed in so great quantity, and which characterizes the disease; and it seems to be liable only to that kind of decomposition to which such matter is liable, by simply chemical affinities, at that temperature, and under the influence of water and oxygen; it is converted into sugar, and runs off by the kidneys, *i. e.*, it seems to be actuated by no vital affinity. Now, if all the starch taken into the

living body were useful, as this theory supposes, only by yielding to the simply chemical action of oxygen, and so giving off caloric, we do not see how these changes in diabetes should interfere with that office, or how they should involve so great derangement of the system, and particularly so much gradual wasting of all the textures. But if the starch taken into the system is liable to transformations resulting from vital affinities, and in which albumen is generated, then we can understand, that a disease in which starch seems to lose all tendency to vital action, and is rapidly thrown off, should be attended with this emaciation and debility.

4. When we attend to the phenomena of Lithiasis, *i. e.*, the morbid formation of uric acid, and the effects of different kinds of diet upon it, we meet with facts hardly to be reconciled to the idea of the albuminous ingesta being all destined for nutrition, and the non-azotised for combination with oxygen and excretion. It is well known, that LIEBIG pointed out that this diseased state depends on imperfect oxidation of the albuminous matter in the blood, which is destined to excretion (causing a formation of uric acid, when a fuller oxidation would produce urea and carbonic acid); and that he supposed all the albuminous matter which unites with oxygen in the blood, to be the product of absorption from the textures, the recently introduced albumen being, according to his theory, destined for nutrition only. Hence he argued, that a vegetable diet, increasing the quantity of non-azotised ingredients of the blood, with which the oxygen most readily unites, would leave less oxygen for the azotised or albuminous constituents, and aggravate the disease. But experience has shewn, particularly since the observations of MAGENDIE were published, that the disease is more generally mitigated by a vegetable diet, under which, as it would appear, the whole quantity of azotised matter in the blood and in the urine is diminished, and the oxygen taken in is sufficient for its full oxidation. And the experiments of several authors have shewn, that the quantity of azotised matter thrown off by the kidneys increases greatly (may be nearly doubled) within a few hours after highly azotised food is taken. From which facts it would appear, that the azotised matter thrown off by the kidneys, is derived not merely from absorption of the textures, but likewise directly from the ingesta; and if so, the distinction of the azotised ingesta, destined only for nutrition, and the non-azotised, destined only for excretion, is not observed by nature; and it becomes extremely probable, that, as part of the albuminous ingesta are excreted, so a portion of fresh albuminous matter is formed in the blood, and applied, in the first instance, to the nutrition of textures.

IV. It is at all events certain, that Gelatin is formed in the living body, and its composition, as stated by LIEBIG, $C_{108} N_{18} H_{84} O_{40}$
 or by MULDER, $C_{117} N_{18} H_{90} O_{45}$
 compared with that of albumen, $C_{144} N_{18} H_{108} O_{42}$

seems evidently to denote that it is most probably formed from the elements of albumen, by a farther separation of carbon and hydrogen, aided by the agency of the oxygen of the air. LIEBIG seems to consider it as certain, that this separation must be from the elements of albumen, and, therefore, that gelatin can only be formed from albumen; but it is possible, also, that it may take place from the elements of starch with ammonia, oil being formed at the same time.

If we take the numbers given by MULDER as representing the composition of gelatin, this appears very distinctly. Thus,

	C	N	H	O
To starch,	120	...	100	100
Add ammonia.	...	6	18	...
	120	6	118	100
From this subtract,				
Elements of gelatin,	39	6	30	15
	81	...	88	85
And again, 5 equivalents of fat,	60	...	50	5
	21	...	38	80

which is exactly 21 equivalents of carbonic acid with 38 of water, excreted by the skin and lungs.

The "trioxide of protein," lately so fully considered by MULDER, approaches so nearly in its properties to gelatin, that we may presume its formation will depend on nearly the same conditions; and accordingly we find, that it may be formed from albumen by the long-continued application of heat, air, and water; and that it is formed in large quantities in inflamed parts, where the stagnation of arterial blood (carrying oxygen) and the increased temperature plainly indicate that an increased application of oxygen is going on.

But as there is a remarkable discrepancy of statement as to the chemical relation of gelatin to the albuminous compounds, we must regard the precise nature of the change effected in this department of the animal economy as somewhat doubtful.

In thus attempting to trace the nature of the processes, wherever they may be carried on, by which carbon, nitrogen, hydrogen, and oxygen, uniting with other elements in smaller proportion, fall into the combinations which constitute the animal textures, and in attempting likewise to assign the province of the vital affinities in these processes, we must admit very material deficiency of information. We do not perceive, for example, how it should happen that the amy-laceous matter, which forms the greater part of the ingesta of so many animals.

should hardly appear in their blood, even in that diseased state (diabetes) in which it passes off so copiously, in the form of sugar, by the kidneys. Neither is it easy to understand why the gelatin, formed probably in the course of the circulation, and deposited in so large quantities from the bloodvessels, should not appear in the blood. We are very imperfectly informed as to the origin, the use, or even the composition, of that animal matter, or rather congeries of animal matters, to which the name Extractive is applied. We are still in doubt as to the purposes served by the globules of the blood, both red and white, and the place and mode of their composition and decomposition.

But, admitting all these difficulties as to the details of the chemical changes, still these leading facts are ascertained :—that, in the cells of living vegetables, amylaceous, fatty, and albuminous compounds are formed,—and that, in the circulation through different parts of animal bodies, these compounds are selected and appropriated, and, in some instances, farther transformed, so that a farther formation of oily matter, and a new formation of gelatin, and probably of albuminous matter, takes place, applicable to the immediate nourishment of textures; that all these materials are formed ultimately from carbonic acid, water, and ammonia, existing in the atmosphere; that the carbon, originally fixed from the carbonic acid, is the most essential of all the ingredients, and the proportion of oxygen in all these organic matters, much less than in the inorganic compounds from which they are derived: that the affinities whereby the carbon is enabled to enter into these combinations with the other elements, existing in these organic compounds, to the exclusion of much oxygen, are peculiar to the state of life, and liable to variations by causes which do not affect dead matter; and that, in so far as the oxygen of the air is concerned in the formation of any of these compounds, it acts only by carrying off such portions of carbon and hydrogen, as enable the remainder of those elements to fall into certain new combinations with the others which are there present.

We may state another difficulty here, as leading directly to the next important question in vital chemistry, the rationale of the Excretions; viz., Why does the oxygen, which certainly attaches itself to the red globules in the lungs, not give evidence of its combining with the carbon in them, by giving them the dark colour, until it has passed along the arteries, and through the capillaries of the system, and entered the veins? This fact is noticed both by PROUT and LIEBIG. “The oxygen absorbed at the lungs,” says Dr PROUT, “remains in some peculiar state of union with the blood (*query*, As oxygenated water, or some analogous compound?) till the blood reaches the ultimate terminations of the arteries. In these minute tubes *the oxygen changes its mode of action*; it combines with a portion of carbon, and is converted into carbonic acid.”—(*Bridgewater Treatise*, p. 536.)

LIEBIG goes a step farther in explanation of the change of mode of action of the oxygen, when he says, “The globules of the blood serve to transport the oxy-

gen, which they give up in their passage through the capillary vessels. Here the current of oxygen *meets with the compounds produced by the transformation of the tissues*, and combines with their carbon to form carbonic acid, and with their hydrogen to form water."—(*Animal Chemistry*, p. 60.) But neither author has stated as clearly as I think may be done, on what principle it is that the oxygen changes its mode of action when it meets with these products of the transformation of the tissues; or, in simpler language, with the matters that have been absorbed from the living tissues. I believe the true reason to be, that this is an exemplification of a general principle of essential importance, which has been partially stated, but never, so far as I know, fully developed, viz., that *all vital affinities are of transient duration only*; and that those which actuate the matter of animal bodies especially, soon fail of efficacy, and at the temperature, and under the other conditions there present, give place to simply chemical affinities, which determine the formation of a very different set of compounds; therefore, that as long as the oxygen is passing along the arteries, and is in contact with albuminous matter, to which vital properties have been recently communicated, and which are actuated by vital affinities, it has little power to affect them; but when it meets with the same compounds in the substance of the textures, or already absorbed from them, *i. e.*, with albuminous or other animal matter, which, according to the expression often, but vaguely, used, has become *effete*, or has lost its vital properties, it can act on them in the living body in like manner as it does, at the same temperature, in the dead body.

But, in order to establish this point, it is necessary to enter on the second part of our inquiry into the chemical changes of animal bodies, *i. e.*, the peculiarities of the Excretions; *first*, of the greatest and most general of all the excretions from living bodies, the carbonic acid thrown off from the respiratory organs, both of animals and plants, of which Dr PROUT says, that "the precise use of its constant evolution we know not,"—and *then*, of the other excretions from animal bodies. Until we have precise knowledge of the purpose which is served, and of the laws which are obeyed, by the matters which are continually expelled from living bodies, it is obvious that our notions in regard to vital affinities must be very unsatisfactory. In entering on this subject, I assume it as ascertained that all the matters, peculiar to the excretions from the living body, pre-exist in the blood, and are only eliminated from the blood at the organs where they appear; so that any chemical changes necessary for their formation, take place either in the cells of the textures, or in the circulating blood, or both, not in the glands which separate them, at least not externally to the vessels of those glands.

The first idea that must occur to every one who considers that large quantities of extraneous matter enter into every living body, different from those that can be traced in any of its textures, is, that the excretions from living bodies are simply those portions of the ingesta which are not applied to the maintenance of the or-

ganized structure. And that certain excretions are strictly of this character, seems to be fully ascertained, *e. g.*, the great excretion of oxygen from living vegetables, is merely separated from the carbon of the carbonic acid which enters them, when that carbon unites with the elements of water to form starch; and a part, at least, of the carbonic acid and of the water which are thrown off from a living animal, when it lives on sugar or starch, and forms oil or fat, or when it lives on albuminous compounds and forms gelatin, appears, from what was formerly stated, to be formed, by help of the oxygen of the air, from such portions of the carbon and hydrogen, of the starch or of the albumen, as are excluded when the new arrangement takes place, by which fat and gelatin are formed.

It is important to keep in mind, that, in regard to *all* the excretions, we have sufficient evidence of their being *partly* furnished in this way; *i. e.*, consisting of elements which have been taken into the body, but which are either redundant, or inapplicable to the nutrition of its textures; and that these are thrown off either alone, or combined only with a portion of the oxygen absorbed from the air, and the influence of which on the excretions will be considered afterwards. Thus it is certain, that part of the excretion from the bowels consists merely of unassimilated ingesta. It has been lately stated, with much probability, that certain matters in a putrescent state, absorbed into the circulation, find a natural vent in the mucous glands of the lower intestines.* When we consider that the bile is secreted chiefly from the venous blood of the vena portæ, and that this must necessarily be usually loaded with matters recently absorbed by the gastric and mesenteric veins, and not yet taken into the general circulation; and when we farther remember the small proportion of azote in the animal matter of bile, and the large quantity of this secretion in herbivorous animals, we can have no doubt that much of the matter (particularly the non-azotised matter) taken up by the veins, is brought to the liver only that it may be discharged thence in the form of choleic acid. We know likewise, that certain volatile matters, as alcohol or turpentine, however taken into the system, are excreted by the lungs, either unchanged or united (as in the case of phosphorus), with a certain portion of oxygen. And, in like manner, we have evidence, already stated, in regard to the secretion at the kidneys (although that evidence was not duly considered by LIEBIG), that a considerable part of it is frequently formed from matters recently absorbed into the blood from the primæ viæ, and which had never been applied to the nutrition of textures. As we know that the quantity of uric acid and urea, the most highly azotised of the animal compounds excreted, is much greater under the use of animal (*i. e.*, highly azotised food) than of vegetable, while the health and even the muscular strength

* See CARPENTER'S Physiology, 3d edition, p. 685. This principle is probably of great importance in the pathology, both of hectic and typhoid fever, and of that form of dysentery which seems to result, as a specific inflammation, from certain putrescent miasmata.

may be equal; and that by the use of highly azotised animal food, the animal matter of the urine may be increased, according to CHOSSAT's observations, from 9.9 grains in the ounce to 17; and the proportion of urea voided may be even increased from 237 to 819; and, as we learn from the experiments of CHOSSAT, that a great part of this increase may take place within a few hours after animal food, rich in azote, is taken, we can have little doubt that a considerable part of that azotised food must have passed off by the kidneys without having been applied to the nutrition of any of the textures. And this appears to be confirmed by observations on that disease which arises from a morbid formation of uric acid in the system, because I think two facts may be regarded as nearly ascertained in regard to that state, viz., 1. That it depends essentially on imperfect oxidation of the azotised matters contained in the blood, and destined to excretion;* and, 2. That it is most generally and effectually diminished by a vegetable diet, lessening the quantity of azotised matter taken into the body; whereas, if all the azotised matter destined to excretion had been the production of absorption in the body itself, the introduction of much non-azotised matter, with which the oxygen of the air certainly combines in the circulation, would have left less oxygen to unite with that effete azotised matter, and would have determined, therefore, a greater production of the imperfectly oxidised uric acid, as proportioned to the urea.†

These facts seem sufficiently to illustrate and justify the common opinion, that the excretions are furnished, in part, by such portions of the ingesta as are either inapplicable to nutrition or redundant; and which are, therefore, either

* This is shewn thus—

	C	N	H	O
Uric acid	100	40	40	60
Add water	40	40
„ oxygen,	60
	100	40	80	160
Subtract urea,	40	40	80	40
	60	120 = 60 CO ₂ Carbonic acid.

† LIEBIG, taking for granted that it is the non-azotised portion of the ingesta only, that is united with oxygen from the air in the course of the circulation, thought the use of vegetable food improper in this state of the body, as absorbing the oxygen, and causing, therefore, imperfect oxidation of the azotised matter absorbed from the textures, and about to form urea and uric acid. But the observations of MAGENDIE and others, shewing that both in health and disease the proportion of uric acid formed is generally less under a vegetable diet than an animal, particularly when taken in connection with the facts stated above as to urea, must be regarded as proving, that the idea of non-azotised food having that exclusive tendency to unite immediately with oxygen in the blood, must be erroneous.—See *Carpenter's Physiology*, § 849, 850.

excluded from the new combinations which are formed in a living body, or rejected from the selections which are there made.

Now, if we consider it as ascertained, that a part of all the aliments taken into a living animal body, combines immediately with the oxygen of the air, in the blood, and is thrown off by the excretions in the form of water, carbonic acid, and ammonia,—or in forms which tend towards, and quickly resolve themselves into, these compounds,—we see a distinct confirmation of what was formerly stated, as to the nature of vital affinity, viz., that it does not, properly speaking, supersede ordinary chemical affinities, but is merely superadded to them; so that chemical compounds, taken into animal bodies, are subjected to these attractions as well as others, and are divided between the substances thus acting upon them, in proportions varying probably, as in other cases, according to the strength of the affinities and the quantities of matter exerting them. This, indeed, appears sufficiently demonstrated by the effect of exercise (already considered) on the excretions by the skin and lungs, on the one hand, and on the deposition of fat or of albuminous compounds, on the other; we know, that, as the quantity of carbonic acid and water thrown off are increased by that cause, the quantity of fat deposited from the blood is diminished,—implying that, by the increased quantity of oxygen presented to them by the blood, portions of the carbon and hydrogen of the ingesta, which would otherwise have been subjected to the vital affinity which forms fat, have yielded to the simply chemical affinity which disposes them to unite with oxygen and pass off; and again, it is at least highly probable, that, under this increased supply of oxygen, increasing, by a simply chemical attraction, the proportion of carbon and hydrogen which escape from the ingesta, the effect of the vital affinity by which the remaining elements of the ingesta combine to form albuminous matter, is likewise increased.

But we have next to consider the evidence for the existence, and the object and importance of another and totally distinct source, long believed to contribute to the formation of the excretions, viz., matter which has formed part of the textures of the living body, and been re-absorbed from them, with the intention of being thrown out of the body; *i. e.*, the dependence of excretion on what Dr PROUT calls “destructive assimilation.”

The mixture of this matter with the blood appears to be necessary for all the changes there, from which the different excreted fluids result; or, it may be supposed not merely to escape itself, but to act as a ferment, promoting these changes, and thereby determining the entrance into these combinations, and the expulsion from the body, of the portions of the ingesta which are not required for nutrition.

The term *effete* matter has been very generally employed in discussions on this subject; but it does not appear to me, that any very definite idea has been annexed to the term, nor that any principle has been pointed out to explain how

animal matter becomes effete,—why the absorption of matter once deposited in the textures should be a necessary concomitant of animal life,—or why the elements composing these textures should enter into new combinations, and then should require to be expelled from the body. But I am persuaded it will appear, on examining the subject, that the principle formerly stated, of the *transient existence of vital affinities* in every portion of matter which becomes endowed with them, is both supported by sufficient evidence, and adequate to the explanation of these phenomena.

The leading facts on which this conclusion may be rested are the following :—

1. We know that a continual process of absorption and change of materials is always going on in every living animal texture, and is, in fact, the cause why a continual act of nutrition (the most characteristic of all the functions of animals) is essential, not only during growth, but even in the decline of the body, to the maintenance of its structure and properties.

2. We know that, simultaneous with this absorption, there is a continual process of excretion going on from every living animal, and that, by these excretions, a quantity of all the elements constituting the animal textures is continually thrown off; and farther, it appears to be indicated, although I cannot say fully established by LIEBIG, that the sum of the chemical elements thrown off by the different excretions sufficiently accounts for (the presence of oxygen and water being kept in mind), not merely the *part* of the blood which is not applied to the nourishment of the textures, but the whole of the blood.*

3. We know that the excretions, at least that some of them, not only continue but increase, particularly under any increased muscular exertion, and that their nature remains the same, in an animal deprived of aliment, and in a state of rapid emaciation, as in one that is fully supplied with aliment, and perfectly nourished. “In a starving man, who is in any way compelled to undergo severe and continued exertion,” says LIEBIG, “more urea is excreted than in the most highly fed individual, if at rest. In fevers, and during rapid emaciation, according to PROUT, the urine contains more urea than in health.”†

While these facts prove incontestably that a great part of the matter thrown off from every living body must be the product of absorption from the body itself, let us next consider the information that we have, as to the change which is wrought upon the absorbed materials before they are expelled from the body.

1. The most leading fact in this part of the subject is, that, in the natural state, *none of the organic compounds which exist in the textures, appear in any of the*

* See Animal Chemistry, p. 136 and 152. This conclusion, however, is not to be regarded as established, various fallacies being connected with it. In fact, it seems to me only certain that the carbon and nitrogen are in the same proportions in the excretions as in the blood.

† Animal Chemistry, p. 139.

excretions, although it can only be through the excretions that they disappear from the body, and although the earthy or saline matters absorbed from the textures are there found. The animal compounds existing in the textures must therefore have undergone a great chemical change, in the process by which they are removed from their place in the living body, and finally expelled from it; and this notwithstanding that they are placed in circumstances exactly similar to those, in which their previous original separation and deposition from the blood in the minute capillaries took place.

2. The substances into which these animal compounds (with or without additions derived directly from the *primæ viæ*) have resolved themselves almost entirely before they are thrown off in the excretions, must be, the water which is the basis of all, the carbonic acid thrown off by the lungs and skin, the choleic acid thrown off by the liver, and the urea and uric acid thrown off by the kidneys. All these last we know to be formed in the course of the circulation, not in the organs by which they are separated from the blood; and all possess these essential peculiarities, distinguishing them from the compounds forming the textures; *first*, that they are crystallizable, *i. e.*, the elements composing them are so arranged as to be capable of assuming the definite forms peculiar to inorganic matter; and *secondly*, that they are poisonous to the living body when they are allowed to accumulate in the blood, and, therefore, that their continual expulsion is essential to life.

3. When we farther examine these compounds, into which the animal textures have resolved themselves before they are expelled from the body, we find that they are substantially the same as those, into which these textures are ultimately converted after death, by help of union with oxygen, when in contact with air and water, and at a certain temperature,—viz., water, carbonic acid, and ammonia, the small quantities of sulphur and phosphorus contained in the animal textures, combining likewise with oxygen so as to form sulphuric and phosphoric acids before they are expelled.

	C	N	H	O	
Thus Urea consists of	100	100	200	100	
Add water,	100	100	
	100	100	300	200	= Carbonic acid and ammonia.
Again, choleic acid consists of	76	2	60	2	
Subtract urea	2	2	4	2	
	74	...	56	20	Adding oxygen freely,
	184	
We have,	74	...	56	204	= 74 CO ₂ + 56 HO carbonic acid and water.

Thus the general fact seems established, that the excretions from the living body are only an intermediate stage between the organic compounds, forming the animal textures, and the inorganic chemical compounds into which these are ultimately resolved after death ; and that in the same living body, and in the same parts of it, at the same temperature, and when in contact with the same substances, the same chemical elements, carbon, nitrogen, hydrogen, and oxygen, are continually acting on one another so as to form two distinct sets of compounds ; the one set peculiar to living bodies, always attaching to them certain saline and earthy matters, sulphur or phosphorus, and always taking the form of cells or fibres, never of crystals,—and building up the organised frame ; the other set rejecting those adventitious matters, tending always to the crystalline forms, and to the same mode of combination of the elements as takes place, under the same temperature, where no living structures exist,—and which are always expelled from the organised frame. These are facts of such obvious importance, so generally observed and characteristic, that the physiologist cannot decline to take cognizance of them, and arrange them together, and have some general expression for them. It does not appear possible to express these facts otherwise than by saying, that the particles of these elements taken into living bodies, are under the influence of different chemical laws at different times ; which is exactly what we mean by saying, that they are first actuated by vital affinities (called vital because they are seen only in living structures, and in connection with the indications of life), by which the organised structure is gradually formed, and afterwards by simply chemical affinities by which it is gradually worn down ; and that both are in continual operation during life. And thus it appears that the chemical change, which always attends the absorption, and discharge by the excretions, of all parts of a living body, is simply this.—that they lose their vital properties, and become liable to the same affinities among themselves, and the same action with the oxygen brought to them by the blood, as prevail in the dead state.

This inference as to the loss of vital properties, has been stated by several authors of late years, in regard to those portions of the living solids which perform distinctly vital actions in a visible or tangible form, as the portions of muscular fibre or nervous matter, which are employed in vital motions and sensations ; but as the facts from which we draw the inference are equally true of bones and membranes, and other animal solids, unconcerned in any such vital actions, it seems to me necessary to extend the inference to all those portions of matter which exhibit in a living body the vital affinities, as well as to those which take on any kind of vital movement, or are concerned in any nervous actions.

That oxygen must be the main agent in effecting the changes of these animal compounds, which precede their expulsion in the excretions, is sufficiently proved by observing, *first*, that it is uniformly and necessarily applied to them when these changes are going on ; *secondly*, that the compounds into which the animal

matters are converted before they are excreted, contain a much larger proportion of oxygen than those compounds themselves ; and, *thirdly*, that it is also necessarily applied to all dead animal matter when the decomposition, leading to the same ultimate results, takes place in it.

It is true that the Bile does not contain a larger proportion of oxygen than albumen, but it contains a larger proportion than any kind of oil or fat, from which it appears certain that it is partly formed ; and, farther, we have perfectly good evidence, very well stated by LIEBIG, that by far the greater part of the bile in all animals, and nearly the whole in the carnivora, is re-absorbed into the blood, and exposed gradually to the action of oxygen on it above indicated, and therefore that the secretion of the liver, so far as it is destined to excretion, resolves itself chiefly into the excretion of carbonic acid and water by the skin and lungs, and partially also into that of urea and uric acid by the kidneys ; which arrangement, we have reason to believe, is designed with a view to the maintenance of animal heat, to be considered afterwards.

It may here be a question, whether the simply chemical attraction of the oxygen, carried to the extremities of the vessels in the blood, is the cause, or part of the cause, of the act of absorption, antagonizing the strictly vital attraction by which the elements of nutrition are brought into the cells of the textures. But the power exercised by the excretory glands themselves appears manifestly to be merely that of selection and attraction of the material destined to pass out by them, by an agency of cells quite analogous to that by which the cells of the textures appropriate their own nourishment ; and by this simple and beautiful principle, of certain cells, or the cells in a certain part of the structure, exerting a peculiar attraction for certain matters only, existing in the compound fluid presented to them, nature has provided both for the nutrition and growth of all the textures, and for the expulsion of such matters as must be evolved from the blood, and have not such a property of volatility as might enable them to pass off by the skin and lungs.

It may be objected to the statement now made as to the respective provinces of vital and simply chemical affinities, that vegetable and animal substances removed from the living structures which formed them, are often of long and nearly indefinite duration ; but it would be an error to infer from this fact, that the affinities which led to their formation act as long as they endure ; we can only infer that the conditions, under which other chemical affinities act on such compounds, are not present ; and the general property of the inertia of matter prevents their changing the condition into which they have been once brought, just as the same substance reduced to the state of charcoal may remain long unaltered, although in contact with oxygen, and liable to an affinity with that gas, which, under a slight variation of circumstances, would convert it into car-

bonic acid. "There exists," says LIEBIG, "in every compound a statical momentum (*moment statique*) of the attractive powers which combine the elements; the inertia of the elementary atoms, or their disposition to persist in the same state, or in the same place, where they actually exist, acts there as a special force. If the atoms of sugar were held together by as strong a force as the elements of sulphate of potass. they would suffer as little disturbance as these, from the presence of a ferment or a putrescent body. But this is not the case. The elements of all organic compounds which are capable of undergoing transformations preserve their condition only in virtue of the *inertia*, which is one of their properties."*

Again, it has been reasonably objected to the doctrine of the nutrition and growth of animals being due to an affinity between their textures and the ingesta taken into them, which ceases when these ingesta lose their vitality, that these aliments are very generally in a dead state before they are submitted to the organs of digestion.† But I apprehend the proper answer to this to be, that,—so far as the chemical phenomena of life are concerned, the death of an entire living structure is quite distinct from the death of any one of its component parts. The whole of a living structure dies when its nutrition, the most essential of its functions, is brought to a stand by the failure of circulation; but the organic compounds, formed, as I believe, by vital affinities in that structure, remain for very various periods of time unaltered, or are preserved, as LIEBIG expresses it, by the inertia of matter, from forming those inorganic compounds to which they are ultimately destined; and as long as they remain *fresh*, or, although undergoing decomposition, have not yet reverted to those inorganic compounds, they seem to be still capable of being acted on by the vital affinities of animals. But, when the simply chemical affinities have really resumed their power, when a part of the body has undergone a certain degree of putrefaction,—when the carbon of these compounds has passed into the state of carbonic acid,—or even when this and the other elements have combined so as to form the excretions, which are steps in the process by which they revert to carbonic acid, water, and ammonia,—they are no longer capable of being applied to the nutrition of animal bodies, until they have been again subjected to the influence of vegetable life. The fact of their falling into the combinations which form the excretions, in the act of absorption from the living textures, must be regarded as proof that they have lost their own living properties, and can no longer form part of a living texture, although still within a living structure. This death of the individual molecules forming the living textures, I take to be the counterpart of the continued nutrition of those textures during life, as a general fact in the history of

* "Sur les Phenomenes de la Fermentation," &c. Annales de Chimie, t. lxxi., p. 19, 193.

† See the Review of PROUT'S 4th edition, in British and Foreign Review.

living animals. It is by thus losing their vitality that these molecules become liable to the interstitial absorption (of HUNTER); and their places are taken by fresh molecules by virtue of the vital attraction which constitutes nutrition.

It appears certain also, that the healthy exercise of the vital functions of any texture (although within certain limits it strengthens all the vital properties, and augments the living structure, apparently by attracting an increased flow of blood) determines the more speedy death of the molecules composing it, and the more rapid change of its particles by absorption. This may be expressed by saying, that this mode of vital action, as well as all muscular and nervous action, is subject to the general law of alternate increase and diminution. Hence the increase of absorption, and therefore of the excretions from exercise, even when all ingesta have ceased. And hence, also, if the vital act of nutrition in any texture is morbidly excited, as happens in every case of inflammation tending to the formation of plastic lymph, we have subsequently an increased loss of vitality in the molecules of that part; and therefore, either the formation of purulent matter destined to excretion, or the increased absorption of the newly formed or effused lymph, or the ulcerative absorption of the solids previously existing, or sloughing, or gangrene,—all well-known results of the inflammation, but which have not been duly regarded as all implying more or less partial *loss of vitality*, and therefore dependent on the same principle; and which experience shews to be linked together and even to graduate into one another.

In like manner the progressive absorption of HUNTER is probably to be ascribed to the influence of pressure, injuring and permanently destroying the vitality of parts not intended nor fitted to undergo pressure, and thereby preparing them for absorption and for the action of oxygen.

It is hardly necessary to add to this statement, after the researches of DULONG and DESPRETZ, of DUMAS and of LIEBIG, that the combination of oxygen with the other constituents of the excretions, and particularly with the carbon and hydrogen, is (as has always been maintained by most physiologists in this country) the true cause of Animal Heat; and it cannot be doubted that one of the uses of the aliments, especially the non-azotised aliments, continually taken into the body, is merely to enter into this combination, and fulfil this purpose. But there is one principle on this subject, not so generally recognised, but which the observations of LIEBIG, and likewise of SCHERER, of PETTENHOFFER, and of BOUCHARDAT and SANDRAS,* seem to make nearly certain, viz., that a principal use of the secretion of the Liver (*i. e.* of the animal matter there secreted) is, to serve as a reservoir for the most easily combustible matter which is taken into the primæ viæ; so that,—just as the chyme of the stomach and intestines furnish a pretty constant supply of nourishment from occasional supplies of aliment,—

* See PAGET's Report in FORBES's Journal, April 1846, pp. 561 and 562.

so the Bile from the liver, likewise reabsorbed as it passes down the *primæ viæ*, furnishes to the blood a pretty constant supply of matter fit for calorific combination with oxygen, out of the occasional ingesta.

The proofs of this proposition, and its importance, appear from the following facts, ascertained by these authors. 1. That by far the greater part of the amy-laceous matter taken into the stomach, is converted into soluble matter (dextrine and sugar) in the *primæ viæ*, and these must necessarily be absorbed by the veins, and of course carried to the *vena portæ* and liver. From thence a part of this matter, no doubt, will pass immediately by the *venæ cavæ hepaticæ* to the right side of the heart and lungs, and come immediately into contact with the oxygen; but a part, meeting a portion of effete animal matter in the venous blood will aid in the formation of bile in this way:

	C	N	H	O
4 equivalents of starch	48	...	40	40
Add 1 of ammonia	...	1	3	...
	48	1	43	40
Subtract elements of choleic acid	38	1	33	11
	10	...	10	29

requiring only one part of oxygen to pass into $10 \text{ CO}_2 + 10 \text{ HO}$, carbonic acid and water; which accounts for the great quantity of bile secreted by herbivorous animals; and accounts likewise for the secretion of bile being chiefly from venous blood, inasmuch as very little oxygen is required for its formation, and its chief pabulum has been recently absorbed by the veins. In so far as bile is formed from fat, it must be by help of more oxygen, and, therefore, probably from arterial blood.

2. That of the bile formed and discharged into the intestines, the greater part, even in the herbivora, and almost the whole in the carnivora, is reabsorbed into the blood, and decomposed in the process, the pure bile appearing distinctly in the *fæces* almost exclusively in the case either of diarrhœa, or of the operation of cathartics. When to these facts we add these considerations, that biliary matter retained in the blood, as in one form of jaundice, acts as a poison, and that it cannot be of use in the nutrition of the textures, which is provided for by the albuminous contents of the blood, we can hardly doubt that it is reabsorbed into the blood, only that it (or its elements) may unite with oxygen, and be thrown off as carbonic acid and water, with a little urea; and therefore, that the liver is an appendage to the digestive organs, destined for the proper disposal of the calorific, rather than the nutritious portions of the food, and for the necessary separation of these two; and that the circulation of the matter destined to this

ultimate object, through the liver, answers the important purpose of equalizing the quantity of matter in the blood, which is always ready for this calorific union with oxygen.

This doctrine, as to the chief use of the animal matter of the bile, appears to correspond perfectly with several known and important facts. When the quantity of bile thrown off by the liver, and discharged by the bowels, is decidedly greater than usual, the animal heat is remarkably depressed, as in cholera, apparently because the quantity reabsorbed and applied to the evolution of heat, is diminished. In herbivorous animals, the quantity of bile discharged from the bowels is much greater than in the carnivorous, because the quantity of amylaceous matter which they consume is so much greater, that a much larger quantity is secreted, and if all reabsorbed into the blood, it would cause a morbid increase of heat. Again, in warm climates and seasons, the formation of bile is apparently stimulated, the liver is excited to increased action, and there is such an increase of the discharge by the bowels, as serves to lessen the quantity of combustible matter in the blood, and keep down the temperature of the body; but then this increased stimulation of the liver renders it more liable to various forms of disease.

When we say that oxygen, acting on the redundant, on the non-azotised, and on the effete matters with which it meets in the blood, is the main agent in forming the excretions, and causing the waste of the body, we use language which is, to a certain degree, ambiguous. It seems to me that the oxygen is probably capable of acting on all the matters in the blood for which there is no strong vital affinity in the body; and that the action of the oxygen on the matters which are ready to be, or have been, absorbed from the textures, is rather the consequence, than the cause, of their having lost their vital properties, and thereby come under the dominion of ordinary chemical affinities. The oxygen is, no doubt, the agent by which the gradual extenuation of the body, in death by famine, or by many lingering diseases, is effected, but this agency of the oxygen is in itself salutary, and even necessary to life; the real cause of death is, that cause which prevents the loss of substance effected by the oxygen from being immediately repaired, *i. e.*, it is the deficiency of nourishment, to take the place of those portions of the textures which have lost their vital properties, and therefore come under the dominion of the oxygen.

This seems to be confirmed by the fact which appears to have been fully ascertained by CHOSSAT, that the rate of waste, *i. e.*, the rapidity of absorption of the textures of the body, is greatest shortly before death, *i. e.*, when the supply of the oxygen must be diminished, rather than increased, from the state of the circulation and respiration,—but when the vital powers, and especially the vital affinities, are losing their power, and the supply of nourishing matter has ceased. This fact alone seems sufficient to shew that the absorption, which is constantly

going on in the textures while life continues, is due to the partial loss of vital power of these textures themselves, and is the cause, rather than the consequence, of the agency of oxygen upon them.

When we consider, farther, how exactly this is in conformity with the general fact, that all other kinds of vital action are *essentially temporary*,—that all nervous actions, and all muscular contractions, necessarily alternate with periods of repose,—I think we can have no difficulty in acquiescing in the general law of all Vital Affinities, at least so far as animals are concerned, which explains at once the necessity of constant nutrition of all animal bodies (even when their weight is stationary or declining), the principle of interstitial absorption, the use of respiration, the maintenance of animal heat, and the necessity and nature of the excretions; viz., that as the perpetuation of each species is provided for only by the successive life and death of numberless individuals, so *the life of each individual is sustained only by the successive life and death of all the portions of matter of which its body is composed*; and that each portion, as it dies, falls under the power of the oxygen absorbed from the atmosphere, as it would do in the dead body, and enters into new combinations which are injurious to the living system, but pass off by the excretions; gradually reverting to those inorganic compounds, from which the power of vegetable life only can again raise them to the condition of organized and living matter.

The general conclusions regarding Vital Affinity, which seem to me to be warranted by this review of the subject, and to be sufficiently established to be stated as principles in Physiology, are the following:—

1. That it is by a power peculiar to the state of life, and equally vital as the irritability of muscles, but varying in the different parts of each organized structure, that the solids, and especially the cells of organized matter, attract, select, consolidate, and arrange in their substance, and within their cavities, certain substances, usually compound, which are brought into contact with them, and reject or exclude others.

2. That in the cells of organized matter, during the living state, and apparently by an influence of these cells analogous to that chemical influence to which the term Catalysis has been applied, analogous also to fermentation, certain definite transformations of chemical elements take place, which are equally peculiar to the state of life; which transformations, at least in animals, appear to be effected more in the cells or corpuscles which float in the fluids, than in those which compose the solid part of the structure.

3. That although we have proof that the origin of all the organized beings now seen on the earth's surface has been of recent date, in comparison with the earth itself, we see these powers, thus exercised, continually transmitted to successive sets of cells in each individual, and to successive generations of individuals, with-

out being able to remount to the origin of this kind of action in this, as in others of the sciences lately called palætiological.

4. That the first essential condition necessary for the development of all organized life, is that vital affinity by which, under the influence of light, the cells of vegetables appropriate and decompose the carbonic acid of the atmosphere, fix the carbon, and attach to it the elements of water, so as to form amylaceous matter.

5. That the ulterior changes, effected within organized structures, by which oily, albuminous, gelatinous, and perhaps extractive compounds, are formed and assimilated to the living textures, appear to belong to certain definite vital affinities of the carbon, originally fixed from the air, and which is the basis of all organized substances, not only for the elements of water, but for hydrogen, for azote, for sulphur, phosphorus, and various salts; that most or all these ulterior changes are effected both in vegetables and animals; and that the oxygen taken in by the organs of respiration, although it may be necessary to the play of all the different affinities in living bodies, appears hardly to enter, if it enter at all, into the constitution of any of the compounds thus formed and applied to the nourishment of the textures.

6. That these compounds, in order that they may be applied to this purpose, must be moved within living bodies, and applied, in the fluid form, to the textures which they are to nourish, although in various instances, both in vegetable and animal life, they have themselves the solid form; and that the requisite fluidity is given by various contrivances, chiefly seen in the *prime via* of animals,—by mechanical attrition, by incipient decomposition of the materials employed, but especially by a simply chemical solution of these,—for which purpose certain parts of living structures are endowed with a vital power of separating acids, and others of separating alkalies out of the compound fluids pervading them, and thus preparing solvents for those solids.

7. That the vital affinities do not, strictly speaking, supersede ordinary chemical affinities in the living animal body, but are superadded to them, so that the ingesta, as they come under their influence, are divided between the combinations to which those different kinds of affinity dispose them, and particularly are partly under the influence of the substances exerting vital affinities, and partly of the oxygen of the air, brought to them by the arterial blood; and that as these ingesta often contain large quantities of matter, especially of non-azotised matter, either inapplicable to the formation of the animal compounds, or redundant, these portions, fall immediately under the influence of the oxygen, and form one source of the excretions from the animal body.

8. That the vital affinities, like all living properties, are liable to an influence of *place* and of *time*, which is not seen in the inorganic world, but is an essential attribute of the organized Creation, which has been superadded, in later times, to the original arrangements of the universe. They are acquired by por-

tions of matter which are brought to particular points in previously existing organized structures; they are vigorous for a time, and are then lost. In all the compounds constituting the animal textures, these affinities become gradually enfeebled, whereby the elements constituting these textures become liable to absorption into the blood, to changes in their arrangements, chiefly effected by the oxygen of the air, to combinations with the redundant matters above noticed, and to the formation of other compounds in the blood, which are either the same as, or rapidly tend to, the combinations with oxygen to which animal matter is liable in the dead state; which are, therefore, properly speaking, due to simply chemical affinities, and therefore crystallizable, like other inorganic compounds, and are noxious to the animal economy. This is another source of the excretions, for the separation of which appropriate organs are furnished, capable by their vital power of absorbing and abstracting them from the blood.

9. That the simply chemical power thus exerted by the oxygen, taken in by respiration, over the redundant (especially non-azotised) matter in the blood, and the *effete* matter of the textures, is the source of Animal Heat.

10. That there is thus effected during the life of animals, but in consequence of the failure of their vital affinities, and restoration of the simply chemical relations of their component elements, a change equivalent to the slow combustion of the organized matter, which had been first prepared by the vital affinities of vegetables; and that the carbon, hydrogen, and other elements employed in the formation of that matter, are thus continually resuming that condition, from which the power of vegetable life is continually abstracting them again, to communicate to them a set of properties at variance with those which they permanently possess: and apply them to a succession of organized beings which can only terminate, as at no very distant period of time it must have originated, by an arbitrary act of Divine power.

The gradual change both in vegetable and animal structures which results from age,—the increase of the proportion of earthy and saline matter, and diminution of the proportion of strictly organic matter,—must be regarded as indicating a peculiarity of the vital affinities equally an ultimate fact as their limited duration in every portion of a living body. And the modification to which these affinities, as well as all other strictly vital powers, are liable in animals, from certain actions of the nervous system, must likewise be regarded as an ultimate fact, quite distinct from any principles that have been ascertained in regard to the nature of the vital affinities themselves.

On reviewing the statements and reasonings which I have laid before the Society on the subject of Vital Affinity, although I may have committed errors in the details, I cannot accuse myself of having occupied their time, either with a vague and useless speculation, or with a verbal dispute.

That there is *something* in the history of all living bodies which is *peculiar* to

them, at variance with the laws that regulate the changes of inorganic matter, and requiring to be investigated by a separate induction of facts, must be admitted by all; and is indeed the only reason we can give for treating Physiology, and the branches of knowledge dependent on it, as a separate science; and this being so, it belongs to the very elements of the science to determine what are the portions of the history of living bodies which come under this category.

I have always held in high respect the aphorism of HEBERDEN, which Dr GREGORY used to recommend to the special attention of his pupils, that the great desideratum in medical science is the detection of the Vital Principle, by which all that goes on in the living body is regulated and governed; but I have always thought likewise, that the object of this investigation is rightly limited by Dr PROUT, when he says that we should inquire, “not what the vital principle or vital power *is*, but what it *does*.” In fact, in all the sciences, we can acknowledge only one principle and one Power, as the origin of all the phenomena that we investigate; and when we use these terms in reference to living beings,—when we say that we inquire how the vital principle acts,—we use the term only as a convenient and simple expression for an investigation of the laws according to which the Divine power acts, in regulating the changes which are continually taking place in the last and noblest of the works of creation, and which differ from the changes that we see around us in other departments of nature.

This precise and definite object of all physiological researches—the determination of the laws that are *peculiar* to the science—has always attracted the attention of physiologists, but has not always been placed in the proper point of view; and the common error in this, as in other sciences, has been, to regard the laws of nature as simpler than they really are, and to stretch a principle, ascertained as to one set of phenomena, in the hope that it would be found sufficient to embrace many more. Thus it was easily observed that the phenomena of sensation and thought, and the visible motions in animals, were quite peculiar to them; and when it was ascertained that the first of these, and that a large portion of the latter (*viz.*, all voluntary motions), depend on the living state of the nervous system, it was hastily concluded that all the phenomena peculiar to animal bodies, depend on their Nervous System. This is illustrated by the title of one of the chapters in GREGORY’S “*Conspectus*.” “*De solido vivo, seu genere nervoso*,” as if there were no living property in any of the animal solids but what is given to them by the nervous system; or, by the explicit declaration of CULLEN, that he considered the vital principle as “lodged in the nervous system.”

The progress of the science has, I think, distinctly shewn that these ideas, as to the parts of the animal economy in which the peculiar laws of vitality operate, were limited and erroneous; although physiologists (trained in the schools of medicine where the authority of these and other teachers, adopting similar doctrines, has been held in just veneration) have been generally reluctant to admit the error.

I have endeavoured, in papers laid at different times before this Society, to

limit and define our notions of the powers exercised by the Nervous System, in producing the phenomena of the life of animals, maintaining on that subject the different parts of one general and fundamental proposition; viz., that there is no good evidence, and that in the absence of such evidence it is unphilosophical to assume, that any changes in the nervous system are essentially concerned in producing any phenomena in the healthy state of the system, except those in which *some mental act is necessarily involved*; but that all the powers which are exercised, in the natural and healthy state, by the nervous system, in a living body, are those by which it fulfils its destined office as the seat, and the instrument, of mental acts,—of Sensation, Thought, and Instinctive or Voluntary effort; and that the nature of these powers, and the uses or intention of the different parts, and of all the arrangements of the nervous system, if judged of simply in reference to these, the specific objects of its creation, are tolerably well ascertained: vindicating, at the same time, the doctrine of HALLER, in regard to the separate vital property of Irritability or Contractility in muscles, and its different modes of connection with the nervous system.

I likewise stated, on a former occasion, to this Society the evidence of another fundamental principle in physiology—of the existence and the chief agencies of a power exercised by living bodies, and peculiar to their living state—which is capable of producing motion, or of influencing motion otherwise produced, but which acts in the way of Attraction and Repulsion; and is, therefore, quite distinct from that living power of animal solids, acting in the way of contraction and impulse, which is well understood; and to which, since the time of HALLER, the name of Irritability, or the more general term Contractility, has been applied.

Although both these principles have been strongly contested, I have had the satisfaction of seeing them adopted, and their importance acknowledged, by most of those who have prosecuted the science of Physiology in this country of late years, with the greatest diligence and success. I have now laid before the Society the general grounds of a third opinion, which I hold to be of equal rank in physiology; viz., that there are laws, peculiar to living bodies, acting to a limited extent only, and already in a considerable degree ascertained, which alter and control the ordinary chemical Affinities of the matter composing those bodies, as distinctly as the laws of muscular contraction, or of vital attractions and repulsions, modify the effects of the ordinary mechanical properties of matter within them. And if this doctrine shall, as I confidently expect, be equally admitted to be correct, then, although laying claim to no credit as a discoverer, I hope I may be allowed the satisfaction of reflecting, that I have contributed somewhat towards fixing the foundations of the noble science of Physiology; and establishing those principles in that science, to which continual reference must necessarily be made, in any speculations to which we can apply the epithet scientific, in regard either to the nature of diseases or the operation of remedies.

XXII.—*An Attempt to Elucidate and Apply the Principles of Goniometry, as published by Mr WARREN in his Treatise on the Square Roots of Negative Quantities.* By the Right Reverend Bishop TERROT.

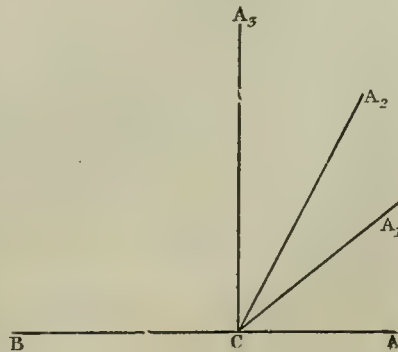
(Read 18th January 1847.)

1. The symbol $\sqrt{-1}$ is called an *impossible* or *imaginary* quantity, because, in analogy with the received laws of algebraic symbolism, it must mean such a quantity as, being multiplied into itself, gives for a product -1 . Assuming, then, that every quantity must be either *plus* or *minus*, it follows that the square of every real quantity must be *plus*; and hence $\sqrt{-1}$, which gives its square *minus*, is called an imaginary or impossible quantity.

If, however, we consider the most simple application of algebra to geometry, we shall perceive that the assumption that every line must be considered and symbolized as either $+$ or $-$, is inconsistent with fact. In algebraic geometry, $+a$ or $+1 \times a$ symbolizes a line whose numerical length is a , drawn in some given direction; while $-a$ or $-1 \times a$, symbolizes a line of the same length, drawn from the same extremity in the same straight line, but in a directly opposite direction. To say, then, that all lines must be either $+$ or $-$, is as much as to say that all lines drawn from the common extremity must be drawn in this one assumed line; and that it is impossible any line should be drawn making an angle with it. But it is evident that an infinite number of such inclined lines may be drawn, and none of them can have $+1$ or -1 as a factor, in accordance with the definition just given of those symbols.

The assumption, therefore, upon which $\sqrt{-1}$ is considered and spoken of as an impossible quantity, is unfounded. All lines drawn from C (Fig. 1.) are as real and possible as CA, which we symbolize by $+1 \times a$, or CB, which we symbolize by $-1 \times c$. None of them, however, except CA and CB, can be symbolized, as to length and position, by a or c multiplied into either a positive or a negative quantity; since that would be equivalent to saying that they are coincident with CA or CB.

Fig. 1.



Ordinary algebra, however, has not provided any system of symbols by which these inclined lines may be expressed, both as to length and position, but affords symbols only for the two extreme cases CA and CB. This deficiency Mr WARREN has undertaken to supply in his Treatise on the Square Roots of Negative Quantities, published in 1828; and has proposed a system of symbols, which, on the same principle as justifies the use of -1 as the coefficient designating the position of CB, designate as coefficients the position of all lines drawn from C, and making angles with CA.

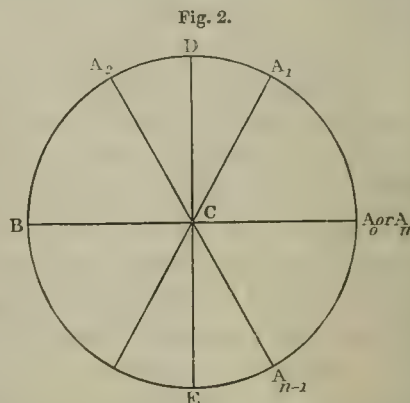
On some points, however, Mr WARREN has been too sparing of his words, and has thus apparently used the common symbols of algebra in a sense very different from their ordinary acceptation. In the following paper I have endeavoured to supply this deficiency of explanation; and then to apply the system of symbols so established to some important problems of goniometry to which, as far as I know, it has not yet been applied. Dr PEACOCK, in his Treatises on Algebra, has made a somewhat similar use of the coefficients of direction, though arriving at his conclusions by a different route.

II. If from C (Fig. 1.) we draw any number of straight lines in the same plane, such that CA, CA₁, CA₂, &c., shall be continued proportionals, according to EUCLID's definition; and make, at the same time, the angles ACA₁, A₁CA₂, A₂CA₃, &c., all equal; then if we call CA=1 and CA₁= a , CA₂ will equal a^2 , CA₃= a^3 , and so on. The several lines then are arithmetically represented as to their respective lengths by the series 1, or a^0 , a^1 , a^2 , &c. But it is manifest that the several indices which determine the length of the several lines, designate, at the same time, the angles which they make respectively with CA. Thus if a' makes with CA, or unity, an angle ϑ , a^2 makes with CA an angle 2ϑ , a^3 an angle 3ϑ , and so on. And conversely the line which makes with CA an angle $n\vartheta$ is properly represented by a^n . If, instead of calling CA unity, we represent it by R or $R \times 1$, then CA₁= $R \cdot a^1$, CA₂= $R \cdot a^2$, and so on.

III. If, next, we assume that the several lines CA, CA₁, &c. are all equal, *i. e.*, that they are the consecutive radii of a circle making equal angles with one another (as in Fig. 2.), the first property, proportion, is not thereby destroyed; and we may still properly represent them (beginning with CA₁) by the series a^1 , $a^2 \dots a^n$.

Now let n be a divisor of $2r\pi$; or, ϑ being that angle which each line makes with

the succeeding, let $n\vartheta = 2r\pi$, or $\vartheta = \frac{2r\pi}{n}$. Then from the last proposition we infer



that n , which is the index of the last term, is also the coefficient of the angle which it makes with that line whose coefficient we assume to be unity, that is, with CA. But $n\vartheta = 2r\pi$, or an integer number of complete circumferences. Hence the radius symbolized by a^n coincides in length and position with the original AC, or $a^n = 1$,

therefore $a^1 = 1^{\frac{1}{n}} = 1^{\frac{\vartheta}{2r\pi}}$.

Now we know, on ordinary algebraic principles, that the several n th roots of unity are properly represented by the several terms of the geometric series $a, a^2, a^3 \dots a^n$, or 1. Since, then, the two series, first that of the successive radii of a circle making equal angles with one another, and secondly, that of the several n th roots of unity are in symbolism the same, it follows, that, dropping this common symbolism, we may take the several roots of unity to represent the successive radii, and conversely.

If, as before, we take not unity but R for the numerical length of the radius, then $R \cdot 1^{\frac{\vartheta}{2r\pi}}$ is the expression for that radius which is inclined to that symbolized by $R \times 1$ at an angle ϑ . And as the direction of the radius, or its angularity

to the original position is noted by the numerator of the index, we call $1^{\frac{\vartheta}{2r\pi}}$ the *coefficient of direction*. We have thus found a function of the angle of inclination which, being affixed as a coefficient or multiplier to the arithmetical expression for the length of the radius, represents the radius so inclined, both in length and position; and which may be employed according to the ordinary rules of algebraic calculation, to find the length and position of other lines under conditions of relation to it.

These *coefficients of direction*, however, it must be observed, have no quantitative or arithmetical value. Thus $a \cdot \frac{1 + \sqrt{-3}}{2}$, expresses a line whose length is simply a ; the coefficient $\frac{1 + \sqrt{-3}}{2}$ affecting not the length, but only the direction of the line.

IV. As illustrative of this reciprocal symbolism, let us suppose that the successive radii are two in number, or, in other words, that a radius revolving round C takes only one fixed position, and makes only two equal angles before it returns to its original position (Fig. 2). Then the circumference is divided into two equal parts, AB is the diameter, and if $CA = 1$, $CB = -1$. In this case $n = 2$, therefore $a^2 = 1$ or $a^2 - 1 = 0 \therefore a = \pm 1$. But the radii being a, a^2 , a must evidently be -1 , and $a^2 = +1$.

Next let the circumference (Fig. 2.) be divided into four equal parts, then CA, CD, CB, CE are the four roots of the equation $a^4 - 1 = 0$. But these roots are ± 1 and $\pm \sqrt{-1}$.

Here CA and CB are, by Art. 1, symbolized by $+1$ and -1 respectively; therefore CD and CE must be symbolized by $+\sqrt{-1}$ and $-\sqrt{-1}$. It is, however, quite optional which direction from C we consider positive, whether in the horizontal or perpendicular line.

V. It appears from the foregoing propositions, that if a line is presented to us under the symbol $a \cdot 1^{\frac{\theta}{2r\pi}}$, we know both its length and the angle θ which it makes with a given line whose coefficient of direction we assume to be unity,

and which, therefore, we symbolize by a simply. The symbol $a \cdot 1^{\frac{\theta}{2r\pi}}$, therefore, represents the actual transference of position in space which a point would undergo by moving from the one extremity of the line to the other, as from A to C (Fig. 3.). But it is clear, also, that if a point be supposed to be removed from A to B, and then from B to C, the actual transference in space, though not the distance travelled, would be the same as if the transference had been direct from A to C. Therefore the symbol which properly represents the one transference, must be symbolically equal to the sum of the two symbols which respectively represent the other two transfereces, or $AC \times \text{its coefficient of direction} = AB \times \text{its coefficient of direction} + BC$ into its coefficient of direction.*

This fundamental proposition is given by Mr WARREN as a definition, That the sum of any two lines making an angle with one another is the diagonal of their parallelogram completed. Even in this startling form, it is only the general assertion of a proposition, particular cases of which are admitted, when we say (Fig. 3.) that $AB_1 + B_1C = AC$, or that $AC + CB_1 = AB_1$. By such assertions we really mean that if a point moves from A to B_1 , and then from B_1 to C, the whole transference in space will be represented by the sum $AB_1 + B_1C$; and that if the point moves from A to C, and then from C to B_1 , the whole transference is expressed by the sum $AC + CB_1$, which is the same thing as the arithmetical difference $AC - B_1C$.

As examples to elucidate this proposition, let us take (Fig. 4.) an isosceles right-angled triangle

Fig. 3.

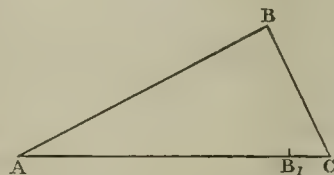
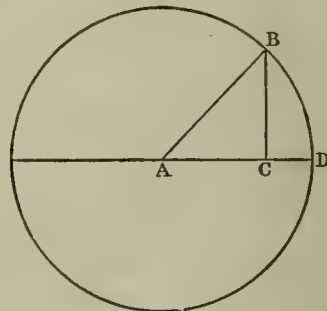


Fig. 4.



* This appears to be the view taken by Sir W. HAMILTON, in the first of his series of papers on Symbolical Geometry, printed in the Cambridge and Dublin Mathematical Journal. He there says, "This symbolic sum of lines represents the total (or final) effect of all those successive rectilineal motions, or translations in space, which are represented by the several summands."

ACB. If we call AB the radius or hypotenuse, a , then each of the sides AC CB is in length $\frac{a}{\sqrt{2}}$, and AB (being inclined at an angle of 45° to AD, which we assume as the original position of the radius) is symbolized by $a \times 1^{\frac{1}{2}} = a \times 1^{\frac{1}{2}} = a \cdot \frac{1+\sqrt{-1}}{\sqrt{2}}$. But $AC = \frac{a}{\sqrt{2}}$, CB being perpendicular to the original position equals $\frac{a}{\sqrt{2}} \cdot \sqrt{-1}$. (Prop. IV.) Therefore $AC + CB = a \cdot \left[\frac{1}{\sqrt{2}} + \frac{\sqrt{-1}}{\sqrt{2}} \right] = a \cdot \frac{1+\sqrt{-1}}{\sqrt{2}} = AB$.

2. Let BAC represent a right-angled triangle whose angle at $A = 60^\circ$, then AB in length and direction $= a \cdot 1^{\frac{2}{3}} = a \cdot 1^{\frac{2}{3}} = a \cdot \frac{1+\sqrt{-3}}{2}$, $AC = \frac{a}{2}$, CB = in length $\frac{\sqrt{3}}{2}$, and therefore in length and direction jointly $a \cdot \frac{\sqrt{3} \cdot \sqrt{-1}}{2} = a \cdot \frac{\sqrt{-3}}{2}$

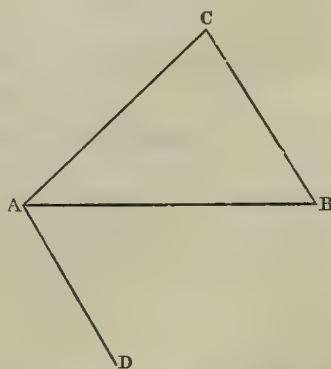
$$\therefore AC + CB = \frac{a}{2} + a \cdot \frac{\sqrt{-3}}{2} = a \cdot \frac{1+\sqrt{-3}}{2} = AB.$$

3. Let the triangle (Fig. 5.) be equilateral, and AB be taken as the original position. Let $AB = a$, $AC = a \cdot 1^{\frac{1}{3}}$, $CB = a \cdot 1^{-\frac{1}{3}}$

$$\begin{aligned} \therefore AC + CB &= a \left[1^{\frac{1}{3}} + 1^{-\frac{1}{3}} \right] = a \cdot \left[1^{\frac{1}{3}} + \frac{1}{1^{\frac{1}{3}}} \right] = a \left[\frac{1^{\frac{2}{3}} + 1}{1^{\frac{1}{3}}} \right] \\ &= a \left[\frac{-1 + \sqrt{-3}}{2} + 1 \right] \times \frac{2}{1 + \sqrt{-3}} = a \left[\frac{1 + \sqrt{-3}}{2} \times \frac{2}{1 + \sqrt{-3}} \right] = a = AB. \end{aligned}$$

VI. In the foregoing Propositions and Examples, it has been assumed that we know not only the several n th roots of unity, but also their proper order, that is, the order in which, as coefficients, they express the radii drawn so as to make angles $3, 23, 33, \&c.$, with the original radius. But when by any analytical process we find the roots of $x^n - 1 = 0$, we procure the symbolical representatives of these radii in no determinate order. To discover this order, we must observe that two roots are always of the form $a \pm \sqrt{-b}$; comparing which expression with figure 6, it is evident that a is the part symbolical of the cosine, and $\sqrt{-b}$ the part symbolical of the sine, because it is affected by the coefficient $\sqrt{-1}$, and is therefore perpendicular to the original radius. It is clear, then, that in the general expression $a \pm \sqrt{-b}$, the sign $+$ belongs to those radii which lie in the upper half of the circle, and $-$ to those which lie in the lower half; and that the two radii whose symbols differ only in the

Fig. 5.



sign of $\sqrt{-b}$, are at equal angles to the original radius, in different directions, that is, on different sides of it.

Again, of those roots which symbolize the radii in the upper half of the circle, that which has a , representing the cosine greatest, is the nearest to the original radii. Thus the roots of $x^6-1=0$, in the order given by Dr PEACOCK, Alg. ii., p, 128, are $1, \frac{-1+\sqrt{-3}}{2}, \frac{-1-\sqrt{-3}}{2}, -1, \frac{1-\sqrt{-3}}{2}, \frac{1+\sqrt{-3}}{2}$. To arrange these in their proper order, if $+1$ be placed first, then -1 , as having no sinal part, and being therefore, neither in the upper nor lower half, must stand in the middle of the remaining roots. Next these are two roots, $\frac{1+\sqrt{-3}}{2}$ and $\frac{-1+\sqrt{-3}}{2}$, each having the sinal part $+$, which must be arranged in this order, because the sign of 1 in the former indicates that the cosine is in CA , and in the latter in CA_n . Finally, considering those roots of which the sinal part is minus; we must place them in the order $\frac{-1-\sqrt{-3}}{2}, \frac{1-\sqrt{-3}}{2}$, because they are thus equidistant from unity with $\frac{-1+\sqrt{-3}}{2}$ and $\frac{1+\sqrt{-3}}{2}$. Hence the roots in their proper sequence are

$$1, \frac{1+\sqrt{-3}}{2}, \frac{-1+\sqrt{-3}}{2}, -1, \frac{-1-\sqrt{-3}}{2}, \frac{1-\sqrt{-3}}{2},$$

symbolizing severally the radii drawn to the extremities of the arcs

$$0 \text{ or } 360^\circ, 60^\circ, 120^\circ, 180^\circ, 240^\circ, 300^\circ.$$

VI. It appears from Props. IV., V., that the radius drawn to the extremity of an arc ϑ , is properly expressed by $1^{\frac{\vartheta}{2\pi}}$, and this again by $a \pm \sqrt{-b}$, where a is what is called in trigonometry the cosine of ϑ , and \sqrt{b} the sine.

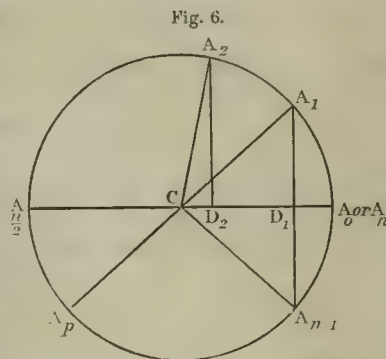
Now let CA_1 (Fig. 6.) make with CA an angle ϑ , CA_2 an angle 2ϑ . . . CA_p an angle $p\vartheta$.

$$\begin{aligned} \text{Then} \quad CA_1 &= CD + \sqrt{-1} \cdot DA_1 = \cos \vartheta + \sqrt{-1} \cdot \sin \vartheta \\ CA_p &= \cos p\vartheta + \sqrt{-1} \cdot \sin p\vartheta. \end{aligned}$$

$$\text{But by Prop. II. } CA_p = \overline{CA_1}^p = (\cos \vartheta + \sqrt{-1} \cdot \sin \vartheta)^p$$

$$\therefore (\cos \vartheta + \sqrt{-1} \cdot \sin \vartheta)^p = \cos p\vartheta + \sqrt{-1} \cdot \sin p\vartheta,$$

which is DEMOIVRE'S Theorem.



COR. If $p \vartheta = 2\pi$, $\cos p \vartheta + \sqrt{-1} \cdot \sin p \vartheta = 1$.

Hence $(\cos \vartheta + \sqrt{-1} \cdot \sin \vartheta)$, $(\cos 2\vartheta + \sqrt{-1} \cdot \sin 2\vartheta)$ &c. represent the several p th roots of unity. If, instead of the order ϑ , 2ϑ , 3ϑ , &c., we arrange the several angles thus in pairs ϑ and $\overline{p-1} \cdot \vartheta$, 2ϑ and $\overline{p-2} \cdot \vartheta$, then the several expressions for x minus the several p th roots of unity, or the several simple factors of the equation $x^p - 1 = 0$, taken in pairs corresponding to the above, will be

$$(x - \cos \vartheta - \sqrt{-1} \cdot \sin \vartheta) \text{ and } (x - \cos \overline{p-1} \cdot \vartheta - \sqrt{-1} \cdot \sin \overline{p-1} \cdot \vartheta),$$

the latter of which equals $(x - \cos \cdot p \vartheta - \vartheta - \sqrt{-1} \cdot \sin \overline{p \vartheta - \vartheta})$

$$= x - \cos \overline{2\pi - \vartheta} - \sqrt{-1} \cdot \sin \overline{2\pi - \vartheta} = x - \cos \vartheta + \sqrt{-1} \cdot \sin \vartheta.$$

In the same way the next pair must be

$$(x - \cos 2\vartheta + \sqrt{-1} \sin 2\vartheta) \text{ and } (x - \cos 2\vartheta - \sqrt{-1} \cdot \sin 2\vartheta), \text{ and so on.}$$

If these several pairs be next multiplied together so as to produce the quadratic factors of $x^n - 1 = 0$, we obtain the products $(x^2 - 2x \cos \vartheta + 1)$, $(x^2 - 2x \cdot \cos 2\vartheta + 1)$ &c. And if it be remembered that in every case $x - 1 = 0$ is a factor; and that if p be even, $x - 1$ and $x + 1$ are simple factors, and consequently $x^2 - 1$ a quadratic factor; therefore if p be even,

$$x^p - 1 = (x^2 - 1) \cdot (x^2 - 2x \cos \vartheta + 1) \cdot (x^2 - 2x \cdot \cos 2\vartheta + 1) \text{ \&c. to } \frac{p}{2} \text{ terms.}$$

But if p be odd,

$$x^p - 1 = (x - 1) \cdot (x^2 - 2x \cos \vartheta + 1) \cdot \text{\&c. to } \frac{p+1}{2} \text{ terms.}$$

Where ϑ , it may be observed, equals $\frac{2\pi}{p}$.

VIII. From these fundamental propositions, Mr WARREN, in his Treatise on Negative Roots, has deduced—

1. The value of each side of a triangle in terms of the other sides and angles. (§ 141.)

2. That the three angles of a triangle are equal to two right angles. (§ 142.)

3. That the sides are respectively proportional to the sines of the opposite angles. (§ 143.)

4. That $\cos A = \frac{b^2 + c^2 - a^2}{2bc}$. (§ 144.)

He then asserts, that from these and the preceding propositions, all the formulæ of plane trigonometry may easily be deduced. In the following propositions, I have applied his principles to the solution of some of the most simple, and to some of comparatively the more difficult problems usually given in elementary books of trigonometry.

$$\begin{aligned} \text{IX.} \quad \sin(A+B) &= A \times \cos B + \cos A \times \sin B \\ \cos(A+B) &= \cos A \times \cos B - \sin A \cdot \sin B. \end{aligned}$$

Let arc AB (Fig. 7.) = A, BD₂ and AD₁ each = B.

$$\text{Then by Prop. III., } CB = r \cdot 1^{\frac{A}{2\pi}},$$

$$CD_1 = r \cdot 1^{\frac{B}{2\pi}}, \quad CD_2 = r \cdot 1^{\frac{A+B}{2\pi}}$$

$$\therefore CD_2 = r \times 1^{\frac{A}{2\pi}} \times 1^{\frac{B}{2\pi}}$$

But Prop. VII.,

$$1^{\frac{A}{2\pi}} = \cos A + \sqrt{-1} \cdot \sin A$$

$$1^{\frac{B}{2\pi}} = \cos B + \sqrt{-1} \cdot \sin B$$

$$\therefore 1^{\frac{A+B}{2\pi}} = \cos A \times \cos B - \sin A \cdot \sin B + \sqrt{-1} \cdot (\sin A \cdot \cos B + \cos A \cdot \sin B)$$

$$\text{but} \quad 1^{\frac{A+B}{2\pi}} = \cos \overline{A+B} + \sqrt{-1} \cdot \sin \overline{A+B}$$

Equating, then, the possible and impossible, or, more properly, the sinal and cosinal, parts of these equal forms

$$\cos A \times \cos B - \sin A \cdot \sin B = \cos \overline{A+B}$$

$$\text{and} \quad \sin A \times \cos B + \cos A \cdot \sin B = \sin \overline{A+B}.$$

This demonstration is the same in principle, and nearly the same in detail, as that given by Dr PEACOCK, in his Algebra, vol. i., p. 392. In his 2d volume, Dr PEACOCK goes more fully into the consideration of the roots of unity as coefficients of direction. Yet there he proves these propositions, not upon that consideration, but by the ordinary geometrical method.

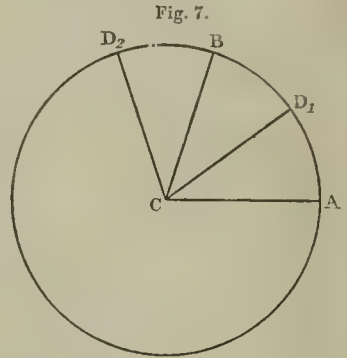
DEF. It should be observed that in the following propositions, a line expressed by letters simply as AB, must be understood as considered in respect both of length and direction; while by the same letters in brackets, thus (AB), is understood the same line in regard to its length only. Thus, if s be the angle

which AB makes with unity, (AB) . $1^{\frac{s}{2\pi}} = AB$.

X. In any right-angled triangle, the sum of the squares of the sides is equal to the square of the hypotenuse.

$$\text{Let} \quad CA \text{ (Fig. 6)} = r, \text{ then } CA_1 = r \cdot 1^{\frac{s}{2\pi}}, \text{ and } CA_{n-1} = r \cdot 1^{-\frac{s}{2\pi}}$$

$$\therefore \quad CA_1 \times CA_{n-1} = r^2 \times 1^{\frac{s}{2\pi}} \times \frac{1}{1^{\frac{s}{2\pi}}} = r^2$$



Also $CA_1 = (CD_1) + \sqrt{-1} \cdot (D_1A_1)$
 $CA_{n-1} = (CD_1) - \sqrt{-1} \cdot (D_1A_1)$ for $(D_1A_1) = (D_1A_{n-1})$
 $\therefore CA_1 \times CA_{n-1} = (CD_1)^2 + (D_1A_1)^2$ which $\therefore r^2 = (CA)^2$

or its equivalent in area $(CA_1)^2$.

XI. COTES' Properties of the Circle.

Let the circumference of the circle be divided into n equal parts; and to the extremities of these let lines be drawn from the centre (Fig. 8), as OP_1, OP_2 , &c., and from any other point C in the diameter. Then

$$\begin{aligned} CP_1 &= OP_1 - OC, CP_2 = OP_2 - OC, \&c. \\ \therefore CP_1 \times CP_2 \times CP_3 \dots CP_n \\ &= \Sigma_n \cdot (OA)^n - \Sigma_{n-1} (OA)^{n-1} \dots \pm OC^n \end{aligned}$$

Where Σ_n is the product of all the coefficients of direction for OP_1, OP_2 , &c., Σ_{n-1} , the sum of these coefficients taken $\overline{n-1}$ together, and so on. But these coefficients (Prop. III.) are also the values of $\frac{1}{n}$, or the roots of the equation $x^n - 1 = 0$. Now the product of the roots of this equation with their signs changed is -1 , and Σ_n is the product with the signs unchanged.

Therefore if n be even, $\Sigma_n = -1$, and, if n be odd, $\Sigma_n = +1$; and in either case, $\Sigma_{n-1}, \Sigma_{n-2}$, &c., each $= 0$.

Hence $CP_1 \times CP_2 \dots \times CP_n = \pm (OA)^n \pm (OC)^n$; the upper signs being used when n is even, the lower when n is odd.

But CP_1, CP_2 , &c., represent the lines considered in relation both to length and direction; therefore, to change the equation into one in which the length only of these lines shall be expressed, we must divide the first side, or multiply the second by the product of all their coefficients of direction.

If n be even, the several pairs, as CP_1, CP_{n-1} , are evidently of the form

$$(CP_1) \cdot 1^{\frac{s}{2\pi}} \text{ and } (CP_{n-1}) \cdot 1^{-\frac{s}{2\pi}} \therefore CP_1 \times CP_{n-1} = (CP_1) \times (CP_{n-1})$$

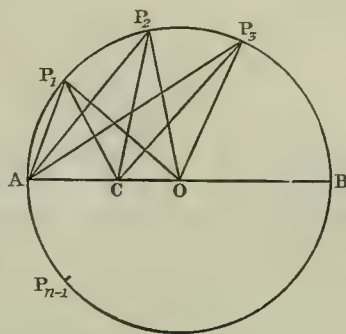
and the same is true for every pair except $CA = (CA) \cdot +1$ and $CB = (CB) \cdot -1$

$$\therefore (CP_1) \times (CP_2) \dots CP_n = [-OA^n + OC^n] \times -1 = OA^n - OC^n.$$

If, again, n be odd, the several pairs remain as before, only, no P falling upon B , -1 is not a coefficient of direction:

$$\therefore (CP_1 \times (CP_2) \times \&c., = OA^n - OC^n \text{ as before.}$$

Fig. 8.



COR. 1. If C be on the opposite side of O from A, the other conditions remaining the same, OC is negative. If n be even, the expression deduced in the proposition remains unchanged. But if n be odd, $(CP_1) \times (CP_2) \times \&c., = OA^n + OC^n$. And here it may be remarked, that when lines, as OA are in the original direction, since the coefficient of direction in that case is unity, it is immaterial whether we write OA or (OA).

Ex. Let $n=3$ and $OC=\frac{1}{2}$

$$\begin{aligned} \text{lien} \quad (AC) &= \frac{3}{2}, (CP_1) = (CP_2) = \frac{\sqrt{3}}{2} \\ \therefore (CA) \cdot (CP_1) \times (CP_2) &= \frac{3}{2} \times \frac{\sqrt{3}}{2} \times \frac{\sqrt{3}}{2} = \frac{9}{8} = 1 + \frac{1}{8} \\ &= 1^3 + \left[\frac{1}{2}\right]^3 = OA^3 + OC^3. \end{aligned}$$

COR. 2. If C be in OA produced, the reasoning and the result will be the same as in the proposition; only, that now CA and CB being of the same affection, -1 is not a divisor of the second number of the equation, and

$$(CP_1) \times (CP_2) \times \&c., = (OC)^n - (OA)^n.$$

XII. If from A, the extremity of the diameter (Fig. 8), the circumference be divided into n equal parts, and lines be drawn to their several extremities from A, then

$$(AP_1) \times (AP_2) \dots (AP_{n-1}) = n \cdot CA^{n-1}$$

As in the preceding proposition $AP_1 = CP_1 - CA$, $AP_2 = CP_2 - CA$, and so on. Therefore $AP_1 \times AP_2 \times \dots \times AP_{n-1} = \overline{CP_1 - CA} \times \overline{CP_2 - CA} \times \&c.,$ to $n-1$ factors

$$= R^{n-1} \cdot \left\{ S_{n-1} - S_{n-2} \dots \pm S_1 \pm 1 \right\}$$

where $S_1, S_2,$ are the sum, sum of products 2 and 2, &c., of all the values of $1^{\frac{1}{n}}$ except unity, there being no line drawn from A to the circumference in the direction CA. $S_1, S_2,$ &c., are, therefore, the coefficients of the equation $\frac{x^n - 1}{x - 1}$, or of $x^{n-1} + x^{n-2} \dots + 1 = 0$, with the signs changed for the products of odd numbers of roots, unchanged for even ones.

If, therefore, $n-1$ be even, $S_{n-1} = +1$, $S_{n-2} = -1$, and so on.

If $n-1$ be odd, $S_{n-1} = -1$, $S_{n-2} = +1$, and so on.

$\therefore AP_1 \times AP_2 \times \&c., \dots = R^{n-1} \times \pm \{1 + 1 + 1 \text{ to } n \text{ terms}\} = \pm n R^{n-1}$
according as $\overline{n-1}$ is even or odd.

If $\overline{n-1}$ be even, then $AP_1 + AP_2 \times \&c. = (AP_1) \times (AP_2) \times \&c.,$ the several pairs of coefficients of direction giving unity as their product.

If $n-1$ be odd, then the several pairs give as before the product unity; but there remains the factor $-AB$, which has for its coefficient -1 .

Therefore, in either case, $(AP_1) \times (AP_2) \dots (AP_{n-1}) = n R^{n-1}$.

XIII. The symbolism employed in the foregoing propositions appears to be applicable to Plane Trigonometry in all its parts. To the elementary propositions of Geometry it is either inapplicable, or applicable by processes and considerations unsuitable to the demonstration of elementary truths. Thus, if by this method we undertake to prove that the angles at the base of an isosceles triangle are equal to one another, we have $(AC) = (BC)$. (Fig. 5.)

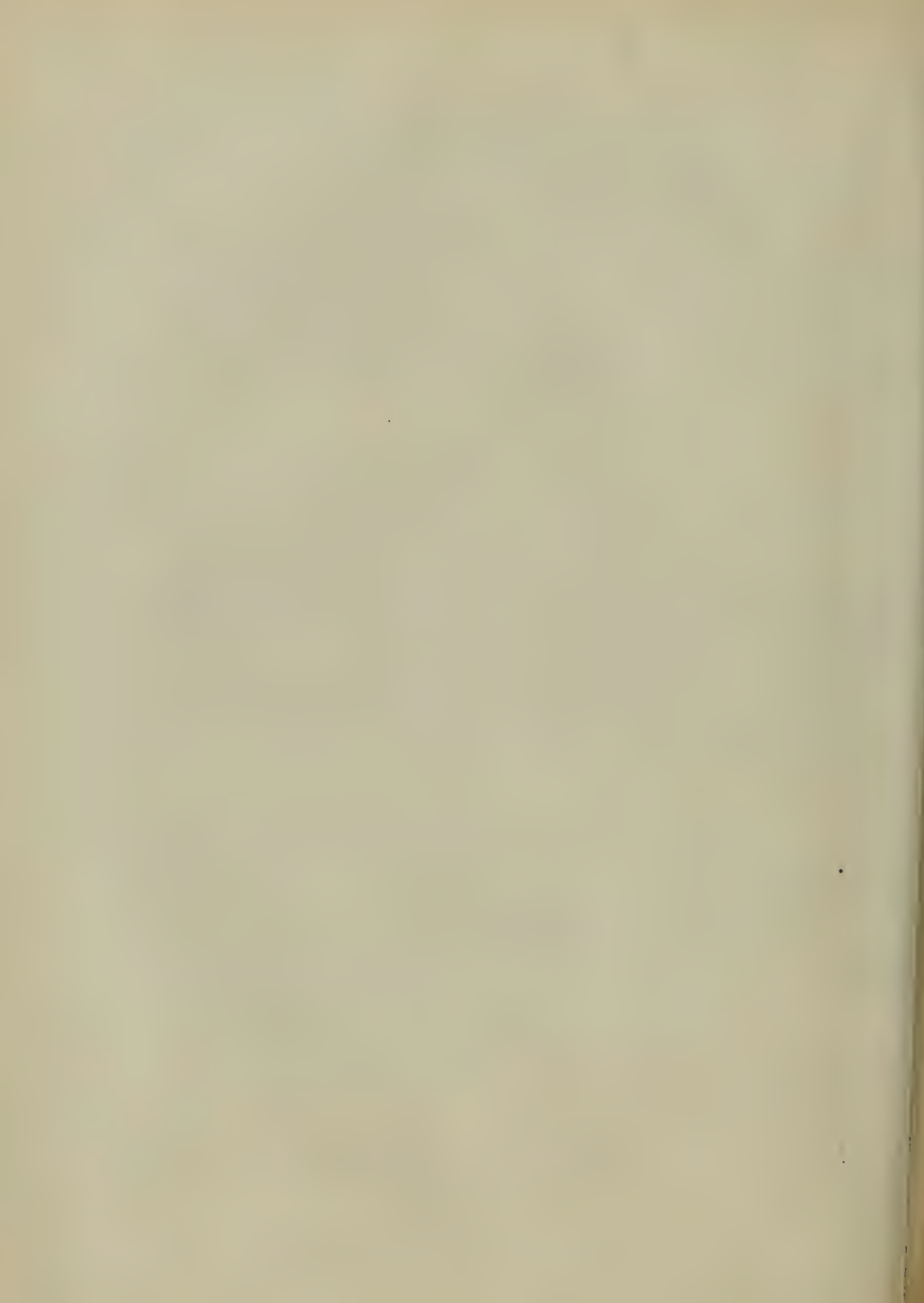
$$\text{But} \quad AC = (AC) \cdot 1^{\frac{A}{2}\pi} = (AC) \cdot [a + \sqrt{-b}]$$

$$CB = AD = (AC) \cdot 1^{\frac{B}{2}\pi} = (AC) \cdot [a' + \sqrt{-b'}]$$

$$\text{But} \quad AC + CB = AB.$$

$\therefore (AC) \cdot [a + a' + \sqrt{-b} + \sqrt{-b'}] = AB$ a positive quantity; consequently the impossible or sinal parts of the coefficient of direction must destroy one another, or $\sqrt{-b} = -\sqrt{-b'}$ or $b = -b'$. Therefore the angles A and B have their sines equal in length, but of different affections. The angles themselves, therefore, being together less than π , are geometrically equal to one another.

COR. Much in the same way we might prove that in every triangle the greater angle has the greater side opposite to it; and, conversely, that the greater side has the greater angle opposite to it.



XXIII.—*On the Reaction of Natural Waters with Soluble Lead Salts.* By ARTHUR CONNELL, Esq., F.R.S.E., *Professor of Chemistry in the University of St Andrews.*

(Read 19th January 1846.)

In a former communication to the Society, I noticed a reaction presented by all spring, well, and river waters which I had examined, that, even after being boiled, they yielded, with acetate of lead, a precipitate readily soluble, in whole or in great part, in acetic acid. This easy solubility in acetic acid shewed that the precipitate was neither a sulphate nor a phosphate, and the comparatively slight action of nitrate of silver proved that it was not a chloride. There seemed, therefore, to remain only the conclusion that it was either a carbonate, or was due to organic matter. The former alternative, of course, depended on whether the solution in acetic acid was attended with effervescence or not; and as this seemed usually not to be the case, and as, on decomposing some of the precipitate by sulphuretted hydrogen, some organic matter in solution was obtained, the conclusion seemed to be, that the appearance was caused by organic matter, probably of the nature of the crenic and apocrenic acids of BERZELIUS. I have since, however, found that by very careful observation, effervescence may be noticed during the solution of the precipitate more frequently than I at first supposed. It is not so easy as might be imagined to determine this point. If acetic acid is added before the precipitate has subsided, no effervescence can be noticed, in almost any case, from the water dissolving the carbonic acid evolved and diffused through the whole liquid. And even when allowed to subside, and the greater part of the liquid is decanted, the addition of acetic acid not unfrequently causes solution without apparent effervescence. The cautious addition, however, of a heavier acid, such as the nitric or even the muriatic, after allowing the lead precipitate to collect at the bottom, and decanting the greater part of the liquid, seldom fails to shew the effervescence where a carbonate is really present.

In so far, then, as the precipitate is dissolved by acids with effervescence, we may conclude that it has been caused by some carbonate remaining dissolved after boiling the water; and in so far as the solution may not exhibit effervescence, we may conclude that it is due to organic matter, provided silver salts do not indicate the presence of a sufficient quantity of chlorides, and provided acetic acid instantly causes solution in whole or great part. In some instances I have found that acetate of lead does not yield a precipitate, *unless* the water has been previously boiled, a circumstance obviously due to excess of carbonic acid retaining the carbonate of lead in solution. In regard to chlorides, I have never met

with any spring, well, or river water, not coming under the denomination of a mineral water, which contained so much of any chloride as to be indicated by a lead salt. The chloride of lead is too soluble to become visible, unless where the contamination is considerable. The portion of the precipitate not soluble in acetic acid is usually due to the presence of some sulphate.

Taking the fact as I have now ascertained it to be, that natural waters which have passed through the strata or soils of the earth, *i. e.*, well, spring, and river waters, very commonly or invariably yield, even after boiling, and filtering, if necessary, a greater or less amount of precipitate with acetate of lead, readily dissolved, in whole or in part, by acetic acid with effervescence; in other words, that such natural waters contain frequently or invariably, even after boiling, one or more dissolved carbonates, the question arises, What is the nature of such carbonate? As these waters, when they have been much concentrated after being boiled and filtered, and have then been made up to their former bulk by distilled water, are found to have lost their power of shewing the same phenomena as before with lead salts, and to have deposited carbonate of lime during concentration, the effect must have been due to this carbonate of lime whilst held in solution. The farther question, therefore, arises, how this carbonate of lime came to be dissolved?

I tried to ascertain whether water would dissolve carbonate of lime in its nascent state as precipitated by boiling a solution in excess of carbonic acid. A current of carbonic acid was passed through lime water prepared with distilled water, until the precipitate at first formed was redissolved. The solution was then boiled for a similar short time as in the original experiments, and filtered. It was then found to be affected only very feebly either by oxalate of ammonia or by acetate of lead; the action being not at all equal to that produced on boiled natural waters by these reagents. The carbonate of lime in the act of precipitation by boiling, had evidently not been dissolved except in very insignificant quantity by the water. I then, before boiling the solution, exposed it for a day to the air in an evaporating basin, after keeping it for some days in a close vessel; but did not find that the quantity of carbonate retained after ebullition, short subsidence, and filtration, was increased.

As it was possible that some of those saline matters contained in natural waters might promote the solubility of carbonate of lime, minute quantities of solutions of muriate of lime, sulphate of lime, muriate of magnesia, and chloride of potassium were added to lime water prepared with distilled water. A current of carbonic acid was then conducted through the liquid so as to redissolve the precipitate which it at first caused. The liquid, after ebullition, short subsidence, and filtration, was found scarcely to be affected by acetate of lead; and any feeble deposit formed was not soluble in acetic acid, being sulphate of lead due to the sulphates which had been added.

It thus seemed evident that by the aid of carbonic acid no sufficient quantity

of carbonate of lime can be dissolved, independently of the continued presence of the free acid, to cause the appearances referred to.

I next tried the solvent action of water alone on finely divided carbonate of lime. Distilled water, which had been boiled and cooled, was left in contact with marble in impalpable powder for several days in a close vessel. It was then found by the action both of acetate of lead and of oxalate of ammonia, that rather more carbonate of lime had been taken up by the pure water than was left in solution after boiling the carbonated water, but still that the amount was considerably less than the reactions which have been referred to, indicate in ordinary natural waters ; and it is remarkable that the effect of the lead salt is usually more decided than that of the oxalate.

I incline, therefore, to think, that the carbonate of lime present, in such circumstances as have been described, has a different origin, viz., Double Decomposition, between a lime-salt and a carbonated alkali ; as it would seem that the carbonate of lime formed is, in this kind of nascent state, dissolved more readily than when precipitated by boiling from a carbonic solution. The following experiments illustrate this origin of the reaction. To half an ounce of distilled water, a few drops of a solution of sulphate of lime in water were added. A single drop of solution of chloride of calcium, and a single drop of solution of carbonate of potash were then added. The liquid remained quite transparent, and did not affect turmeric or cabbage paper. When boiled it still remained transparent. When a drop of solution of acetate of lead was added to a portion of this liquid, either before or after boiling, a considerable white cloud was formed, which disappeared on the addition of a drop of acetic acid. Thus was the reaction of the spring waters exactly imitated. I at first inclined to think that in such cases no actual double decomposition ensued until the liquid was concentrated by heat, and that the action on the lead-salt was due to the carbonated alkali present. But farther experiments lead me to believe that the carbonate of lime is actually formed, at least to a considerable extent, and then dissolved by the water ; for, if a couple of drops of solution of chloride of calcium and a drop of solution of carbonate of potash be added to a few drops of distilled water, muddiness will be produced ; and this will disappear when half an ounce of distilled water is shaken with the mixture, without any deposit being formed by rest. The solution, however, of the carbonate of lime is dependent on the action of the water taking place either on the nascent salt, or at least immediately after its formation ; for I found that when carbonate of lime, precipitated by double decomposition, was collected on a filter, washed, and allowed to stand some minutes, and then left in contact all night with boiled and cooled distilled water, acetate of lead had only a very feeble effect on the liquid.

It was, of course, necessary, in order to establish this view, to ascertain that those natural waters which exhibit the reaction referred to, actually contain

alkaline matters. This was, accordingly, done in numerous instances by ordinary methods. Although the potash or soda present may have been originally dissolved as a carbonate; yet we, of course, ultimately obtain it on evaporation, as a chloride or sulphate, through double decomposition with the lime or magnesian salts present; or through the stronger affinity of the acids of these salts, if their earths have been previously removed by chemical means. In no instance of a natural water which gave the reaction with lead salts, did I fail to detect either potash or soda, or both; and it ought to be recollected that a very minute quantity of either is sufficient. It will be found that one drop each of solutions of carbonate of potash, of sulphate of magnesia, and of chloride of calcium, added to several ounces of distilled water, will produce the reactions referred to with lead salts and acetic acid.

If these views are well-founded, it is evident that lead salts become a probable indication, at least where their effect is considerable, of the presence of alkalis in natural waters. And, in general, we may conclude, that if after boiling, and filtration if necessary, any water yields a considerable cloud with acetate of lead, readily soluble by adding a drop or two of acetic acid, the cause will be either carbonate of lime, probably due to double decomposition, or it will be organic matter, if any such matter precipitable by lead salts is present in sufficient quantity.* In so far as it is dissolved by an acid, after subsidence, with effervescence, it will be due to the former cause; in so far as, without effervescence, to the latter.

It seems, at all events, evident from the experiments which have been detailed, that the carbonate of lime present has not owed its presence to the solvent agency of carbonic acid, even when first taken up.

It is plain, that the carbonate of lime thus held dissolved by spring waters, from whatever source it may be obtained, must be of considerable importance in the economy of nature in furnishing a supply, through the intervention of these waters, of that lime which is so essential a constituent, in its various states of combination, of the inorganic portion of plants. This will hold whether such waters are applied to the land in the way of irrigation, or by the more slow processes of natural infiltration.

* Dr CHRISTISON informs me that moss-water is not precipitated by acetate of lead. This, I have no doubt is a correct observation; but still other states of organic matter may occasion a precipitate. The crenic and apocrenic acids are both known to precipitate lead salts.

Although fluorine is now known to be occasionally present in ordinary natural waters, and although fluoride of lead is sparingly soluble in water, yet I am not aware that fluorine is ever present in such quantity in such waters as to affect lead salts; and, if it were, acetic acid might very likely not dissolve the precipitate. Dr WILSON mentions that fluoride of barium is less soluble in acids than carbonate or phosphate of barytes.

POSTSCRIPT.

Since this paper went to press, I have ascertained that the town water of St Andrews, which is one of those which gives the reaction referred to with lead salts, yields by evaporation, after having been boiled and filtered, $\frac{1}{31323}$ of its weight of carbonate of lime. Other waters may of course contain more. I also observe, that FRESSENIUS found, that when distilled water was boiled a long time (probably, from the context, several hours) with freshly precipitated carbonate of lime, so as to form a saturated hot solution, and this solution was then kept for four weeks at common temperatures, in contact with undissolved carbonate of lime, under frequent agitation, it yielded by evaporation $\frac{1}{10601}$ of its weight of carbonate of lime.—*Liebig's Annalen*, July 1846. In so far as regards spring waters, it is unnecessary to say, that Nature does not take such pains to charge them with lime. The method suggested above seems a more simple one, and may often be as effectual, possibly even more so; when the still more simple means of free carbonic acid are not brought into play. From the experiments of FRESSENIUS, it appears, that carbonate of lead is much less soluble in water than carbonate of lime, viz., in 50,551 parts, which is quite conformable to the results above stated.

XXIV.—*On certain Products of Decomposition of the Fixed Oils in contact with Sulphur.* By THOMAS ANDERSON, Esq. M.D., F.R.S.E., *Lecturer on Chemistry, Edinburgh.*

(Read 19th April 1847.)

Numerous researches have established as a general rule that the products of the decomposition of organic substances vary with the circumstances of the experiment, and the nature of the agents under the influence of which it is performed. If, for instance, we examine the action of heat alone, we find it causing a set of decompositions specially characterised by the evolution of carbonic acid, formed by the union of part of the carbon of the substance with the whole or part of its oxygen; and this action is rendered more definite, and the number of the products circumscribed by all circumstances facilitating the formation of carbonic acid, such as the presence of a base, which will even cause its evolution when heat alone is incapable of producing decomposition. Acids, on the other hand, have a precisely opposite effect, they, in some instances, altogether prevent the formation of carbonic acid, and cause the oxygen to exert its action on the hydrogen of the compound, and to eliminate one or more atoms of water which do not generally exist ready formed in it.

In these particular instances decomposition takes place at the expense of the constituent atoms of the compounds themselves, the extraneous substances serving merely as disponents to the oxidation, in the one case of part of their carbon, in the other of their hydrogen; but there is another class of agents which, besides eliminating one or more substances, are capable at the same time of entering into union with the residual atoms, and forming a new derivative of the original compound. The best investigated of this class of agents are chlorine, bromine, nitric acid, and ammonia, the three former of which exert their action on the hydrogen, the latter on the oxygen of the substance, and form compounds the complete investigation of which is important, not merely in a purely chemical point of view, but also from the light which they seem likely to throw on the general question of the atomistic constitution of matter. In fact, the great object of the researches of organic chemistry at the present moment is that of developing the relations which the individual atoms bear to the molecules of their compound, by a knowledge of which we hope eventually to arrive at some definite conclusions with regard to the mode in which the elementary atoms are grouped together in a complex molecule. Almost all the scanty information which we possess on this subject has been derived from investigating the products of the action of different agents upon organic substances; and it is sufficiently obvious, that the more varied the circumstances, and numerous the points of view under which these re-

actions can be examined, so much the more likely are we to arrive at definite results.

It was the consideration of these points which led me to undertake an investigation into the nature of the action of Sulphur in the free state upon organic compounds, a subject hitherto totally uninvestigated, unless we except the curious researches of ZEISE* on the simultaneous action of ammonia and sulphur upon acetone, which yields a variety of remarkable products, the properties of which he has described, without however determining their constitution. The results at which I have already arrived in these researches are contained in the following pages. They are, however, to be considered only as the commencement of the investigation; and I am desirous of submitting them to the Society even in their present very imperfect state, as it is impossible to fix a period within which a series of researches, surrounded by so many difficulties, can be completed. No one who has not been specially occupied with such experiments can have any conception of the numerous sources of annoyance which they present, and of the expenditure of time and labour which is necessary for their performance. Indeed, I have more than once felt inclined altogether to abandon a subject occupying so much time in proportion to the results obtained, and the completion of which is further protracted by the nauseous odour of the compounds, which is so disgusting that it is impossible to pursue the investigation for any length of time continuously.

At the commencement of these researches I endeavoured to examine the action of sulphur upon some of the simpler organic compounds, in the hope of arriving at results of corresponding simplicity. My expectations, however, were disappointed, and I was obliged to have recourse to the fixed oils, on which sulphur has been long known to exert an action; the product obtained by heating together olive oil and sulphur until an uniform balsam-like substance was formed, having been employed in medicine by the older physicians under the name of the Balsam of Sulphur.

The phenomena which manifest themselves during the mutual action of Sulphur and a Fixed Oil are these:—At the first application of heat the sulphur melts and forms a stratum at the bottom of the oil; but as the temperature rises it slowly dissolves, with the formation of a thick viscid fluid of a dark red colour. As the heat approaches that at which the oil undergoes decomposition when heated alone, a violent action takes place attended by the evolution of sulphuretted hydrogen in such abundance that the viscid mass swells up and occupies a space many times its original bulk. If at this point the mixture be allowed to cool, it concretes into a tough sticky tenaceous mass, adhering strongly to the fingers, and having a disagreeable sulphureous odour; if, however, the heat be

* Förhandlingar vid de Skandinaviska Naturforskarnes tredje möte, p. 303.

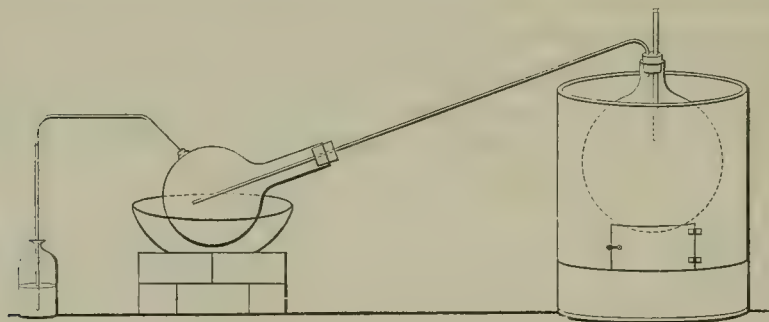
sustained, the frothing and evolution of sulphuretted hydrogen continue, and at the same time, an oil of a peculiar disgusting odour, resembling that of garlic, but more disagreeable, passes into the receiver.

In the investigation of the products of this action, the first and most essential step was to determine the particular constituents of the oil from which they are derived. In order to do this, it was necessary to examine separately the action of sulphur upon each of its components. I commenced, therefore, by making use of stearic acid, which can be readily obtained in a pure state: experiment however, shewed, that none of the peculiar products were derived from it: for when mixed with half its weight of sulphur and distilled, mere traces of sulphuretted hydrogen were evolved, and the products were identical with those obtained from the unmixed acid. The nauseous smelling oils being then obviously derived either from the oleic acid, or the glycerine of the oil, I prepared a quantity of pure oleic acid, by the decomposition of the ethereal solution of the oleate of lead. This, when mixed with half its weight of sulphur, and distilled in a capacious retort, underwent decomposition precisely as the crude fixed oil did: sulphuretted hydrogen was developed in great abundance, and the product of the distillation could not be distinguished from that which I had previously obtained. I was unable to obtain glycerine in sufficient quantity to make a separate investigation of the products of its decomposition, but these must also be peculiar, as I could not distinguish the presence of acroleine during any period of the distillation of an oil with sulphur.

The product of the distillation of oleic acid was in the form of a reddish-brown oil, having an extremely nauseous odour, in which that of sulphuretted hydrogen was apparent. When rectified, this sulphuretted hydrogen was driven off, and the first portions which distilled were perfectly transparent and colourless. As the process continued, however, the products became gradually darker in colour, and the last portions which distilled became semisolid on standing, from the deposition of a quantity of white crystalline plates. These were separated by filtration through cloth, expressed strongly, and purified by successive crystallizations from alcohol, until they were entirely free from smell and colour. The product was then in the form of white pearly scales, which possessed acid properties, and were totally insoluble in water; they were not therefore sebacic acid, no trace of which could be discovered among the products; but, on the contrary, possessed all the properties of margaric acid. These crystals were obtained from quantities of oleic acid, prepared at different times, and with the greatest possible care, and must have been formed during the decomposition. In order, however, to set this point at rest, some of the same oleic acid was distilled alone, when abundance of sebacic acid was obtained, and the latter portions of the rectified product did not deposit any crystals on cooling, but remained perfectly fluid. As this solid acid is produced only in comparatively small quantity, and

I was unable to obtain enough of oleic acid, I made use, in preparing it on the large scale, of pure almond oil, which, according to SCHÜBLER and GASSEROW, is entirely free of margarine. The oil which I employed was expressed specially for these experiments, at a temperature slightly above 32° ; and in order to satisfy myself of the absence of margarinic acid in the products of its ordinary decomposition, a quantity was distilled alone, and the product rectified. The latter portions being collected apart did not deposit margarinic acid; and this I have also found to be the case with the ordinary almond oil of commerce, in the expression of which a moderate degree of heat is employed.

In distilling the oil and sulphur on the large scale, it became impossible to perform the process by the simple admixture of the substances, the frothing being so great as inevitably to expel the materials from the retort. After a trial of various methods, I found it most convenient to employ the apparatus, of which this is a sketch. The oil was introduced into a large glass balloon, to the mouth



of which two tubes were adapted, one descending to near the middle, and furnished with a cork at the upper end, the other which constituted the neck of the distilling apparatus passed into a tubulated receiver, kept cold by immersion in water or ice. To the tubulature, a doubly bent tube was affixed, which descended into a vessel of alcohol, for the purpose of retaining any of the more volatile portions which might be carried over by the rapid current of sulphuretted hydrogen. The heat must be applied by means of an open charcoal fire, and the furnace should be so constructed, that the fire may be rapidly withdrawn in the event of the action becoming too violent. It is very desirable too, that the balloon should go down into the furnace, so that it may be entirely surrounded by hot air. The oil is introduced into the balloon, of which it must not occupy more than a fifth, or a fourth at most, along with a few small pieces of sulphur, and heat is gradually applied. So soon as effervescence commences, the cork of the small tube is withdrawn, and a small piece of sulphur is introduced; and this is continued gradually, so as to keep up an uniform action. A dark reddish-brown oil passes

into the receiver, and at the same time sulphuretted hydrogen passes in torrents through the alcohol; it there deposits a certain quantity of oil, and on escaping, may be kept burning during the whole operation, with a flame eight or nine inches high. The principal difficulty of this process consists in regulating the heat, so as to keep up a steady action. If the heat be allowed to fall, the contents of the balloon become so viscid, as inevitably to boil over; and at the same time too high a temperature causes the whole action to go on with excessive violence. I have generally operated on quantities of three pounds, each of which requires a complete day for its distillation, during which time the operator must never leave it, but constantly attend to the regulation of the heat, and the gradual addition of sulphur in small quantities. When a quantity equal to about half the oil employed has distilled over, the remaining mass becomes excessively viscid; and just at this point the balloon frequently cracks, the contents escape, and the whole catches fire, and blazes off with a bright yellow flame, and smell of sulphurous acid.

The product of this distillation, which exactly resembled that of the pure oleic acid, was rectified, and the crystals which deposited from the latter portions were expressed and purified by successive crystallizations in alcohol. They then presented all the characters of margaric acid, and gave the following results of analysis:—

I.	{ 5.275 grains of the acid gave		
	14.558	...	carbonic acid, and
	5.919	...	water.
II.	{ 6.358 grains of the acid gave		
	17.578	...	carbonic acid, and
	7.212	...	water.

Which gives the following results per cent.—

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	75.27	75.40	75.55	C ₃₄	2500.0
Hydrogen, . .	12.51	12.66	12.59	H ₃₄	425.0
Oxygen, . . .	12.22	11.94	11.86	O ₄	400.0
	100.00	100.00	100.00		3325.0

These results agree completely with the formula for margaric acid, and were farther confirmed by the analysis of its silver salt and ether.

4.643 grains of the silver salt gave 1.325 of silver = 28.53 per cent.

7.926 grains of the silver salt gave 2.284 of silver = 28.70 per cent.

The calculated result for margarate of silver gives 28.65 per cent.

The ether was prepared in the usual manner, by dissolving the acid in absolute alcohol, and passing dry hydrochloric acid gas through the solution. The product, which possessed all the properties of margaric ether, gave the following results of analysis :

$\left\{ \begin{array}{ll} 5.596 \text{ grains of the ether gave} \\ 15.662 \quad \dots \quad \text{carbonic acid,} \\ 6.399 \quad \dots \quad \text{water.} \end{array} \right.$			
Experiment.		Calculation.	
Carbon,	76.33	C_{38}	2850.0
Hydrogen,	12.70	H_{38}	475.0
Oxygen,	10.97	O_4	400.0
	100.00		3725.0

These analyses establish, in a satisfactory manner, that the acid produced was margaric acid. It is scarcely possible, however, in the present state of the investigation, to give anything like a rational explanation of the mode in which it is here formed. Its production from oleic acid has been already observed by LAURENT as the first product of oxidation by nitric acid; but the action of sulphur is certainly of a very different character, and cannot be considered as bearing any analogy to that of an oxidising agent. The quantity of margaric acid produced does not appear to be constant, but varies with the rapidity of the distillation, and is always most abundant when it is slowly performed.

The oil which distils previous to and along with the margaric acid, and constitutes by far the most abundant product of the action of sulphur upon oleic acid and oil of almonds, is a very complex substance, and contains some of its constituents in very small proportion. On this account I found it necessary to prepare it in very large quantity; and in doing so I abandoned the use of almond oil and employed linseed oil instead, which is a much cheaper substance, and yields the same fluid products. When the product of the action of sulphur is carefully rectified, the first portions which pass over, are perfectly transparent and colourless, highly limpid and mobile, and boil at the temperature of 160° Fahr. Only a small quantity, however, passes at this temperature, and the immersed thermometer gradually rises without indicating any fixed boiling point for the fluid. My first attempts to purify this oil, and separate it into its various constituents, did not afford any satisfactory conclusions. Numerous analyses of the more volatile portions were made without obtaining comparable results, although all indicated the presence of carbon and hydrogen nearly in the proportion of equal atoms. The following are the details of three of these analyses:—

I.	{	4·657 grains of the most volatile oil gave	
		12·688 ... carbonic acid, and	
		5·127 ... water.	
II.	{	5·501 grains of an oil less volatile than the preceding gave	
		15·762 ... carbonic acid, and	
		6·292 ... water.	
III.	{	4·191 grains of another portion of oil gave	
		12·185 ... carbonic acid, and	
		4·720 ... water.	

Which correspond to the following results per cent. :

	I.	II.	III.
Carbon,	75·03	78·79	79·95
Hydrogen, . . .	12·20	12·72	12·75

All these oils, when treated with fuming nitric acid, yielded an abundant precipitate of the sulphate of barytes; but as the results of the combustion were not constant, no quantitative determination was made.

The action of precipitants, however, upon this oil, afforded a more satisfactory method of obtaining some of its constituents. It gives, with corrosive sublimate, a bulky white precipitate, and with bichloride of platinum, a yellow compound, the characters of which vary slightly, according as it is prepared from the more or less volatile portion of the oil. Nitrate of silver and acetate of lead, mixed with the alcoholic solution of the oil, produce only a slight cloudiness, but on boiling the solutions, the sulphurets of silver and lead are deposited.

The Mercury Compound. In order to obtain this substance in the pure state, the oil was dissolved in alcohol, and an alcoholic solution of corrosive sublimate added. The precipitate which fell was collected on a filter, and washed with ether, until the oil was thoroughly extracted, for which purpose a considerable quantity of ether is required. It is then boiled with a large quantity of alcohol, which dissolves a part of it, and the solution being filtered hot, allows the compound to deposit, on cooling, in the pure state. It is then in the form of a white crystalline powder, having a very fine pearly lustre, and exhibiting under the microscope crystals of a very peculiar form. They are six-sided tables, two opposite angles of which are rounded off, so as to give them a very close resemblance to the section of a barrel. It possesses, even after long-continued washing with ether, a peculiar slight sickening smell, which becomes more powerful on heating, and its powder irritates the nose. It is insoluble in water, which moistens it with difficulty. It requires several hundred times its weight of boiling alcohol for solution, and is almost entirely deposited, on cooling, in microscopic crystals. In ether, it is almost insoluble. When heated, it is decomposed with the evolu-

tion of a peculiar nauseous smelling oil. The sparing solubility of this compound in alcohol renders its preparation in sufficient quantity for analysis an extremely tedious process, and I have sought in vain for a more abundant solvent. The only substance which I have found capable of taking it up in larger quantity, is coal-tar naphtha, but its employment is inadmissible, as the best which can be procured is an extremely impure substance, and the crystals of the compound deposited from it always acquire a rose or violet tint from some of its impurities. Oil of turpentine likewise dissolves it, but not more abundantly than alcohol.

By many successive solutions in alcohol, I obtained enough of this substance for an analysis, of which the following are the results:—

{	12·302	grains, dried in vacuo, gave
	6·592	... of carbonic acid, and
	3·018	... of water.

8·061 grains, deflagrated with a mixture of nitre and carbonate of soda, gave 7·297 grains of sulphate of baryta = 1·0067 = 12·48 per cent. of sulphur.

The mercury and chlorine were determined together by mixing the substance with quicklime, and introducing the mixture into a combustion tube. The end was then drawn out into an elongated bulb, into which the mercury sublimed, and which was afterwards cut off, dried in the water-bath, and weighed, both with and without the mercury; the chlorine was determined in the usual way from the residue in the tube.

9·958 grains gave 5·976 mercury = 60·01 per cent., and 4·310 grains chloride of silver = 10·67 per cent. of chlorine.

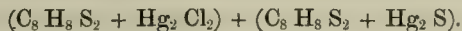
5·797 grains gave 2·409 of chloride of silver = 10·25 per cent. of chlorine.

These results correspond closely with the formula $C_{16} H_{16} S_5 Hg_4 Cl_2$, as is shewn by the following comparisons:—

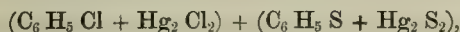
	Experiment.		Calculation.			
	I.	II.				
Carbon . . .	14·61	...	14·46	C_{16}	1200·0	
Hydrogen . . .	2·72	...	2·42	H_{16}	200·0	
Mercury . . .	60·01	...	60·32	Hg_4	5003·6	
Chlorine . . .	10·67	10·25	10·67	Cl_2	885·3	
Sulphur . . .	12·48	...	12·13	S_5	1005·8	
	100·49	...	100·00		8294·7	

It is sufficiently obvious that the formula $C_{16} H_{16} S_5 Hg_4 Cl_2$ cannot be supposed to represent the rational formula of this substance. On the contrary, the remarkable analogy between its properties and those of the mercury compound of sulphuret of allyl appear clearly to indicate a similarity in their chemical constitution,—a similarity which, as we shall afterwards see, is borne out by the properties of the platinum compound. I consider this substance to contain an or-

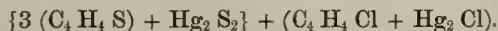
ganic sulphuret, analogous to sulphuret of allyl, the constitution of which must be represented by the formula $C_8 H_8 S_2$, to which I give the provisional name of Sulphuret of Odmyl (from *ὀδμήν odor*), and that the rational formula of the mercury compound is—



On contrasting this with the formula of the allyl compound, which is—



two important points of difference are apparent, namely, that in the new compound we have the sulphuret, and not the chloride, of the base in union with corrosive sublimate, and the presence of subsulphuret in place of sulphuret of mercury in the second member of the compound. It is even possible to approximate more closely the formulæ of the allyl and odmyl compounds, by assuming the sulphuret of odmyl to be represented by $C_4 H_4 S$; in which case, the mercury compound becomes:—



This formula is, however, incompatible with its reactions, as it involves the presence of calomel in the compound. Treatment with caustic potash, however, shews that this is not the case; as it immediately becomes yellow, from the separation of oxide of mercury, while the black suboxide would have been formed had calomel been present.

When a current of sulphuretted hydrogen is passed through the mercury compound suspended in water, it becomes rapidly black, a peculiar smell is observed, along with that of sulphuretted hydrogen, and, by distillation, an oil passes over, which is obtained floating on the surface of the water. It is perfectly transparent and colourless. Its smell is peculiar, and resembles the nauseous odour developed by crushing some umbelliferous plants. When dissolved in alcohol, it gives, with corrosive sublimate, a white precipitate, soluble in hot alcohol, from which it is deposited in crystals precisely similar to those from which it had been originally separated, and, with bichloride of platinum, a yellow precipitate, slightly soluble in hot alcohol and ether. This oil is, in all probability, the sulphuret of odmyl $C_8 H_8 S_2$, but the small quantity in which I have been able to obtain it, has prevented my performing any analysis of it.

The Platinum Compound. When a solution of bichloride of platinum is added to the alcoholic solution of the crude oil, a yellow precipitate makes its appearance, which does not fall immediately, but goes on gradually increasing for some time, precisely as is the case with the allyl compound. The properties of this precipitate are not, however, perfectly constant, but vary according to the portion of the oil employed to yield it. That obtained from the more volatile portion has a fine sulphur-yellow colour, but the less volatile oil gives an orange precipitate. It is

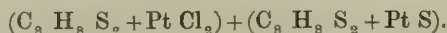
insoluble in water, sparingly soluble in alcohol and ether. When heated it becomes black, an oil is evolved smelling exactly like that obtained from the mercury compound and sulphuret of platinum is left behind, which requires a high temperature to drive off all its sulphur, and leaves metallic platinum as a silver-white mass. When treated with hydrosulphuret of ammonia, it is converted into a brown powder, exactly like that obtained under similar circumstances from allyl.

The analysis of the yellow compound has not hitherto given results of a satisfactory character. I have found the amount of platinum to oscillate between 43·06 and 49·66 per cent. The former of these was obtained from the most volatile oil, the latter from that which boiled between 300° and 400° Fahr., and intermediate results were obtained at intermediate temperatures. The results obtained from the oil which boiled at a high temperature were remarkably constant; thus I have found, in different experiments, 49·00, 49·51 and 49·66 per cent. of platinum, which appear to indicate the presence of some compound of rather sparing volatility. The precipitate obtained from the most volatile oil appears to be that corresponding to the mercury compound which has just been described. Of it I have been able only to perform a very incomplete analysis, which is insufficient to establish its constitution, especially as it is impossible to ascertain whether it is a homogeneous substance. As the results, however, approximate to a formula analogous to that of the mercury compound, I give the details, such as they are.

{ 9·155 grains of the platinum compound gave		
7·474	...	carbonic acid, and
3·294	...	water.

5·701 grains gave 2·455 grains of platinum, = 43·06 per cent.

These results approximate to a formula similar to that of the mercury compound :—viz.



Experiment.		Calculation.		
Carbon,	22·26	20·83	C_{16}	1200·0
Hydrogen,	3·99	3·47	H_{16}	200·0
Platinum,	43·06	42·84	Pt_2	2466·6
Chlorine,	...	15·38	Cl_2	885·3
Sulphur,	...	17·48	S_6	1005·8
...		100·00		5757·7

The analogy which those substances bear to allyl is exceedingly interesting, as shewing the possibility of forming, by artificial processes, substances similar in constitution to so remarkable a compound, which is not a product of decomposition, but exists ready formed in a variety of different vegetables, where it must

obviously be produced under circumstances very different from the artificial substance; for allyl cannot exist at all at a high temperature, and is entirely decomposed at, or even below, its point of ebullition. Unfortunately, however, the examination of this substance is much complicated by the necessity of examining its compounds in place of itself. Had it been possible to separate it directly from the crude oil, the determination of its constitution and that of its compounds would have presented comparatively little difficulty, and been arrived at with much less labour than that expended upon the imperfect details I have been able to accumulate. Another point worthy of observation, is the total alteration of the products of decomposition of oleic acid produced by the presence of sulphur; no sebacic acid, and, in fact, none of its ordinary products being evolved, although all the substances produced contain carbon and hydrogen in the proportion of equal atoms, just as they exist among the ordinary products,—a circumstance which, taking into consideration the abundant evolution of sulphuretted hydrogen, we certainly should not have anticipated.

The oil which remains after the separation of the mercury compound, likewise contains sulphur as one of its constituents; but I have not yet had time to commence the investigation of this part of the subject. The discussion of it, as well as various other points connected with the compounds already described, I hope to make the subject of a future communication.

XXV.—*Experiments on the Ordinary Refraction of Iceland Spar.* By WILLIAM SWAN, Esq. Communicated by Professor KELLAND.

(Read 19th April 1847.)

According to the theory devised by HUYGENS, to explain the phenomenon of double refraction in Iceland spar, a pencil of light transmitted through that substance is divided into two pencils; the index of refraction for the one being constant, while for the other it varies with the inclination of the transmitted light to the optical axis of the crystal.

Dr WOLLASTON, in 1802, verified the spheroidal form of the wave of light, which HUYGENS had assumed to account for the refraction of the extraordinary pencil, by a careful experimental investigation, conducted by means of his elegant instrument for “examining refractive and dispersive powers by prismatic reflection.”* In 1810, MALUS, in his *Théorie de la Double Réfraction*, also demonstrated experimentally the accuracy of the Huygenian law for the extraordinary pencil. I have not had an opportunity of consulting the memoir of MALUS, so as to know the precise nature of his experiments, with reference to the refraction of the ordinary ray; but the object of Dr WOLLASTON’S researches was simply to prove the law of extraordinary refraction, and the constancy of the index of refraction for the ordinary ray, is therefore tacitly assumed by him.

More recently, Professor MACCULLAGH of Dublin, in order to account for certain phenomena observed by Sir DAVID BREWSTER, in the reflexion of light from Iceland spar, was led to propose a law of double refraction, according to which the ordinary ray in that substance has a variable index of refraction; and at his request, Sir DAVID BREWSTER made an experiment to ascertain whether the ordinary refraction of Iceland spar is different at different inclinations to the axis. Two prisms were cut out of the same piece of spar, so that in one the transmitted ray was at right angles to the axis, and in the other, it was coincident with it; and both being cemented to a plate of glass, had their surfaces ground and polished together, so as to ensure the equality of their refracting angles. It was then found that the images of a narrow slit, illuminated by homogeneous yellow light, seen through the prisms, were perfectly coincident, which proved that the index of refraction for the ordinary ray was the same in both prisms, “within the limits of the errors of observation.”†

* Philosophical Transactions, 1802, pp. 365 and 387.

† See Experiment on the ordinary refraction of Iceland spar, by Sir DAVID BREWSTER.—*Notices and Abstracts of Communications of the British Association*, 1843, p. 7.

Some time ago, Mr WILLIAM NICOL of Edinburgh, whose skill in cutting and polishing Iceland spar is well known, requested me to undertake the examination of the ordinary refraction of several prisms of Iceland spar, with which he had the kindness to entrust me, and which he had cut so that the transmitted light is inclined at various angles to the axis. The refractive power of these prisms was examined by means of an instrument devised by me for facilitating such inquiries, and described in the Transactions of the Royal Scottish Society of Arts for 1844, p. 293.* It will be sufficient here to explain that the prism is mounted in front of the telescope of a theodolite, with plates of sextant glass in accurate contact with its faces. The deviation of the refracted rays is then measured as in FRAUNHOFER'S method of determining refractive powers; and the refracting angle of the prism is ascertained by measuring the deviation of rays that have suffered two reflexions at the surfaces of the sextant glasses. The prism being placed in its position of minimum deviation, the index of refraction is ascertained from the formula $\mu = \frac{\sin \frac{1}{2}(\theta + \alpha)}{\sin \frac{1}{2}\theta}$ where θ is the angle of the prism, and α the minimum deviation of the refracted rays.

The theodolite I used in this investigation is made by TROUGHTON. The horizontal limb, measuring 6·5 inches in diameter, is furnished with two verniers reading 20", and the telescope magnifies twelve times. As I had not the means of observing an object at a greater distance than 40 feet, and as the correction for parallax due to the distance of the prism from the centre of the theodolite could not be ascertained with sufficient accuracy, owing to the difficulty of finding the exact position of the pencil of incident rays, I determined to adopt a method for avoiding this correction.

This consisted partly in mounting the prism over the centre of the theodolite by means of a simple and ingenious arrangement suggested by Mr JOHN ADIE. A rod of well-seasoned mahogany, fitted to the Ys of the theodolite, was furnished at one end with temporary Ys, placed so as to shift the telescope out from the centre of the instrument; while, at the other, it carried a counterpoise to the weight of the telescope. To this I added stays of wire passing from the ends of the rod to the extremities of the horizontal axis of the theodolite, which were tightened by means of screws so as to prevent any lateral shake. The whole apparatus was mounted on a very firm portable tripod, and was sufficiently stable.

But although the prism, from its position at the centre of the instrument, did not suffer any material displacement on turning round the telescope, it was still desirable to get rid of any remaining uncertainty as to the direction of the incident light. The method I devised for effecting this object, was to use a collimator so as to obtain a beam of sensibly parallel rays, and thus to place

* Also in the Edinburgh New Philosophical Journal, January 1844.

the luminous object I observed, virtually at an infinite distance. Having fitted a pair of cross fibres of silk* in the anterior focus of the object-glass of a telescope, I carefully adjusted it to distinct vision on a star, so that, on moving the eye up and down, its image remained fixed on the wires. The eye-piece being then cautiously removed, the wires were illuminated by a lamp; and the beam of rays issuing from the object-glass having been directed upon the prism, the optical axis of the collimator was made parallel to the horizontal limb of the theodolite by means of adjusting screws. A common oil-lamp was used in ascertaining the angles of the prisms; but when the deviation of the refracted rays was observed, the wires were illuminated with the homogeneous yellow light of a spirit-lamp with a salted wick: and it must be regarded as a remarkable proof of the perfect homogeneity of this light, that the refracted image of a single fibre of silk was always distinctly visible with a good prism.

I shall now give the results of the examination of Mr NICOL's prisms, to which I shall refer according to the numbers he has attached to them. The angle of the prism was generally determined by four, and the deviation of the refracted rays by six observations.

The prism marked No. 1 is cut out of the crystal, so that in the position of minimum deviation the transmitted rays are parallel to the axis; and Mr NICOL has worked with such accuracy, that the images produced by the ordinary and extraordinary rays coincide almost exactly in this position. The angle of this prism was found to be $60^{\circ} 8' 8''$, the deviation of the refracted rays $52^{\circ} 14' 36''$, and consequently $\mu = 1.658367$.†

The plane of refraction in the prism No. 2 is perpendicular to the axis. Its angle was found to be $44^{\circ} 29' 20''$, and the deviation of the refracted rays $33^{\circ} 17' 8''$. From which $\mu = 1.658366$.

Two other prisms, No. 3 and No. 4, were also examined, in which the transmitted rays are perpendicular to the axis; but in either case the prism is cut so that the plane of refraction differs from that of No. 2.

For No. 3, the angle of the prism was found to be $59^{\circ} 36' 32''$, the deviation of the refracted rays $51^{\circ} 25' 25''$, and $\mu = 1.658384$.

For No. 4, the angle of the prism was found to be $44^{\circ} 55' 24''$, the deviation $33^{\circ} 42' 34''$, and $\mu = 1.658361$.

In No. 5, the transmitted rays are inclined 45° to the axis; the refracting angle of the prism was found to be $45^{\circ} 3' 51''$, the deviation of the refracted rays $33^{\circ} 50' 58''$, and $\mu = 1.658385$.

* Silk is not the most suitable material for the purpose, owing to its transparency; but I could procure no better at the time.

† I have also examined another prism, No. 1, and have found $\theta = 44^{\circ} 23' 2''$, $\delta = 33^{\circ} 11' 0''$, and $\mu = 1.658362$.

In No. 6, one of the faces is a cleavage plane, and the principal section of the prism is in the same plane with the axis. Therefore, since the cleavage plane is inclined $45^{\circ} 23' 25''$ to the axis, it follows that the inclination of the transmitted rays in the position of minimum deviation is nearly $66^{\circ} 51'$. The angle of this prism was found to be $44^{\circ} 28' 29''$, the deviation of the refracted rays $33^{\circ} 16' 22''$, and $\mu = 1.658389$.

These results are combined in the following Table :—

Prism.	Inclination of the plane of incidence, or of the principal section of the prism to the optical axis of the crystal.*	Inclination of the transmitted rays to the optical axis.	Index of refraction for the ordinary ray (μ).	Difference of each result from the mean value of μ .
No. 1.	0°	0°	1.658367	—0.000008
2.	90°	90°	1.658366	—0.000009
3.	0°	90°	1.658384	+0.000009
4.	45°	90°	1.658361	—0.000014
5.	0°	45°	1.658385	+0.000010
6.	0°	$66^{\circ} 51'$	1.658389	+0.000014
Mean			1.658375	0.000011

From this summary it will be seen, that the greatest difference between the observed index of refraction of any prism and the mean of the whole results is only .000014; while the difference of the greatest and least results is less than .000003. So close an agreement in six essentially different cases, seems to render it very probable that the index of refraction is really constant; and the result of the investigation thus confirms the accuracy of the Huygenian law.

* As the term, principal section, is employed in more than one sense, it may be proper to observe, in order to avoid ambiguity, that I use it to denote a plane perpendicular to both faces of the prism.— See Sir John Herschel's *Treatise on Light*, in the *Encyclopædia Metropolitana*, p. 370, art. 197.

XXVI.—*Observations on the Temperature of the Ground at Trevandrum, in India, from May 1842 to December 1845.* By JOHN CALDECOTT, Esq., Astronomer to the Rajah of Travancore. Communicated in a Letter to Professor J. D. FORBES.

(Read 15th March 1847.)

122 Pall Mall, Feb. 3, 1847.

MY DEAR SIR,—I have taken the opportunity of a short visit to England to bring with me a complete copy, to the end of 1845, of the Observations of Terrestrial Temperature, made at Trevandrum (of which I have already sent you a partial account), and which I have now the pleasure to forward to you herewith, to be dealt with in any way you may think proper. Should you think them worthy of presentation to the Royal Society of Edinburgh for publication in their Transactions, it will be gratifying to me. The observations made with the 12 feet thermometer are now at an end, in consequence of the fracture of the instrument during a high wind last year; but, after rejecting those made immediately on the introduction of the instruments, say all those of 1842, there will still remain three entire years of trustworthy readings, which I hope may be found to be sufficient for any purpose for which such observations in a tropical region may have been considered desirable.

JNO. CALDECOTT.

Professor FORBES, Edinburgh.

EXPLANATION.

These three thermometers, made by Messrs ADIE and SON of Edinburgh, were put down on May 1, 1842. They are buried at the respective depths of 12 French feet, 6 French feet, and 3 French feet, on *the top* of the Observatory hill, which is composed of the stone called “laterite.” The situation is about 200 feet above the sea.

In December 1841 and January 1842, they were suspended in the Observatory, and compared with a standard thermometer by Traughton and Simms, as follows, viz.:—

No. 1, or the 12 feet thermometer, by a comparison made 12 times in 24 hours for 29 days, *i. e.*, by 348 comparisons, requires, in order to reduce it to the standard thermometer, the addition to the readings here given of $2^{\circ}133$. The greatest deviation from this mean of any single comparison is $0^{\circ}54$.

No. 2, or the 6 feet thermometer, by the same number of comparisons, requires the addition of $2^{\circ}172$; the greatest deviation from which is $0^{\circ}69$.

No. 3, or the 3 feet thermometer, by the same number of comparisons, requires the addition of $2^{\circ}922$; the greatest deviation from which is $0^{\circ}57$.

Latitude of Trevandrum Observatory,	. . .	$8^{\circ} 30' 32''$ N.
Longitude from Greenwich,	. . .	$5^h 7^m 59^s$ E.

[It may here be stated, that the original registers furnished by Mr CALDECOTT include four observations, daily (except on Sundays), at 6 A.M., Noon, 6 P.M., and Midnight. The extent of the Tables is, consequently, very considerable; and, in printing them, it was necessary to ascertain whether these six-hourly observations possessed a value proportional to the great additional space which they would have occupied. A careful examination shewed that the variation from one hour to another, in the different thermometers, was due *mainly* to the influence of the heat of the air in expanding the *exposed* part of the column of spirit, and did not at all indicate the course of the *diurnal* curves at different depths; nor were there data given for eliminating this discrepancy. It was, therefore, decided to publish the 6 A.M. observations alone,* as representing fairly the general result, and as being less liable to error from the irregular expansion of the exposed column of spirit by the intense heat of the tropical sun.

It will be observed that all the temperatures in the Register, and the means, are UNCORRECTED for the large index error of the instruments. A *corrected* summary will be given, together with a few deductions, at the conclusion of the paper.—J. D. F.]

* The monthly means, four times a day, are, however, given in a separate Table.

When blanks occur, the liquid is above the Scale.

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain.	
1842.	°	°	°	°	in.		1842.	°	°	°	°	in.	
May 1	The Thermometers were put down this day.					0.1852	July 1	84.90	84.21	81.62	76.35	
2	83.80	84.02	83.40	82.58	0.0168		2	85.00	84.21	81.62	78.32	
3	83.91	84.16	83.54	84.40		3						Sunday
4	84.03	84.37	83.78	84.00		4	81.90	84.10	81.29	78.60	
5	84.11	84.54	83.89	79.71	0.1095		5	84.92	84.07	81.21	80.07	
6	84.22	84.74	84.10	79.79	0.3915		6	84.90	84.01	81.18	78.48	0.3409	
7	81.24	84.83	84.17	75.97	3.8596		7	84.88	83.89	81.00	75.28	0.7702	
8					0.9639	Sunday	8	84.88	83.82	81.20	76.60	
9	84.90	84.82	83.85	77.33	0.6902		9	84.88	83.80	81.20	78.18	
10	84.44	85.21	83.55	78.41	0.4168		10						Sunday
11	84.54	85.32	83.40	81.05		11	84.82	83.71	81.05	78.04	
12	84.57	85.32	83.20	81.77		12	84.04	83.70	80.94	79.57	
13	84.67	85.35	83.18	81.22		13	84.83	83.68	80.88	80.45	0.0042	
14	84.70	85.33	83.15	81.50		14	84.82	83.60	80.70	80.21	
15						Sunday	15	84.80	83.57	80.79	76.81	0.4629	
16	84.90	85.28	83.11	81.24		16	84.76	83.49	80.65	74.37	1.6331	
17	84.76	85.18	83.19	79.57		17				0.2947		Sunday
18	84.84	85.21	83.38	80.73	0.084		18	84.72	83.40	80.88	78.16	0.2063	
19	84.78	85.17	83.50	82.10		19	84.74	83.41	80.70	78.77	0.0463	
20	84.90	85.23	83.70	82.69	0.0379		20	84.80	83.40	80.74	79.85	
21	84.88	85.21	83.80	82.87		21	84.78	83.42	80.79	80.44	
22						Sunday	22	84.67	83.31	80.70	80.42	0.0463	
23	84.98	85.27	84.22	80.51		23	84.66	83.30	80.72	80.25	
24	84.99	85.29	84.43	81.13	0.0505		24						Sunday
25	81.88	85.34	84.52	82.58		25	84.60	83.22	80.80	80.49	
26	85.02	85.48	84.70	77.44	0.3620		26	84.60	83.20	80.86	79.97	0.7197	
27	85.00	85.42	84.59	76.08	2.0667		27	84.55	83.17	80.90	78.03	0.5262	
28	85.20	85.54	84.70	75.00	0.5393		28	84.55	83.18	80.88	79.77	0.1179	
29					1.6878	Sunday	29	84.51	83.16	81.00	79.33	0.0084	
30	85.08	85.61	83.66	74.87	2.7737		30	84.52	83.18	81.02	79.50	0.7745	
31	84.88	85.60	83.20	78.89	0.2736		31						Sunday
Means	84.66	85.11	83.77	80.09	14.5134		Means	84.76	83.62	80.97	78.73	5.9516	
June 1	85.02	85.60	82.75	79.58	0.0758		Aug. 1	84.44	83.15	80.96	77.66	0.2652	
2	84.88	85.49	82.50	80.30	0.0210		2	84.45	83.17	80.95	78.33	0.0842	
3	85.14	85.41	82.34	79.18	0.4671		3	84.43	83.12	80.85	77.11	0.0589	
4	85.12	85.30	82.30	77.45	0.8712		4	84.42	83.16	80.91	78.09	0.0547	
5					0.4420	Sunday	5	84.38	83.11	80.71	78.93	0.0337	
6	85.01	81.63	76.34	4.1502		6	84.36	83.05	80.52	77.37	0.0295	
7	84.80	81.64	78.49	0.3956		7				0.0463		Sunday
8	84.69	81.30	80.53	0.0168		8	84.39	83.04	80.59	77.49	0.2947	
9	84.60	81.42	80.80		9	84.33	83.00	80.55	78.98	
10	84.55	81.43	80.97		10	84.30	82.98	80.45	79.57	0.0421	
11	84.43	81.29	80.89		11	84.31	82.92	80.41	78.81	0.4462	
12						Sunday	12	84.30	82.91	80.35	79.24	
13	84.41	81.66	80.49		13	84.23	82.85	80.39	73.91	0.4840	
14	84.30	81.60	80.60	0.3072		14				0.1979		Sunday
15	84.30	81.83	80.91		15	84.23	82.80	80.39	76.07	0.1726	
16	84.30	81.90	81.37		16	84.22	82.80	80.30	74.01	1.0691	
17	84.30	82.10	81.34	0.1136		17	84.18	82.79	80.10	74.84	0.0379	
18	84.33	82.17	81.34	0.0253		18	84.14	82.65	79.89	75.12	0.3031	
19						Sunday	19	84.13	82.67	79.80	75.30	0.2021	
20	84.29	82.20	80.23	0.0982		20	84.15	82.61	79.50	77.42	
21	84.30	82.38	78.42	0.8840		21						Sunday
22	85.10	84.30	82.49	80.33	0.0243		22	84.16	82.59	79.50	78.67	0.0168	
23	84.30	82.50	78.47	0.0274		23	84.10	82.50	79.30	79.16	0.1347	
24	85.00	84.34	82.50	77.17	0.8250		24	84.11	82.25	79.29	79.42	0.0084	
25	85.00	84.35	82.21	77.56		25	84.08	82.39	79.57	80.17	
26						Sunday	26	84.08	82.38	79.70	80.24	
27	84.98	84.35	82.10	79.23		27	84.08	82.30	79.70	80.17	0.0210	
28	84.94	84.32	81.95	79.45		28				0.3872		Sunday
29	84.91	84.30	81.90	75.44	0.9890		29	84.10	82.27	80.05	80.17	0.0253	
30	81.96	84.30	81.87	75.02	0.0136		30	84.19	82.21	80.00	78.61	0.0084	
							31	83.90	82.25	80.21	78.41	
Means	84.58	82.00	79.32	8.7473		Means	84.23	82.74	80.18	77.90	4.4240	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain.	
1842.	°	°	°	°	in.		1842.	°	°	°	°	in.	
Sept. 1	83-95	82-30	80-44	77-76	0-0168		Nov. 1	83-68	83-49	81-61	76-11	0-0716	
2	83-96	82-31	80-30	79-34		2	83-67	83-42	81-41	77-78	0-6524	
3	83-92	82-35	80-51	79-07		3	83-66	83-40	81-23	77-64	0-0253	
4					Sunday	4	83-67	83-38	81-12	79-01	
5	83-89	82-35	80-45	79-39	0-0758		5	83-70	83-32	80-98	78-46	
6	83-82	82-40	80-60	78-71		6					1-0354	Sunday
7	83-84	82-47	80-76	79-35		7	83-70	83-21	80-93	76-49	0-6397	
8	83-82	82-49	80-75	80-23		8	83-69	83-10	80-59	76-57	0-3199	
9	83-80	82-51	81-02	80-45		9	83-67	83-06	80-70	75-28	0-1684	
10	83-74	82-55	81-10	80-18		10	83-69	83-01	80-62	75-42	0-8165	
11					Sunday	11	83-72	82-97	80-42	75-86	0-6272	
12	83-75	82-64	81-45	79-19		12	83-70	82-93	80-32	76-50	0-2778	
13	83-77	82-73	81-64	79-45	0-0042		13					1-3132	Sunday
14	83-78	82-80	81-72	79-18	0-0168		14	83-70	82-75	79-91	77-27	0-8165	
15	83-76	82-87	81-88	78-67		15	83-66	82-61	79-70	78-05	0-0716	
16	83-72	82-92	81-96	79-02		16	83-68	82-54	79-63	78-94	0-1179	
17	83-74	82-99	82-00	78-93	0-3114		17	83-66	82-45	79-50	78-41	1-4269	
18					Sunday	18	83-62	82-38	79-58	78-10	0-0126	
19	83-72	83-10	82-10	76-52	0-2063		19	83-64	82-30	79-52	79-22	
20	83-72	83-18	82-10	76-66	0-5261		20					Sunday
21	83-71	83-21	82-10	76-12	0-5262		21	83-54	82-19	79-79	79-25	
22	83-71	83-37	82-00	75-46	0-1010		22	83-56	82-17	79-90	79-32	
23	83-69	83-27	81-85	75-77	1-8604		23	83-58	82-19	79-85	80-06	
24	83-73	83-32	81-74	76-56	1-1744		24	83-58	82-20	80-09	78-62	0-3956	
25					0-9555	Sunday	25	83-51	82-11	79-91	78-32	
26	83-72	83-30	81-28	76-67	0-2105		26	83-52	82-19	80-25	78-54	0-0168	
27	83-72	83-26	81-11	76-37	1-3258		27					Sunday
28	83-74	83-25	80-85	77-87	0-3157		28	83-49	82-20	80-39	79-32	
29	83-71	83-12	80-78	78-82	0-0969		29	83-76	82-21	80-35	78-05	
30	83-73	83-10	80-70	79-46		30	83-48	82-27	80-48	76-68	
Means	83-75	82-85	81-28	78-28	7-7238		Means	83-62	82-69	80-37	77-82	8-8053	
Oct. 1	83-72	83-00	80-55	77-84		Dec. 1	83-29	82-19	80-35	76-79	
2					Sunday	2	83-43	82-29	80-52	78-01	
3	83-74	82-91	80-50	79-05	0-0379		3	83-41	82-31	80-55	78-32	
4	83-75	82-81	80-49	79-52		4					Sunday
5	83-71	82-81	80-50	79-66		5	83-36	82-30	80-45	76-38	
6	83-73	82-80	80-51	79-94		6	83-41	82-38	80-66	77-30	
7	83-72	82-79	80-50	80-29		7	83-39	82-37	80-67	78-87	
8	83-73	82-75	80-64	80-69		8	83-38	82-40	80-65	77-86	
9					Sunday	9	83-34	82-43	80-83	78-36	0-0674	
10	83-69	82-69	80-78	80-02		10	83-36	82-45	80-79	79-17	
11	83-72	82-75	80-85	80-07		11					Sunday
12	83-75	82-70	80-99	79-75		12	83-36	82-57	81-15	80-20	0-0295	
13	83-70	82-72	81-09	80-32		13	83-35	82-59	81-19	80-21	0-0631	
14	83-68	82-75	81-25	80-29		14	83-34	82-61	81-25	79-29	
15	83-65	82-72	81-35	80-44		15	83-34	82-70	81-44	79-97	
16					Sunday	16	83-32	82-70	81-40	80-08	0-0042	
17	83-64	82-87	81-70	79-22	0-2273		17	83-32	82-76	81-63	79-63	
18	83-65	82-93	81-90	80-19		18					Sunday
19	83-64	83-02	82-05	80-04	0-0337		19	83-29	82-80	81-70	78-65	
20	83-63	83-07	82-20	79-48	0-1894		20	83-26	82-90	81-96	78-32	
21	83-65	83-10	82-09	78-69	0-1979		21	83-30	82-91	81-90	78-91	
22	83-61	83-19	82-31	79-01		22	83-32	83-05	82-15	79-07	
23					0-0126	Sunday	23	83-32	83-11	82-28	78-60	
24	83-65	83-36	82-39	79-63		24	83-31	83-15	82-15	79-14	
25	83-64	83-40	82-30	77-69	0-2021		25					Sunday
26	83-63	83-42	82-30	78-97		26	83-27	83-19	82-25	80-02	
27	83-63	83-41	82-23	77-45	0-6776		27	83-35	83-35	82-55	79-91	
28	83-64	83-48	82-14	75-47	2-4075		28	83-36	83-40	82-60	79-68	
29	83-65	83-54	82-06	78-50	0-0631		29	83-40	83-51	82-78	81-07	
30					0-1473	Sunday	30	83-35	83-41	82-59	
31	83-68	83-50	81-70	74-50	1-2964		31	83-34	83-49	82-70	
Means	83-68	83-04	81-44	79-10	5-4928		Means	83-34	82-89	81-52	78-96	0-1642	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 6 P.M. to 6 P.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 6 P.M. to 6 P.M.	
1813. Jan. 1	°	°	°	°	in.	Sunday	1843. March 1	°	°	°	°	in.	
2	83-35	83-52	82-68	79-77	0-090		2	84-11	85-43	85-42	82-05	
3	83-42	83-80	83-15	78-59		3	84-12	85-52	85-49	82-01	
4	83-47	83-87	83-29	79-06		4	84-12	85-58	85-67	82-32	
5	83-41	83-77	83-05	79-30		5	84-18	85-51	85-50	82-97	
6	83-47	83-96	83-31	79-98		6	84-19	85-74	86-09	82-33	Sunday
7	83-45	83-99	83-00	76-52		7	84-23	85-85	86-24	81-94	
8				0-037	Sunday	8	84-22	85-90	86-29	82-79	0-082	
9	83-52	84-10	83-15	76-55	0-033		9	84-21	85-98	86-42	82-41	
10	83-56	84-12	83-17	75-79	0-183		10	84-27	86-02	86-48	80-49	0-186	
11	83-51	84-01	82-79	77-43		11	84-29	86-12	86-61	80-55	0-558	
12	83-46	84-17	83-11	79-53		12					0-803	Sunday
13	83-60	84-18	82-78	79-60		13	84-44	86-29	86-73	81-03	
14	83-60	84-15	82-50	80-27		14	84-38	86-28	86-61	81-01	
15						Sunday	15	84-40	86-41	86-50	81-56	
16	83-46	83-99	83-00	78-25		16	84-39	86-31	86-10	82-10	
17	83-61	84-01	82-29	78-61	0-465		17	84-46	86-48	86-22	81-15	
18	83-66	84-05	82-50	79-66	0-287		18	84-45	86-49	86-17	82-46	
19	83-67	84-02	82-51	80-31		19						Sunday
20	83-64	83-26	82-76	70-31		20	84-52	86-50	86-22	84-15	
21	83-70	84-00	82-51	79-91	0-059		21	84-58	86-54	86-33	82-83	
22						Sunday	22	84-53	86-51	86-37	83-95	
23	83-70	83-90	82-21	89-91		23	84-62	86-55	86-58	83-60	
24	83-77	83-96	82-50	80-17		24	84-65	86-62	86-74	82-72	
25	83-75	83-95	82-50	80-90		25	84-68	86-63	86-82	82-44	0-092	
26	83-74	83-92	82-54	79-67		26						Sunday
27	83-78	83-93	82-52	77-37		27	84-72	86-73	87-08	83-18	
28	83-75	83-91	82-37	77-36		28	84-72	86-81	87-01	83-03	
29						Sunday	29	84-76	86-74	87-11	83-58	
30	83-76	83-92	82-71	78-90		30	84-76	86-90	87-14	83-53	
31	83-75	83-91	82-55	79-92		31	84-80	86-92	87-18	82-36	
Means	83-61	83-94	82-75	79-05	1-154		Means	84-43	86-27	86-41	82-36	1-721	
Feb. 1	83-76	83-94	82-80	79-60		April 1	84-82	86-79	87-25	78-60	0-420	
2	83-81	84-02	83-07	80-29		2					0-122	Sunday
3	83-79	83-99	82-85	79-53		3	84-82	87-38	78-86	0-007	
4	83-82	84-08	83-20	81-92	0-001		4	84-88		87-35	80-16	
5						Sunday	5	84-92		87-16	79-71	
6	83-84	84-18	83-15	79-29	0-005		6	84-95		86-94	82-30	0-071	
7	83-80	84-18	83-54	79-27	0-027		7	84-92		86-60	83-72	
8	83-84	84-23	83-38	79-56		8	84-90		86-35	83-95	
9	83-29	84-19	83-25	80-21		9						Sunday
10	83-36	84-20	83-36	81-21		10	85-00	86-29	84-45	
11	83-85	84-37	83-49	81-46		11	84-90	86-35	84-01	
12						Sunday	12	85-10	86-47	84-29	
13	83-84	84-46	83-56	81-11		13	85-00	86-49	82-69	
14	83-90	84-47	83-82	80-52		14	86-60	81-45	0-227	
15	83-85	84-42	83-89	79-56		15	86-75	82-51	0-012	
16	83-91	84-53	84-18	79-24		16					1-476	Sunday
17	83-90	84-60	84-33	79-53		17	86-81	80-20	0-042	
18	83-92	84-68	84-45	79-24		18	86-69	77-46	
19						Sunday	19	86-60	78-36	1-180	
20	83-94	84-81	84-70	79-44		20	86-38	77-46	1-544	
21	83-84	84-80	84-70	79-41		21	86-10	81-49	1-411	
22	83-95	84-92	84-86	79-64		22	85-71	76-36	1-099	
23	84-02	84-98	84-96	80-27		23					0-134	Sunday
24	84-00	85-09	85-00	79-91		24	85-05	82-57	0-228	
25	83-99	85-10	85-00	79-51		25	84-84	81-50	
26						Sunday	26	84-65	80-55	1-437	
27	84-05	85-26	85-23	81-13		27	86-61	84-51	82-53	0-001	
28	84-08	85-24	85-32	81-23		28	86-52	84-49	83-72	0-163	
							29	86-50	84-50	84-09	
							30						Sunday
Means	83-85	84-53	84-00	80-09	0-033		Means	84-92	86-60	86-17	81-58	9-274	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 6 P.M. to 6 P.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 6 P.M. to 6 P.M.	
1843.	°	°	°	°	in.		1843.	°	°	°	°	in.	
May 1	86-30	84-50	84-60		July 1	84-40	82-51	79-53	75-30	0-177	
2	86-27	84-58	83-04	0-007		2	0-654	Sunday
3	86-18	84-63	85-39	0-057		3	84-34	82-37	79-32	74-34	0-573	
4	86-20	84-76	84-82		4	84-31	82-31	79-28	76-16	1-144	
5	86-10	84-80	83-75		5	84-29	82-25	79-12	76-50	0-298	
6	86-12	84-86	85-57		6	84-25	82-17	78-89	77-54	0-048	
7	Sunday	7	84-22	82-15	79-05	76-31	
8	86-10	85-17	80-34	0-204		8	84-15	82-05	78-90	74-33	1-411	
9	86-12	85-30	84-33		9	1-301	Sunday
10	86-12	85-38	83-57	0-014		10	84-11	81-88	78-79	75-31	0-965	
11	86-15	85-37	80-66	0-172		11	84-09	81-85	78-77	76-60	0-338	
12	86-17	85-38	83-60		12	84-01	81-71	78-60	77-65	0-241	
13	86-25	85-55	81-80		13	83-95	81-61	78-50	75-30	0-598	
14	Sunday	14	83-91	81-55	78-50	76-42	0-787	
15	86-32	85-44	81-37	0-299		15	83-92	81-55	78-54	75-92	0-342	
16	86-26	85-38	78-46	0-122		16	0-270	Sunday
17	86-21	85-40	78-24	0-563		17	83-81	81-41	78-49	78-58	
18	86-35	85-30	81-46		18	83-82	81-46	78-61	78-44	
19	86-30	85-25	80-32		19	83-80	81-47	78-67	78-19	
20	86-28	84-99	80-97		20	83-71	81-40	78-65	77-01	0-166	
21	2-407	Sunday	21	83-74	81-40	78-25	78-41	0-282	
22	86-27	84-50	76-99	2-267		22	83-79	81-40	78-58	78-62	0-192	
23	86-16	83-98	77-25	1-647		23	0-080	Sunday
24	86-12	83-45	78-81	1-471		24	83-62	81-38	78-86	78-95	
25	85-92	82-81	79-97		25	83-60	81-40	78-95	78-64	0-041	
26	85-78	82-52	76-89	0-261		26	83-59	81-38	78-85	78-16	
27	85-62	82-19	77-90	2-343		27	83-55	81-39	78-90	78-08	0-062	
28	0-902	Sunday	28	83-52	81-35	78-89	79-33	
29	85-25	81-17	76-50	1-188		29	83-51	81-40	78-95	79-57	
30	85-07	81-41	77-23	1-320		30	0-465	Sunday
31	84-94	81-22	78-97	0-745		31	83-45	81-35	79-00	79-30	0-464	
Means	86-03	84-26	80-62	15-989		Means	83-90	81-70	78-82	77-29	10-899	
June 1	84-73	81-05	81-35	0-432		Aug. 1	83-41	81-35	78-99	79-05	
2	84-58	80-90	80-16	0-065		2	83-39	81-31	78-99	79-55	
3	84-45	81-01	79-23	0-196		3	83-39	81-37	79-05	80-00	
4	0-042	Sunday	4	83-44	81-38	79-07	79-91	
5	84-25	81-10	79-22	0-211		5	83-43	81-35	79-10	78-70	
6	84-17	81-15	78-74	0-451		6	0-003	Sunday
7	84-13	81-30	77-62	0-128		7	83-24	81-35	79-11	79-28	
8	84-09	81-25	79-18	0-560		8	83-27	81-42	79-30	79-49	
9	84-04	81-35	79-60		9	83-28	81-41	79-29	79-72	
10	83-99	81-30	77-55	0-178		10	83-23	81-44	79-31	78-62	
11	Sunday	11	83-30	81-40	79-29	78-79	
12	83-89	81-21	78-60	0-162		12	83-18	81-48	79-38	79-41	
13	85-00	83-88	81-24	78-60	0-240		13	Sunday
14	85-00	83-81	81-00	79-60	0-454		14	83-19	81-50	79-53	79-53	
15	84-96	83-77	81-16	76-36	2-322		15	83-17	81-61	79-60	79-69	
16	84-90	83-65	80-85	74-97	1-941		16	83-13	81-51	79-70	79-85	
17	84-89	83-54	80-65	75-55	1-263		17	83-13	81-59	79-79	79-37	
18	1-768	Sunday	18	83-10	81-59	79-90	77-11	0-108	
19	84-81	83-26	81-14	79-39	0-020		19	83-07	81-60	80-04	76-23	0-505	
20	84-85	83-28	81-17	78-31		20	0-089	Sunday
21	84-75	83-08	79-85	78-05	0-147		21	83-06	81-60	80-02	77-31	0-229	
22	84-69	83-02	80-03	78-89	0-275		22	83-05	81-70	80-22	78-05	0-101	
23	84-65	82-98	79-91	77-26	0-230		23	83-00	81-73	80-20	78-19	0-541	
24	84-84	82-93	79-99	75-56	1-168		24	83-04	81-81	80-21	79-85	0-009	
25	Sunday	25	83-04	81-88	80-24	80-55	
26	84-59	82-87	79-90	79-17	0-858		26	83-00	81-89	79-90	80-66	
27	84-56	82-90	80-05	78-60	0-038		27	Sunday
28	84-54	82-84	80-02	76-61	1-767		28	83-01	81-88	80-12	79-82	
29	84-48	82-62	79-60	76-72	1-780		29	82-95	81-85	80-13	79-09	0-029	
30	84-44	82-54	79-55	76-50	0-236		30	82-95	81-85	80-09	77-67	0-084	
							31	83-00	81-90	80-21	78-87	0-400	
Means	84-75	83-59	80-68	78-21	16-932		Means	83-16	81-58	79-66	79-05	2-098	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.	
1843.	°	°	°	°	in.		1843.	°	°	°	°	in.	
Sept. 1	82-99	81-91	80-31	77-79	0-800		Nov. 1	82-95	82-48	81-00	80-72	
2	82-98	81-91	80-31	76-48	1-872		2	83-10	82-90	81-06	80-72	0-010	
3					0-565	Sunday	3	82-99	82-56	81-10	79-62	
4	82-95	81-95	80-20	79-80	0-093		4	82-95	82-51	80-99	81-42	0-566	
5	82-92	81-99	80-10	79-10		5						Sunday
6	82-98	81-98	80-09	79-09		6	82-96	82-52	81-16	79-30	0-343	
7	82-95	81-91	79-81	79-17		7	82-95	82-50	81-05	79-92	0-059	
8	82-91	81-88	79-80	79-58		8	82-92	82-52	81-12	81-05	
9	82-91	81-85	79-65	79-64		9	82-98	82-60	81-25	81-52	0-352	
10						Sunday	10	82-96	82-57	81-25	80-40	0-256	
11	82-91	81-81	79-61	79-60		11	82-96	82-60	81-32	79-99	0-107	
12	82-94	81-80	79-70	80-22		12						Sunday
13	82-91	81-79	79-70	79-83		13	82-98	82-50	81-39	81-01	
14	82-92	81-75	79-92	80-18		14	82-91	82-60	81-40	78-02	0-042	
15	82-87	81-70	79-85	80-40		15	82-99	82-68	81-46	77-99	
16	82-90	81-75	79-98	80-12		16	82-98	82-68	81-45	80-19	
17						Sunday	17	82-95	82-68	81-49	80-19	
18	82-90	81-81	80-30	79-74		18	82-91	82-67	81-30	79-45	
19	82-91	81-80	80-27	79-13		19						Sunday
20	82-86	81-85	80-56	79-81		20	82-90	82-65	81-18	79-39	
21	82-85	81-88	80-61	80-06		21	82-95	82-74	81-21	79-80	
22	82-85	81-92	80-88	80-14		22	82-95	82-69	81-52	80-22	
23	82-88	82-03	80-90	80-79		23	82-96	82-70	81-20	79-75	0-078	
24						Sunday	24	82-90	82-65	80-93	79-38	
25	82-80	81-97	81-02	80-44		25	82-95	82-68	81-00	78-51	
26	82-85	82-19	81-23	80-37		26						Sunday
27	82-81	82-22	81-30	79-94		27	82-95	82-67	81-07	79-40	
28	82-81	82-28	81-47	79-45		28	83-01	82-75	81-15	79-38	
29	82-85	82-40	81-64	79-95		29	82-95	82-63	81-01	79-77	
30	82-84	82-40	81-67	79-01		30	82-90	82-60	80-95	77-99	
Means	82-90	81-95	80-46	79-54	3-330		Means	82-96	82-63	81-19	79-72	1-813	
Oct. 1					0-616	Sunday	Dec. 1	82-92	82-61	80-99	76-80	
2	82-82	82-87	81-88	78-23		2	82-94	82-60	81-10	78-42	
3	82-81	82-62	81-81	78-39	0-297		3					1-468	Sunday
4	82-82	82-68	81-87	78-05	0-046		4	83-02	82-86	81-25	76-70	3-200	
5	82-90	82-82	81-90	77-51	0-266		5	82-97	82-70	81-15	76-90	3-880	
6	82-89	82-81	81-86	77-32	0-335		6	82-98	82-71	81-10	75-90	2-480	
7	82-88	82-84	81-61	78-22		7	82-98	82-62	80-20	77-78	
8						Sunday	8	82-95	82-58	80-12	79-07	0-212	
9	82-85	82-82	81-27	79-22		9	82-97	82-68	80-13	78-79	0-688	
10	82-91	82-88	81-50	77-19	1-239		10						Sunday
11	82-94	82-87	81-22	76-36	1-689		11	82-91	82-32	79-72	77-50	
12	82-89	82-80	81-06	78-00		12	82-94	82-28	79-76	77-27	1-435	
13	82-91	82-82	81-01	78-48		13	82-90	82-10	79-80	78-50	0-037	
14	82-90	82-71	80-75	79-13		14	82-94	82-16	79-82	78-35	
15					0-092	Sunday	15	82-94	82-10	79-85	76-06	
16	82-91	82-68	80-50	78-89		16	82-86	82-02	79-92	74-20	
17	82-98	82-66	80-61	79-78		17						Sunday
18	82-95	82-61	80-60	80-30	0-012		18	82-81	81-95	79-92	78-81	
19	82-95	82-56	80-64	80-37	0-019		19	82-88	82-00	79-98	76-50	
20	82-85	82-48	80-55	79-44		20	82-86	81-98	79-96	78-50	
21	82-90	82-48	80-61	79-42	2-290		21	82-92	82-05	79-85	79-57	
22					0-150	Sunday	22	82-86	82-02	79-89	80-00	
23	83-00	82-49	80-80	76-92	0-135		23	82-82	82-08	79-95	77-08	
24	82-99	82-49	80-81	79-31	0-046		24						Sunday
25	83-00	82-46	80-89	80-59		25	82-84	81-92	79-85	75-12	
26	82-98	82-42	80-85	81-00		26	82-89	82-02	79-93	77-57	
27	82-95	82-45	80-80	79-10	0-420		27	82-80	81-92	80-00	79-70	
28	83-00	82-50	80-95	78-39	0-598		28	82-80	81-95	80-00	79-32	
29					0-051	Sunday	29	82-82	82-00	80-00	78-70	
30	83-10	82-50	80-91	80-14		30	82-80	82-00	80-10	78-30	
31	82-96	82-49	81-08	80-54	0-531		31						Sunday
Means	82-95	82-65	81-09	78-86	8-830		Means	82-90	82-24	80-17	77-69	13-400	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.	
1844.	°	°	°	°	in.		1844.	°	°	°	°	in.	
Jan. 1	82-60	82-06	80-10	77-25		March 1	83-45	84-90	84-38	81-28	
2	82-75	82-01	80-32	76-53		2	83-50	84-92	84-50	82-50	
3	82-71	82-00	80-45	76-54		3						Sunday
4	82-75	82-08	80-45	76-69		4	83-54	85-02	84-60	82-33	
5	82-75	82-10	80-53	77-73		5	83-51	85-00	84-58	83-11	
6	82-82	82-06	80-57	79-61		6	83-61	85-22	84-90	83-51	
7						Sunday	7	83-64	85-20	85-12	83-68	
8	82-75	82-24	81-71	80-09	0-360		8	83-66	85-25	85-16	83-52	
9	82-80	82-00	80-42	78-81		9	83-65	85-28	85-30	82-96	
10	82-75	82-31	80-90	77-59		10						Sunday
11	82-75	82-34	81-04	78-42		11	83-70	85-42	85-65	82-48	
12	82-78	82-38	81-15	77-43		12	83-75	85-52	85-80	82-51	
13	82-73	82-40	81-12	77-84		13	83-75	85-57	85-88	81-92	
14						Sunday	14	83-78	85-60	86-00	82-70	
15	82-75	82-54	81-22	79-36		15	83-84	85-72	86-18	83-85	
16	82-77	82-60	81-30	78-90	0-078		16	83-84	85-82	86-28	84-55	
17	82-78	82-65	81-30	78-85		17						Sunday
18	82-78	82-60	81-20	78-70		18	83-90	85-97	86-50	84-45	
19	82-79	82-70	81-39	77-92		19	83-92	86-05	86-60	84-41	
20	82-75	82-69	81-38	78-95		20	83-95	86-16	86-73	84-55	
21						Sunday	21	83-92	86-24	86-82	85-81	
22	82-80	82-80	81-48	79-12		22	84-02	86-32	87-01	84-82	
23	82-88	82-82	81-54	78-93		23	84-03	86-38	87-10	83-71	0-610	
24	82-80	82-92	81-54	79-45		24						Sunday
25	82-84	82-92	81-70	80-25		25	84-21	86-57	87-45	83-89	
26	82-81	82-90	81-69	79-96		26	84-14	86-68	87-57	84-05	
27	82-85	83-02	81-94	79-58		27	84-16	86-72	87-83	85-58	
28						Sunday	28	84-20	87-50	84-22	
29	82-90	83-16	82-14	78-42		29	84-21	87-25	84-08	
30	82-88	83-12	82-22	78-40		30	84-28	87-58	83-95	
31	82-86	83-14	82-25	80-46		31						Sunday
Means	82-78	82-54	81-19	78-54	0-438		Means	83-85	85-72	86-16	83-61	0-610	
Feb. 1	82-98	83-21	82-30	80-46		April 1	84-32	Above	87-61	85-12	
2	82-90	83-31	82-42	81-02		2	84-38	the	87-70	84-00	0-058	
3	82-95	83-40	82-56	81-89		3	84-40	Scale.	87-75	84-40	
4						Sunday	4	84-47	87-90	85-07	
5	82-92	83-48	82-58	80-22		5	84-48	87-90	84-50	
6	83-02	83-56	82-63	80-99		6	84-41	87-71	85-40	
7	82-98	83-57	82-90	80-26		7						Sunday
8	82-98	83-64	83-07	80-19		8	84-58	87-90	84-17	
9	83-02	83-70	83-22	80-75		9	84-64	87-92	81-42	0-245	
10	83-04	83-80	83-42	80-95		10	84-68	87-90	85-07	
11						Sunday	11	84-65	87-88	85-27	
12	83-08	83-94	83-66	79-33		12	84-71	87-94	84-83	
13	83-08	83-97	83-65	80-06		13	84-75	87-92	84-13	
14	83-10	84-08	83-77	79-52		14						Sunday
15	83-13	84-12	83-86	80-90		15	84-86	87-79	84-86	
16	83-12	84-25	84-18	79-88		16	84-90	87-72	85-17	
17	83-12	84-24	84-02	78-30		17	84-80	87-81	83-18	0-148	
18						Sunday	18	84-90	87-80	81-88	0-247	
19	83-18	84-40	84-10	77-20		19	84-90	87-81	83-93	
20	83-20	84-41	84-11	78-10		20	84-88	87-79	85-10	
21	83-23	84-52	84-20	79-25		21						Sunday
22	83-28	84-52	84-18	80-33		22	87-65	85-19	
23	83-26	84-63	84-13	79-61		23	87-64	85-77	
24	83-28	84-67	84-10	79-85		24	87-59	85-10	
25						Sunday	25	87-61	84-68	
26	83-37	84-77	84-23	80-35		26	87-61	84-42	
27	83-40	84-82	84-40	81-31	0-038		27	87-73	83-32	0-037	
28	83-46	84-82	84-48	81-94		28					1-125	Sunday
29	83-44	84-86	84-31	82-30		29	87-84	84-97	
							30	87-82	85-38	
Means	83-14	84-11	83-62	80-13	0-038		Means	84-65	87-78	84-53	1-860	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		
1844. May	o	"	o	o	in.		1844. July	o	o	o	o	in.		
1	Above	Above	87-93	85-65		1	84-48	81-92	80-19	0-128		
2	the	the	87-94	85-65		2	84-36	81-98	79-98	0-587		
3	Scale.	Scale.	87-88	84-33	0-213		3	84-28	82-07	81-21		
4	87-70	85-38	Sunday	4	84-28	82-05	80-88		
5		5	84-23	82-03	80-45		
6	87-67	84-41		6	84-26	82-03	78-26	0-011		
7	87-62	84-47		7	0-278	Sunday	
8	87-53	83-25		8	84-17	81-98	75-29	0-335		
9	87-50	82-31		9	85-00	84-15	81-90	77-77	0-186		
10	87-54	84-21		10	84-98	84-18	81-93	77-65	0-124		
11	87-54	84-02	Sunday	11	84-99	84-16	81-78	77-08	0-580		
12		12	84-90	84-09	81-78	76-50	0-245		
13	87-42	82-16		13	84-90	84-05	81-76	77-67	0-245		
14	87-30	84-48		14	Sunday	
15	87-36	82-88	0-160		15	84-97	83-98	81-45	79-45		
16	87-25	83-41		16	84-88	83-98	81-32	79-87		
17	87-22	83-64	0-218		17	84-90	83-91	81-22	80-38		
18	87-15	80-80	0-552		18	84-90	83-85	81-08	78-80	0-112		
19	1-093	Sunday	19	84-86	83-75	81-02	77-86	0-055		
20	87-09	80-39		20	84-79	83-76	81-08	78-71	0-035		
21	86-94	81-20	0-144		21	Sunday	
22	86-73	80-77		22	84-70	83-62	81-03	78-33	0-015		
23	86-48	80-43	0-445		23	84-68	83-55	81-00	79-43		
24	86-23	79-11	0-400		24	84-67	83-55	81-05	79-53	0-132		
25	86-08	80-97	0-636		25	84-68	83-52	81-11	76-02	0-834		
26	0-687	Sunday	26	84-61	83-48	81-03	76-85	0-069		
27	85-61	79-73	0-205		27	84-60	83-45	81-05	79-54	0-094		
28	85-40	80-21	0-075		28	0-279	Sunday	
29	85-27	78-91	0-020		29	84-58	83-49	81-10	79-34		
30	85-09	77-60	0-176		30	84-56	83-44	81-12	78-41	0-210		
31	84-93	77-36	1-577		31	84-50	83-41	81-00	79-09	0-093		
Means	86-93	82-11	6-601		Means	84-78	83-91	81-48	78-69	4-647		
June	1	84-60	79-11	0-043		Aug.	1	84-49	83-35	81-00	77-75	0-131	
2	0-075	Sunday	2	84-50	83-47	81-07	75-64	0-262		
3	84-07	79-52	0-138		3	84-46	83-39	80-98	76-70	0-169		
4	86-62	83-80	81-62	0-043		4	0-183	Sunday	
5	86-54	83-66	80-05	0-345		5	84-39	83-30	80-93	78-25	0-284		
6	86-38	83-50	79-22	0-240		6	84-35	83-20	80-85	79-26	0-312		
7	86-24	83-45	79-50	0-120		7	84-39	83-25	80-78	79-65		
8	86-20	83-38	80-24		8	84-36	83-24	80-62	77-73	1-016		
9	0-159	Sunday	9	84-35	83-24	80-71	75-85	0-459		
10	85-98	83-22	80-49		10	84-29	83-10	80-45	77-75	0-481		
11	85-88	83-10	81-80		11	0-017	Sunday	
12	85-81	83-07	80-07	0-130		12	84-29	83-09	80-35	78-77	0-283		
13	85-72	82-85	79-84	0-043		13	84-26	82-98	80-29	78-41	0-147		
14	85-62	82-89	77-96	1-434		14	84-23	82-94	80-22	78-63	0-058		
15	85-50	82-78	76-93	0-811		15	84-28	82-88	80-18	80-07		
16	1-951	Sunday	16	84-18	82-82	80-12	79-82		
17	85-36	82-48	78-67	0-265		17	84-22	82-79	80-22	79-30	0-027		
18	85-25	82-27	80-86		18	Sunday	
19	85-12	82-06	81-09		19	84-15	82-70	80-21	78-37	0-073		
20	85-10	82-00	81-40		20	84-10	82-60	80-20	78-14	0-030		
21	84-93	81-82	79-90	0-109		21	84-08	82-62	80-37	78-06	0-134		
22	84-72	81-80	79-72	0-193		22	84-10	82-68	80-40	79-20		
23	0-055	Sunday	23	84-02	82-62	80-35	79-67		
24	84-62	81-86	79-57	0-254		24	84-00	82-59	80-38	79-64		
25	84-55	81-76	80-09	0-848		25	Sunday	
26	84-51	81-81	80-54		26	83-98	82-62	80-50	79-85		
27	84-61	81-88	81-36		27	84-00	82-66	80-51	79-85		
28	84-45	81-91	81-43		28	83-91	82-60	80-61	78-98		
29	84-40	81-88	80-52	0-851		29	83-91	82-58	80-50	79-77		
30	0-082	Sunday	30	83-88	82-61	80-67	79-62		
							31	83-91	82-67	80-71	79-36		
Means	85-40	82-72	80-06	7-389		Means	84-19	82-91	80-54	78-65	4-066		

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.	
1844. Sept. 1	°	°	°	°	in.		1844. Nov. 1	°	°	°	°	in.	
2	83-85	82-66	80-80	77-62	Sunday	2	83-50	82-96	81-51	78-30	0-745	
3	83-80	82-65	80-86	76-48	0-264		3	83-50	83-02	81-50	77-70	0-042	Sunday
4	83-82	82-73	81-03	78-66		4	83-48	83-08	81-62	78-15	0-412	
5	83-84	82-75	81-12	79-15		5	83-54	83-12	81-50	79-06	1-218	
6	83-86	82-77	81-16	78-56	0-406		6	83-50	83-11	81-52	79-16	0-249	
7	83-80	82-79	81-03	79-10	0-214	Sunday	7	83-40	83-10	81-45	80-30	
8				0-538		8	83-48	83-15	81-40	80-66	
9	83-75	82-84	81-13	79-85		9	83-50	83-18	81-40	80-52	0-822	Sunday
10	83-76	82-87	81-12	79-85		10						
11	83-72	82-81	81-00	78-92		11	83-49	83-13	81-38	79-36	0-193	
12	83-74	82-88	81-01	79-62		12	83-49	83-19	81-45	79-25	0-036	
13	83-71	82-89	80-78	79-81		13	83-48	83-12	81-41	80-55	0-025	
14	83-72	82-87	81-02	79-73	Sunday	14	83-51	83-19	81-50	80-25	
15							15	83-47	83-12	81-49	81-12	
16	83-71	82-87	81-00	79-97		16	83-48	83-10	81-41	80-90	0-060	Sunday
17	83-66	82-85	81-06	80-01		17						
18	83-68	82-81	81-09	79-55		18	83-48	83-12	81-45	81-55	
19	83-66	82-88	81-16	80-54		19	83-48	83-18	81-82	81-31	
20	83-64	82-82	81-14	79-72		20	83-48	83-19	81-99	81-06	
21	83-64	82-90	81-30	80-65	Sunday	21	83-47	83-18	81-94	79-78	
22							22	83-45	83-21	82-04	78-43	
23	83-64	82-91	81-50	80-48		23	83-43	83-22	82-09	80-92	Sunday
24	83-66	83-02	81-67	81-20		24						
25	83-62	82-99	81-72	81-50		25	83-47	83-33	82-20	79-65	0-004	
26	83-62	83-09	81-90	79-53	0-040		26	83-44	83-31	82-20	79-54	
27	83-68	83-08	82-05	80-50	1-060		27	83-45	83-34	82-24	80-90	
28	83-62	83-18	82-29	81-25	Sunday	28	83-49	83-42	82-29	80-70	
29							29	83-46	83-44	82-21	81-31	
30	83-50	83-26	82-61	80-50		30	83-48	83-47	82-25	81-99	
Means	83-71	82-89	81-23	79-70	2-527		Means	83-48	83-19	81-74	80-02	3-873	
Oct. 1	83-48	83-29	82-59	79-02	0-591		Dec. 1						Sunday
2	83-58	83-41	82-69	78-69	0-182		2	83-45	83-40	82-39	80-14	
3	83-63	83-50	82-70	80-45		3	83-46	83-51	82-30	78-92	
4	83-61	83-50	82-62	78-42	1-288		4	83-46	83-53	82-40	78-60	
5	83-52	83-50	82-58	76-77	1-914	Sunday	5	83-45	83-51	82-45	78-52	
6				4-203		6	83-50	83-55	82-52	78-25	
7	83-37	83-34	82-50	76-55	1-043		7	83-48	83-61	82-50	78-48	0-050	
8	83-60	83-69	82-10	77-18	1-249		8					0-256	Sunday
9	83-55	83-66	81-74	76-12	2-204		9	83-51	83-70	82-56	78-26	0-014	
10	83-55	83-52	81-35	76-96	0-416		10	83-50	83-75	82-58	78-17	
11	83-58	83-49	81-00	77-96	0-020		11	83-51	83-75	82-50	78-73	
12	83-56	83-40	80-81	77-26	Sunday	12	83-50	83-78	82-41	79-36	
13				0-014		13	83-54	83-81	82-50	79-59	
14	83-56	83-20	80-61	79-96		14	83-52	83-78	82-38	80-01	Sunday
15	83-55	83-10	80-60	79-22		15						
16	83-56	83-07	80-65	80-00		16	83-52	83-72	82-30	80-45	
17	83-56	83-01	80-69	80-58		17	83-58	83-88	82-39	79-32	0-230	
18	83-56	82-98	80-70	79-08		18	83-61	83-82	82-42	77-97	1-216	
19	83-57	82-88	80-68	80-25		19	83-50	83-80	82-35	78-77	
20						Sunday	20	83-59	83-85	82-42	78-77	
21	83-58	82-92	80-90	79-59		21	83-60	83-81	82-40	78-86	0-011	
22	83-58	82-91	80-90	78-21	0-795		22					0-012	Sunday
23	83-56	82-90	81-01	80-51		23	83-61	83-80	82-32	78-47	
24	83-55	82-87	81-02	79-87	0-286		24	83-57	83-78	82-20	78-00	
25	83-52	82-92	81-11	80-37		25	83-61	83-89	82-21	78-29	
26	83-54	82-91	81-12	80-08	0-036		26	83-61	83-80	82-12	78-98	
27				0-042	Sunday	27	83-64	83-80	82-12	80-17	
28	83-55	82-95	81-18	80-24		28	83-65	83-80	82-15	79-79	0-049	
29	83-50	82-95	81-30	78-97		29					0-058	Sunday
30	83-51	82-97	81-39	78-37	0-146		30	83-68	83-78	82-11	80-11	
31	83-51	83-00	81-40	78-82		31	83-64	83-79	82-08	78-57	1-804	
Means	83-55	83-18	81-41	78-94	14-399		Means	83-55	83-73	82-35	79-09	3-700	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.	
1845.	o	o	o	o	in.		1845.	o	o	o	o	in.	
Jan. 1	83-64	83-84	82-14	76-99	0-005		March 1	83-94	85-28	84-88	82-61	
2	83-65	83-70	82-08	80-23		2					0-004	Sunday
3	83-65	83-78	82-11	79-50	0-406		3	84-00	85-36	84-87	82-16	
4	83-68	83-77	82-08	79-46	0-705		4	84-00	85-38	84-80	82-36	
5					0-690	Sunday	5	84-00	85-44	84-99	84-36	
6	83-66	83-72	82-07	78-33		6	84-06	85-48	84-90	80-38	0-534	
7	83-65	83-68	82-09	78-55		7	84-06	85-54	85-00	79-38	0-022	
8	83-59	83-74	82-04	79-16		8	84-06	85-45	85-03	82-52	
9	83-66	83-70	82-00	79-36		9						Sunday
10	83-69	83-70	82-04	77-98		10	84-11	85-52	85-12	81-43	
11	83-68	83-65	81-95	79-17		11	84-13	85-60	85-08	82-43	
12						Sunday	12	84-17	85-65	85-10	79-23	1-291	
13	83-65	83-67	82-11	79-09		13	84-18	85-65	85-05	79-13	0-263	
14	83-65	83-64	81-80	77-66		14	84-20	85-70	85-05	80-74	0-322	
15	83-68	83-63	81-61	77-51		15	84-24	85-70	85-01	78-89	0-140	
16	83-68	83-60	81-55	78-86		16					0-470	Sunday
17	83-65	83-59	81-60	78-15		17	84-27	85-71	84-84	80-12	0-040	
18	83-66	83-52	81-38	78-31		18	84-28	85-68	84-60	78-58	1-928	
19						Sunday	19	84-27	85-68	84-68	80-50	0-286	
20	83-69	83-56	81-50	77-85		20	84-32	85-70	84-42	80-62	
21	83-60	83-50	81-46	79-63		21	84-35	85-67	84-38	83-00	
22	83-68	83-47	81-35	79-42		22	84-32	85-61	84-20	84-06	
23	83-68	83-50	81-50	79-69		23						Sunday
24	83-68	83-55	81-61	80-43		24	84-44	85-61	84-21	84-92	
25	83-68	83-48	81-58	82-13		25	84-44	85-58	84-13	84-91	
26						Sunday	26	84-44	85-53	84-20	85-64	0-003	
27	83-66	83-52	81-94	78-97	0-280		27	84-45	85-58	84-25	84-89	
28	83-66	83-52	81-98	79-33		28	84-49	85-61	84-40	84-92	
29	83-66	83-57	82-16	81-07		29	84-52	85-50	84-49	84-89	
30	83-68	83-60	82-20	80-98		30						Sunday
31	83-65	83-59	82-18	80-54		31	84-50	85-51	84-60	85-05	
Means	83-66	83-62	81-86	79-20	2-086		Means	84-24	85-57	84-70	82-22	5-303	
Feb. 1	83-66	83-70	82-40	80-04		April 1	84-50	85-49	84-70	84-23	0-075	
2						Sunday	2	84-49	85-50	84-82	83-90	
3	83-70	83-77	82-65	80-09		3	84-52	85-58	84-92	83-50	
4	83-68	83-78	82-70	80-49		4	84-60	85-62	85-08	83-84	
5	83-65	83-81	82-70	80-26		5	84-55	85-65	85-20	83-76	Sunday
6	83-68	83-90	82-80	80-30		6						
7	83-68	83-92	82-90	80-65		7	84-55	85-72	85-31	84-91	
8	83-65	83-90	82-92	81-13		8	84-55	85-79	85-40	85-15	
9						Sunday	9	84-54	85-76	85-48	83-95	
10	83-64	84-05	83-05	81-47		10	84-55	85-88	85-54	84-30	
11	83-76	84-12	83-12	81-08		11	84-64	85-79	85-61	84-58	
12	83-70	84-15	83-41	81-04		12	84-60	86-00	85-78	84-45	Sunday
13	83-70	84-18	83-50	81-78		13						
14	83-72	84-21	83-55	81-30		14	84-66	86-12	86-01	84-22	
15	83-71	84-32	83-75	81-00		15	84-60	86-15	86-15	84-20	
16						Sunday	16	84-64	86-13	86-19	84-74	
17	83-75	84-46	84-10	81-44		17	84-66	86-20	86-25	84-57	
18	83-72	84-50	84-20	82-01		18	84-70	86-37	86-56	83-75	0-280	
19	83-72	84-60	84-32	81-83		19	84-68	86-45	86-50	83-02	0-078	
20	83-82	84-70	84-50	82-09		20					0-275	Sunday
21	83-82	84-78	84-61	80-29		21	84-64	86-50	86-39	82-60	
22	83-80	84-70	84-69	78-85	0-006		22	84-78	86-65	86-92	83-71	
23					0-960	Sunday	23	84-78	86-69	86-81	84-42	
24	83-78	84-95	84-90	80-64		24	84-78	86-70	86-65	81-72	0-162	
25	83-86	85-08	84-99	80-41		25	84-75	86-69	86-70	84-35	
26	83-88	85-10	85-01	81-89		26	84-82	86-50	83-19	
27	83-87	85-15	84-90	81-26		27						Sunday
28	83-88	85-18	84-95	81-26		28	84-84	86-41	84-97	
							29	84-98	86-35	83-11	
							30	84-88	86-50	84-83	
Means	83-74	84-38	83-78	80-94	0-966		Means	84-66	86-06	85-72	84-00	0-870	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.	
1845. May	°	°	°	°	in.		1845. July	°	°	°	°	in.	
1	85-00	86-50	81-61	0-351	Sunday	1	84-86	83-99	80-98	77-11	0-236	Sunday
2	84-92	86-60	81-52	1-351		2	84-85	83-84	80-90	80-05	
3	84-98	86-64	81-91	0-357		3	84-80	83-70	80-75	80-89	
4	0-595		4	84-75	83-70	80-55	80-81	
5	84-95	86-64	78-92	0-490	Sunday	5	84-76	83-50	80-50	80-43	Sunday
6	84-90	86-52	80-81	0-550		6	
7	84-90	86-41	82-67	0-372		7	84-72	83-43	80-60	79-57	0-153	
8	86-45	81-77	0-201		8	84-65	83-37	80-58	79-85	0-087	
9	85-99	79-49	1-211	Sunday	9	84-65	83-30	80-60	79-93	0-021	Sunday
10	85-70	80-48	0-607		10	84-60	83-32	80-52	80-82	0-093	
11		11	84-62	83-25	80-76	80-80	
12	85-36	82-51		12	84-58	83-20	80-22	78-29	0-140	Sunday
13	86-70	85-10	82-90	0-080	Sunday	13	0-122	
14	86-65	85-00	82-63		14	84-50	83-07	80-60	77-21	0-170	
15	86-60	84-80	82-43		15	84-50	83-15	80-70	74-59	0-182	
16	86-50	84-71	82-90	Sunday	16	84-48	83-13	80-75	75-33	0-575	Sunday
17	86-42	84-60	83-27		17	84-43	83-19	80-89	77-51	0-274	
18		18	84-50	83-05	80-55	78-77	0-502	
19	86-22	84-42	81-55	0-880		19	84-40	83-10	80-50	79-79	0-050	Sunday
20	86-25	84-48	81-89	0-230	Sunday	20	0-003	
21	86-17	84-52	82-57	0-042		21	84-38	83-00	80-32	80-12	
22	86-10	84-40	83-11		22	84-35	83-00	80-41	78-57	0-160	
23	86-10	84-50	83-78	Sunday	23	84-30	82-90	80-40	79-56	0-020	Sunday
24	86-10	84-50	83-39		24	84-26	82-84	80-37	80-21	
25		25	84-25	82-88	80-34	80-06	
26	86-11	84-53	82-83	0-156		26	84-40	82-82	80-41	80-17	Sunday
27	85-98	84-55	82-79	Sunday	27	
28	85-99	84-47	83-72		28	84-21	82-85	80-61	80-26	
29	86-00	84-60	83-04	0-021		29	84-15	82-76	80-62	80-77	
30	85-92	84-61	79-51	0-320	Sunday	30	84-15	82-75	80-60	80-76	Sunday
31	85-90	84-55	78-81	2-158		31	84-10	82-80	80-78	80-81	
Means	84-94	86-22	85-23	82-08	9-902		Means	84-49	83-18	80-59	79-37	2-788	
June							Aug.						
1	85-88	84-49	78-95	0-087	Sunday	1	84-10	82-80	80-70	81-08	0-078	Sunday
2	85-90	84-41	79-10	1-193		2	84-08	82-78	80-70	81-12	
3	85-86	84-35	80-77		3	
4	85-52	84-32	79-52	0-471		4	84-05	82-80	81-05	80-76	
5	85-70	84-09	80-41	0-030	Sunday	5	84-00	82-80	81-02	81-34	Sunday
6	85-86	83-91	82-04	0-003		6	83-90	82-80	81-24	80-90	0-029	
7		7	83-98	82-86	80-96	81-61	
8		8	83-96	82-80	81-30	81-50	
9	85-72	83-74	80-38	0-922	Sunday	9	83-94	82-92	81-53	80-99	0-043	Sunday
10	85-58	83-50	79-69	0-022		10	
11	85-52	83-49	78-45	1-649		11	83-89	82-87	81-49	80-57	
12	85-45	83-00	80-27		12	83-95	82-89	81-75	81-00	
13	85-41	83-33	79-94	0-048	Sunday	13	83-85	83-05	81-91	81-09	0-149	Sunday
14	85-39	83-30	78-09		14	83-86	83-11	81-90	78-50	
15	0-655		15	83-85	83-15	82-00	77-70	0-133	
16	85-10	82-98	75-99	1-552		16	83-80	83-18	82-00	79-90	
17	85-13	82-80	78-23	Sunday	17	Sunday
18	85-11	82-71	79-53	0-144		18	83-90	83-11	81-90	77-55	0-526	
19	85-09	82-73	80-58		19	83-76	83-22	81-82	79-28	
20	84-91	82-31	81-49		20	83-81	83-33	81-91	79-70	
21	84-88	82-02	80-07	Sunday	21	83-79	83-36	81-90	79-77	Sunday
22	0-047		22	83-80	83-35	81-89	79-75	0-124	
23	84-68	81-90	76-75	1-276		23	83-81	83-34	81-89	80-73	
24	84-59	81-86	76-03	0-981		24	
25	84-40	81-90	76-36	0-744	Sunday	25	83-76	83-40	81-74	81-31	Sunday
26	85-00	84-20	81-77	76-30	0-355		26	83-88	83-37	81-89	81-23	
27	84-98	84-60	81-69	78-40	0-112		27	83-79	83-36	81-75	80-23	
28	84-92	84-34	81-61	76-43	0-857		28	83-75	83-30	81-75	79-68	0-052	
29	1-280	Sunday	29	83-75	83-32	81-81	78-93	0-005	Sunday
30	84-85	84-08	81-33	76-31	1-059		30	83-76	83-40	81-98	79-48	
Means	84-94	85-17	82-95	78-80	13-584		Means	83-89	83-10	81-60	79-27	1-139	

Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.		Date.	No. 1. 12 feet Therm. 6 A.M.	No. 2. 6 feet Therm. 6 A.M.	No. 3. 3 feet Therm. 6 A.M.	Mean Temp. of Air.	Rain 8½ A.M. to 8½ A.M.	
1845.	°	°	°	°	in.		1845.	°	°	°	°	in.	
Sept. 1	83-80	83-39	81-94	79-43		Nov. 1	84-10	83-72	81-87	80-41	
2	83-75	83-40	82-15	80-00		2					1-273	Sunday
3	83-78	83-42	82-10	80-01		3	84-10	83-72	82-12	79-96	
4	83-79	83-51	82-40	79-04	0-030		4	84-10	83-75	82-11	79-87	
5	83-59	83-55	82-37	78-31	0-104		5	84-08	83-73	82-20	79-58	
6	83-76	83-55	82-55	78-92		6	84-05	83-75	82-15	81-00	
7					0-025	Sunday	7	84-10	83-72	82-20	79-22	2-138	
8	83-70	83-42	82-50	79-70	0-047		8	84-05	83-81	82-22	79-41	
9	83-70	83-60	82-61	79-47		9						Sunday
10	83-80	83-75	82-65	79-82		10	84-00	83-86	82-30	78-79	
11	83-75	83-72	82-61	79-54		11	84-00	83-85	82-25	80-01	
12	83-70	83-72	82-60	79-52		12	84-00	83-85	82-24	79-97	0-015	
13	83-77	83-75	82-62	80-35		13	84-05	83-90	82-18	76-93	0-420	
14						Sunday	14	84-00	83-90	82-12	79-06	0-281	
15	83-85	83-88	82-87	80-25		15	84-02	83-89	82-10	80-03	0-007	
16	83-79	83-85	82-93	80-08		16						Sunday
17	83-80	83-81	82-90	79-57		17	84-00	83-84	82-05	79-90	
18	83-78	83-95	83-20	81-15		18	83-95	83-78	81-98	77-90	
19	83-80	84-00	83-39	80-30		19	84-00	83-72	81-94	78-43	0-200	
20	83-76	84-02	83-50	80-89		20	84-00	83-82	81-90	79-33	
21						Sunday	21	84-00	83-85	81-81	79-64	0-390	
22	83-80	84-16	83-76	80-67		22	84-00	83-75	81-80	79-87	0-142	
23	83-82	84-20	83-80	80-14		23					1-077	Sunday
24	83-80	84-15	83-91	79-59		24	84-03	83-70	81-84	80-48	
25	83-80	84-32	83-99	80-45		25	83-92	83-74	81-59	79-55	0-386	
26	83-80	84-40	84-10	80-42		26	83-98	83-70	81-70	79-54	
27	83-85	84-48	84-22	81-47	0-012		27	83-96	83-60	81-72	79-49	
28					0-030	Sunday	28	83-89	83-59	81-70	79-94	
29	83-88	84-61	84-22	81-31		29	83-92	83-57	81-70	79-39	
30	83-85	84-64	84-25	81-32		30						Sunday
Means	83-78	83-89	83-08	80-68	0-248		Means	84-01	83-36	81-99	79-51	5-422	
Oct.							Dec.						
1	83-88	84-69	84-30	80-81		1	83-92	83-50	81-51	79-22	
2	83-88	84-78	84-25	80-10	0-403		2	83-96	83-51	81-75	76-80	2-348	
3	83-92	84-75	84-30	80-61	0-113		3	83-74	83-34	81-33	77-46	0-042	
4	83-98	84-92	84-50	80-61	0-070		4	83-70	83-29	81-30	78-80	
5						Sunday	5	83-84	83-42	81-40	79-90	
6	83-98	84-91	84-50	79-98		6	83-92	83-48	81-22	79-99	
7	83-95	84-96	84-50	77-35	3-656		7					0-031	Sunday
8	83-95	84-95	84-45	76-79	0-992		8	83-90	83-46	81-23	79-47	0-376	
9	83-98	84-98	84-50	78-42	0-314		9	83-90	83-35	81-15	78-49	0-102	
10	84-00	84-99	84-50	78-14	0-415		10	83-88	83-29	81-10	79-58	
11	84-00	85-01	84-02	77-32	2-530		11	83-90	83-50	81-12	79-08	0-040	
12					2-771	Sunday	12	83-86	83-25	81-20	79-02	
13	84-00	84-98	84-54	77-01	3-892		13	83-86	83-21	81-20	78-91	
14	84-05	85-00	83-05	76-94	0-734		14						Sunday
15	84-00	85-00	82-20	77-58	1-251		15	84-00	83-44	81-30	79-44	
16	84-16	84-80	81-98	79-50	0-143		16	83-84	83-20	81-19	80-76	
17	84-14	84-65	81-91	80-60	0-100		17	83-84	83-20	81-21	80-51	
18	81-15	84-51	81-70	79-35		18	83-82	83-21	81-18	79-97	0-035	
19						Sunday	19	83-80	83-20	81-17	78-81	
20	84-16	84-22	81-50	80-05		20	83-78	83-16	81-20	79-29	
21	84-22	84-22	81-50	80-37		21						Sunday
22	84-15	84-12	81-45	80-54		22	83-79	83-19	81-29	80-11	0-026	
23	84-21	84-02	81-41	80-10		23	83-82	83-20	81-10	79-59	
24	84-15	83-90	81-32	80-85		24	83-90	83-18	81-15	80-14	
25	84-10	83-88	81-30	80-64	0-074		25	83-75	83-19	81-24	79-85	0-640	
26						Sunday	26	83-76	83-26	81-42	77-60	1-356	
27	84-16	83-78	81-34	81-00		27	83-80	83-31	81-39	79-54	
28	84-17	83-75	81-60	80-48		28						Sunday
29	84-13	83-71	81-72	80-08		29	83-75	83-28	81-32	79-84	
30	84-11	83-69	81-80	79-45		30	83-76	83-44	81-52	79-56	
31	84-10	83-73	81-85	79-90		31	83-70	83-30	81-32	79-50	
Means	84-06	84-48	82-82	79-43	17-458		Means	83-80	83-31	81-28	79-31	4-997	

ABSTRACT

*Monthly Means of Terrestrial Temperature, at the Trevandrum Observatory.**Lat. 8° 30' 32" N. Long. 5° 7' 59" E.*

No. 1. 12 feet Thermometer.				No. 2. 6 feet Thermometer.				No. 3. 3 feet Thermometer.				Mean Temp. of Air.	Rain.	Average of Four Daily Observations, corrected for Index Error.			
A.M. 6.	Noon.	P.M. 6.	Mid.	A.M. 6.	Noon.	P.M. 6.	Mid.	A.M. 6.	Noon.	P.M. 6.	Mid.						
84.66	84.74	84.73	84.57	85.11	85.25	85.20	85.18	83.77	83.92	83.80	83.79	80.09	14.5134	86.808	87.357	86.742	
.....	84.58	84.66	84.57	84.48	82.00	82.10	82.11	82.01	79.32	8.7473	86.742	84.977	
84.76	84.83	84.87	84.76	83.62	83.61	83.59	83.62	80.97	81.07	81.03	80.99	78.73	5.9516	86.938	85.782	83.901	
84.23	84.29	84.24	84.23	82.74	82.82	82.77	82.74	80.18	80.29	80.22	80.21	77.90	4.4240	86.380	84.940	83.148	
83.75	83.86	83.79	83.74	82.85	82.92	82.88	82.87	81.28	81.38	81.31	81.29	78.28	7.7238	85.918	85.052	84.237	
83.68	83.78	83.71	83.69	83.04	83.16	83.07	82.99	81.44	81.60	81.51	81.49	79.10	5.4928	85.843	85.237	84.437	
83.62	83.72	83.65	83.61	82.69	82.80	82.73	82.69	80.37	80.45	80.38	80.34	77.82	8.8053	85.783	84.899	83.307	
83.34	83.49	83.41	83.37	82.89	82.98	82.85	82.82	81.52	81.73	81.58	81.51	78.96	0.1642	85.535	85.057	84.507	
83.61	83.74	83.67	83.58	83.94	84.14	84.06	84.02	82.75	82.92	82.86	82.82	79.05	1.154	85.783	86.212	85.759	
83.85	84.05	83.99	83.92	84.53	84.78	84.70	84.54	81.00	81.30	81.16	81.04	80.09	0.033	86.085	86.809	87.047	
84.43	84.60	84.52	84.46	86.27	86.68	86.37	86.31	86.41	86.70	86.55	86.48	82.36	1.721	86.643	88.579	89.457	
84.92	85.01	84.94	84.96	86.60	86.68	86.59	86.53	86.17	86.33	86.16	86.11	81.58	9.274	87.090	88.772	89.114	
.....	86.03	86.12	86.07	85.99	84.26	84.38	84.27	84.21	80.62	15.989	88.224	87.202	
84.75	84.78	84.74	84.71	83.59	83.59	83.56	83.53	80.68	80.65	80.60	80.58	78.21	16.932	86.878	85.739	83.549	
83.90	83.96	83.90	83.88	81.70	81.77	81.70	81.66	78.82	78.91	78.85	78.84	77.29	10.399	86.043	83.879	81.777	
83.16	83.22	83.17	83.16	81.58	81.67	81.60	81.58	79.66	79.79	79.74	79.72	79.05	2.098	85.310	83.779	82.649	
82.90	82.99	82.97	82.94	81.95	82.06	82.00	81.96	80.46	80.55	80.45	80.44	79.54	3.330	85.083	84.164	83.397	
82.95	83.01	82.96	82.94	82.65	82.74	82.66	82.65	81.09	81.18	81.11	81.09	78.86	8.830	85.098	84.847	84.039	
82.96	83.05	83.00	82.95	82.63	82.75	82.68	82.54	81.19	81.33	81.25	81.20	79.72	1.813	85.123	84.822	84.167	
82.90	83.00	82.92	82.89	82.24	82.35	82.26	82.22	80.17	80.26	80.20	80.10	77.69	13.400	85.060	84.439	83.104	
82.78	82.92	82.84	82.78	82.54	82.73	82.64	82.58	81.19	81.43	81.30	81.20	78.54	0.438	84.963	84.794	84.202	
83.14	83.29	83.21	83.17	84.11	84.34	84.23	84.15	83.62	83.90	83.74	83.64	80.13	0.038	85.335	86.379	86.647	
83.85	84.04	83.94	83.89	85.72	85.92	85.80	85.73	86.16	86.51	86.33	86.28	83.61	0.610	86.063	87.964	89.242	
84.65	84.77	84.68	84.65	87.78	88.06	87.86	87.80	84.53	1.860	86.820	90.797	
.....	86.93	87.06	86.94	86.84	82.11	6.601	89.564	
.....	85.40	85.49	85.38	85.34	82.72	82.82	82.70	82.65	80.06	7.389	87.574	85.644	
84.78	84.84	84.79	84.74	83.91	84.01	83.91	83.86	81.48	81.57	81.49	81.44	78.69	4.647	86.094	84.417	
84.19	84.27	84.21	84.15	82.91	83.01	82.92	82.89	80.54	80.63	80.55	80.50	78.65	4.066	86.338	85.104	83.477	
83.71	83.80	83.74	83.69	82.89	83.03	82.95	82.90	81.23	81.44	81.35	81.29	79.70	2.527	85.868	85.114	84.249	
83.55	83.66	83.59	83.53	83.18	83.28	83.22	83.17	81.41	81.52	81.42	81.36	78.94	14.399	85.715	85.384	84.349	
83.48	83.59	83.51	83.47	83.19	83.33	83.24	83.19	81.74	81.92	81.81	81.76	80.02	3.873	85.645	85.409	84.729	
83.55	83.78	83.59	83.54	83.73	83.91	83.80	83.73	82.35	82.53	82.41	82.36	79.09	3.700	85.748	85.964	85.334	
83.66	83.80	83.71	83.65	83.62	83.79	83.68	83.62	81.86	82.30	81.91	81.85	79.20	2.086	85.838	85.850	84.902	
83.74	83.91	83.82	83.74	84.38	84.79	84.49	84.40	83.78	84.06	83.92	83.84	80.94	0.966	85.933	86.687	86.822	
84.24	84.39	84.28	84.22	85.57	85.74	85.61	85.54	84.70	84.88	84.73	84.68	82.22	5.303	86.415	87.787	87.669	
84.66	84.79	84.72	84.66	84.06	86.20	86.13	86.06	85.72	86.20	86.09	86.03	84.00	0.870	86.840	88.284	88.932	
84.94	84.91	84.84	86.22	86.24	86.22	86.17	85.23	85.40	85.23	85.15	82.08	9.902	88.174	
84.94	84.98	84.96	84.92	85.17	85.31	85.15	85.03	82.95	83.04	82.89	82.81	78.80	13.584	87.337	85.844	
84.49	84.59	84.52	84.46	83.18	83.29	83.18	83.14	80.59	80.75	80.63	80.57	79.37	2.788	86.658	85.369	83.557	
83.89	83.99	83.90	83.85	83.10	83.24	83.17	83.11	81.60	81.77	81.65	81.58	79.27	1.139	86.035	85.327	84.572	
83.78	83.90	83.82	83.76	83.89	84.05	83.97	83.89	83.08	83.28	83.17	83.10	80.68	0.248	85.948	86.122	86.079	
84.06	84.20	84.09	84.03	84.48	84.60	84.49	84.41	82.82	82.96	82.83	82.80	79.43	17.458	86.228	86.667	85.779	
84.01	84.14	84.06	84.00	83.36	83.52	83.42	83.35	81.99	82.15	82.06	82.00	79.51	5.422	86.185	85.584	84.972	
83.80	84.01	83.88	83.84	83.31	83.45	83.30	83.28	81.28	81.44	81.32	81.26	79.31	4.997	86.015	85.507	84.247	

Remarks on the Preceding Observations. By Professor J. D. FORBES.

Mr CALDECOTT's observations possess an extraordinary interest from being the first of the kind prosecuted between the tropics, from the great care and extent of the observations, and from the circumstances being altogether comparable with those of observations lately made in Europe. [The depths of the thermometers are the same as those at Brussels, Edinburgh, and Greenwich.]

In conformity with Mr CALDECOTT's suggestion, I have had the *corrected* means of 1843-4-5 united, so as to give the mean temperature of each month (the observations of 1842 being omitted). The results are given in the following Table. The readings of Nos. 1 and 2 are deficient in some of the months, owing to the liquid having risen above the scale :—

MEAN OF THREE YEARS, 1843-5.

	No. 1. 12 feet Thermo- meter.	No. 2. 6 feet Thermo- meter.	No. 3. 3 feet Thermo- meter.	Air Temperature.
January	85.528	85.618	84.954	78.930
February	85.784	86.625	86.838	80.386
March	86.373	88.110	88.789	82.730
April	86.916	88.527*	89.614	83.370
May	88.224†	88.413	81.603
June	86.878†	86.883	85.012	79.023
July	86.537	85.114	83.250	78.450
August	85.894	84.736	83.566	78.990
September	85.633	85.133	84.575	79.973
October	85.680	85.632	84.722	79.076
November	85.651	85.271	84.622	79.750
December	85.607	85.303	84.228	78.030
Means	86.043	86.264	85.715	80.025
* Mean of Two Years only. † Result of 1843 only.				

The following conclusions are plainly deducible :—

I. The Temperature of the ground at Trevandrum is from 5° to 6° Fahr. *higher* than that of the air. This result is confirmed by observations on the temperature of springs and wells at Trevandrum, which have been obligingly communicated to me by Major-General CULLEN of the Madras Artillery. These observations are printed in the "Proceedings" of this Society.

II. When the monthly means of the thermometers are projected, so as to shew the curves of annual temperature, they are found to have one great inflection and a smaller one. The principal maximum of the temperature of the AIR occurs about the beginning of April, after which the rainy season sets in, and the annual

curve goes through its extreme range in three months; the principal minimum occurring about the middle of July. The remaining fluctuations are comparatively insignificant, but indicate a slight maximum about the middle of October.

III. The epochs of temperature are retarded with the depth below the surface in the usual manner, and, at the same time, casual fluctuations disappear, and the ranges diminish. At 12 French feet, the principal maximum occurs five weeks later than in the open air, and the range is still at least a degree and a half.

From these facts, it is easy to infer that the phenomena of the propagation of heat into the ground near the equator resemble those of temperate latitudes, though modified in extent and character. Mr CALDECOTT's experiments conclusively establish (as he himself has pointed out) the error of the doctrine of BOUSSINGAULT (at least for the eastern hemisphere), that the annual temperature near the equator remains unchanged at the depth of a foot below the surface in the shade. This mistake it is the more important to correct, because M. POISSON has attempted to confirm his mathematical theories of heat by applying them to this alleged fact.*

Mr CALDECOTT's experiments appear farther to prove a considerable excess of the temperature of the earth above that of the air at Trevandrum. This result is in opposition to the opinion of KUPFFER, which supposes the earth temperature to be *less* than that of the air between the tropics, and that of BOUSSINGAULT, which supposes them to be the same.

The results of Mr CALDECOTT are confirmed in both particulars by Captain NEWBOLD of the Madras Army, in a paper lately published in the London Philosophical Transactions.†

* Théorie de la Chaleur, p. 508.

† For 1845, p. 125.







XXVII.—*On the Parallel Roads of Lochaber, with Remarks on the Change of Relative Levels of Sea and Land in Scotland, and on the Detrital Deposits in that Country.* By DAVID MILNE, Esq.

(Read 1st March and 5th April 1847.)

There are few questions in geology which have given rise to so many theories, and so much speculation, as the origin of the parallel roads in the valleys of Lochaber.

In the year 1817, the late Dr MACCULLOCH gave an elaborate description of them, in a paper read before the Geological Society of London. In the year 1818, Sir THOMAS DICK LAUDER read before the Royal Society of Edinburgh a paper, full of equally interesting details. Both of these observers suggested, in explanation of the shelves which mark the mountain sides of these valleys, that they had been occupied by lakes, which, by earthquakes or other violent convulsions, had been drained. This theory was generally received, until, in the year 1839, Mr DARWIN, so justly celebrated as a geologist, and an accurate observer, published his views, and pronounced the shelves to have been formed by the sea; an opinion which, besides being rested on proofs derived from the locality, he enforced also by his observation of similar appearances in South America.

Mr DARWIN's opinion has received the assent of Sir RODERICK I. MURCHISON, Mr LYELL, and Mr HORNER, all successively Presidents of the Geological Society, besides other geologists, both at home and abroad, who are justly regarded as authorities in physical science. Relying on the soundness of their views, I confess that when I went to Glen Roy, in the year 1845, it was with a strong conviction that the lake theory was indefensible; a view to which I was the more inclined, from having studied certain marks along different parts of the Scottish coast, on both sides of the island, which satisfied me that the sea had recently stood at a much higher relative level than at present; and that, in its recession, it had formed, all round our coasts, shelves or beach lines, very analogous to those in the Lochaber valleys. I had not been two days in Glen Roy, before I satisfied myself that these views were inapplicable to the shelves in it and its associated valleys. But I was unable, during my visit of 1845, to remain long enough to obtain evidence of the manner in which the lakes had been dammed up, and eventually drained. I therefore resolved to defer the farther consideration of the subject, until I could pay a second visit. This I accomplished in September 1846, when I spent a week in the examination.

In the following paper, I shall attempt to explain my reasons for thinking

Mr DARWIN'S theory inadmissible, and to point out the manner in which, as it appears to me, that the lakes were drained,—not as supposed by Dr MACCULLOCH and Sir THOMAS DICK LAUDER, by convulsions of nature, but by the gradual operation of ordinary causes.

Though it is the principal object of this paper to account for the formation of the Lochaber shelves, there are no views regarding them which can be suggested, which have not a more general bearing, and the soundness of which may be tested by evidence supplied from other sources. Former writers, accordingly, and especially Mr DARWIN, have felt it to be necessary, after giving their explanation of the parallel roads, to shew, that the principles on which it rests, are, at least, not inconsistent with any established truths in other branches of geology.

I shall not shrink from subjecting the Lake theory, which I have to submit, to a similar ordeal; and the more so, as I feel satisfied that it receives great support from geological considerations now held to be well established.

As the whole details of the parallel roads have been fully described by former writers, I shall limit myself to points on which I have obtained new information, or with regard to which doubts have been expressed.

1. One of the points of the class last referred to, is the absolute horizontality of the shelves. Mr DARWIN, referring to Sir THOMAS DICK LAUDER'S observations on this point (p. 76.), hints at the possibility of errors and omissions in the calculation. M. BRAVAIS, in his paper on the lines of former sea-level in Finmark, suggests, "that an accurate geodetic levelling should be applied in the case of the *doubtful lines* in Scotland," evidently referring to Glen Roy. Mr HORNER, the president of the Geological Society, in his last year's address, observes: "Mr DARWIN'S explanation of the parallel roads of Glen Roy, that they are ancient sea-beaches, appears to be now generally accepted; and it would be most interesting, if it were ascertained by exact levellings, such as those of M. BRAVAIS, *whether they really are parallel*." Similar doubts had been expressed by Sir R. I. MURCHISON, Mr HORNER'S predecessor, in his anniversary address of 1843; in support of which, he refers to the concurrent opinion of M. de BEAUMONT and Professor PHILLIPS.

In accordance with the doubts expressed by these authorities, the Geological Section of the British Association, at their last meeting, agreed on an address to Her Majesty's Government, requesting them to cause the parallel roads of Lochaber to be examined by the officers of the Ordnance Survey, to ascertain their supposed horizontality.

I have no doubt that the result of this official survey, if made, will be to establish the absolute horizontality of the shelves. In August 1844, Mr D. STEVENSON, at my request, was so obliging as to examine them, and the conclusion at which he arrived, is explained in a letter to me, from which I make the following extracts. "I have had a number of levels taken, the particulars of which I shall

give you afterwards. The *result*, I think, *leaves no doubt as to the perfect horizontality of the 'roads.'* The glen is much more extensive, both as regards length and breadth, than I anticipated, and the height of the roads above its bottom is also very considerable, and any thing like a series of cross sections, referred to the same datum, would be a work of very great magnitude; a month, I should say, would not complete it. The whole we have been able to do, therefore, is to test the uniformity of the levels of the different roads, by viewing them with a good instrument from several points, as was done by Sir THOMAS DICK LAUDER; and, in addition to this, a section was made along the middle road, where it is pretty well defined from Glen Turret downwards, for a distance of nearly $3\frac{1}{2}$ miles, and throughout that stretch, the road was found to be *perfectly horizontal.*" . . . "If I had seen that any thing further could be done, I would have left my assistants for a few days longer; they were there a week."

These observations of Mr STEVENSON, whose professional accuracy is undeniable, confirming, as they so completely do, the result of Sir THOMAS DICK LAUDER's survey (and he, too, was aided by an engineer), leave no doubt in my mind, as to the horizontality of the roads. It is scarcely necessary to refer to any farther and weaker testimony on the subject. But it may be proper to add, that during the two occasions when I visited Glen Roy, I had a pocket-level with me, which I constantly used; and that on the last visit I was accompanied by Mr R. CHAMBERS of Edinburgh, who had a larger spirit-level, and we never could detect any deviation from horizontality.

2. There is a point of some importance bearing on the theory of the shelves, about which former observers have disputed. MACCULLOCH found by his barometric observations, that the Glen Gluoy uppermost shelf is 12 feet above the highest in Glen Roy; but he attributed this difference to errors of observation, and his theory in regard to the formation of the shelves proceeds expressly on the assumption, that these shelves are precisely on the same level. Sir THOMAS DICK LAUDER mentions, however, that Mr McLEAN, the engineer who assisted him, made the Glen Gluoy shelf 12 feet above that in Glen Roy, whilst Sir THOMAS himself made it 15 feet. According to the observations made by myself and Mr CHAMBERS last September, the difference is much greater. By levelling, we made it 29 feet; by joint barometric and sympiesometer observations, I made it 23 feet.

3. Whilst on the subject of Glen Gluoy, I may mention that I discovered in it a second shelf, which the barometer shewed to be 200 feet, and the sympiesometer 213 feet, below the level of the one before referred to. I detected it first immediately above the mouth of Glen Fintec. It is traceable on both sides of the glen, and for several miles upwards.

4. There is a circumstance of great importance, in the theory of these roads, on which I was so fortunate as to obtain farther information. I allude to the fact, that most of the shelves are coincident with some summit level, so as to ad-

mit of the waters flowing over that level as over a lip. Thus the uppermost shelf of Glen Gluoy No. 1, in Sir THOMAS DICK LAUDER's Memoir, is (as he explains) exactly coincident with the water-shed ridge which divides Glen Gluoy from Glen Roy, so that the waters (whatever they were) which stood at that height and formed the beach No. 1, must have flowed out at the head of Glen Gluoy into Glen Roy. In like manner, the uppermost shelf in Glen Roy, No 2 in Sir THOMAS DICK LAUDER's Memoir, is (as he also mentions) exactly coincident with the water-shed ridge which divides Glen Roy from the valley of the Spey; so that the waters which stood in Glen Roy at No. 2 beach, must have flowed over the head of the Glen into Spey valley. In like manner, the only shelf which occurs in Glen Spean, No. 4 in Sir THOMAS DICK LAUDER's Memoir, is exactly coincident with, or rather is a few feet above, the pass of Mukkul at the head of Loch Laggan, through which pass, the waters standing at the level of No. 4 must have flowed eastward into Spey valley. These coincidences, as Mr DARWIN admits, "are so remarkable, that they must (I use his own words) be intimately connected with the origin of the shelves; although such relation is not absolutely necessary, *inasmuch as the middle shelf of Glen Roy, is not on a level with any water-shed.*" (P. 43.)

The middle shelf here alluded to is No. 3 in Sir THOMAS DICK LAUDER's list. The discovery which I made, was its exact coincidence with a water-shed at the head of Glen Glaster, a glen which, though branching up from Glen Roy near the bottom of it, oddly enough does not appear to have been visited, and certainly not to have been described, by any former observer.

Shelves 3 and 4 are the only shelves which enter and run up this glen. Sir THOMAS DICK LAUDER's map inaccurately represents shelf 2 as marking it on both of its sides. Shelf 2 stops, however, on both sides of Glen Roy a little to the eastward of, or above the mouth of Glen Glaster.

In following shelf 3 to the head of this glen, I found that it was there lost in a low mossy flat. A little beyond this flat, and a few feet below the summit-level, an *old river-course* can be distinctly traced down a slope towards Loch Laggan. It has a rocky bed, over which a great body of water had evidently flowed at some former period. The breadth of the rocky bed is from 30 to 40 feet; the knolls of rock are from 2 to 5 feet high, and amongst them are rounded blocks of stone, such as occur in all great Highland rivers. I traced this rocky channel for about a mile towards Loch Laggan; and I afterwards found the place where it had discharged its waters into Loch Laggan, when that loch stood at shelf 4. It is marked by a huge delta, forming a projecting buttress at the level of that shelf, and bulging far beyond the general side of the Laggan valley.

On examining the rocky knolls attentively in this ancient river-course, I found that the smooth faces were all towards Glen Glaster, and the rough faces in the opposite direction, affording proof, if such were needed, that the stream which flowed there had come from Glen Glaster.

A small rivulet trickles now among the rocks, infinitely too feeble to have produced the appearances.

It is now, therefore, established, not only that the whole of the 4 shelves of Lochaber are coincident with water-sheds respectively, but that a great body of water had filled Glen Glaster, and of course Glenroy, the outlet of which was down this ancient river-course to shelf 4 in Loch Laggan, which is at a lower level by 212 feet.

Whilst on this subject, I may mention farther, that I examined narrowly the interval of space between shelf 1 at the head of Glen Gluoy, and shelf 2 at the head of Glen Turret, where the last shelf is nearest to Glen Gluoy. This space also appeared to me to exhibit the features of an ancient river-course, though they are not so striking as those just described. The distance from the one shelf to the other, is about a mile. Where the Glen Gluoy shelf ends, rocky knolls rise above the moss, water-worn below the level of the shelf, but rough above that level. Their smooth faces are all towards Glen Gluoy. Near shelf 2, in Glen Turret, the rocks have evidently been excavated and cut into by some considerable stream; at present a very small burn runs in this rocky channel, quite incapable of producing the appearances.

The grandest exhibition of an ancient and deserted river-course is, however, at the head of Loch Laggan. The Pass of Mukkul is a channel, the bed and sides of which are entirely rock. It is, at its narrowest part, about 70 feet wide, the wall faces being on each side from 40 to 50 feet high. The rocks at the sides are evidently water-worn for about 30 feet up. To the eastward, this gorge expands into a broad channel of several hundred yards in width, divided in the middle by what has formerly been a rocky islet, against which the waters of this large river had chafed in issuing from the pass. For nearly a mile towards the east, the rocky banks continue on each side, but they gradually diverge, having between them a mossy flat sloping gently eastward. The smooth faces of the rocks within the probable reach of the river-waters, are all towards the west, where Loch Laggan is situated. The height of shelf 4 above the highest point of this deserted channel, is, by barometric measurement, about 21 feet, which affords, therefore, some probable estimate of the average depth of the river. I have only to add, that no stream whatever now occupies this water-course, except where, for a short part of it, the river Pattaig flows in a reverse direction into the head of Loch Laggan. This stream was, when I visited it last September, only about 18 inches deep and 30 feet wide, and must be quite inadequate to have formed the rocky banks on each side of it.

The ancient river-course now described is of much greater size than that at the head of Glen Glaster, just as the Glen Glaster river-course is of greater dimensions than those respectively at the head of Glen Gluoy and Glen Roy. The reason is obvious. The river at Mukkul had to discharge not merely the waters

which belonged to Glen Spean, but also those which flowed out from Glen Glaster, comprehending Glen Roy, Glen Collarig, and Glen Gluoy. The Glen Glaster river-course discharged the waters of Glen Collarig, Glen Gluoy, and Glen Roy, whilst the Glen Gluoy stream discharged only the waters of one lake. Mr DARWIN did not visit the Pass of Mukkul. If he had studied the appearances presented by it, and by those almost as strikingly exhibited at Glen Glaster, he would have found it impossible to deny that the waters which formed shelves 3 and 4 flowed down river-courses, and therefore could not be arms of the sea.

His proposition is, "that the waters of the sea, in the form of narrow arms or lochs, such as those now deeply penetrating the western coast, once entered and gradually retired from these several valleys;" and he adds, that after considering the "several and successive steps of the argument, the theory of the marine origin of the parallel roads of Lochaber, appears to me *demonstrated*." (P. 56.) I regret that Mr DARWIN should have expressed himself in these very decided and confident terms, especially as his survey was incomplete; for I venture to think, that it can be satisfactorily established, that the parallel roads of Lochaber were formed by fresh water lakes.

1. The first circumstance which I shall notice as fatal to Mr DARWIN's theory, is suggested by the fact last referred to, that the waters which formed the different shelves, must have *flowed out of the glens, and descended by river-courses to lower levels*. The waters which formed No. 1 shelf in Glen Gluoy descended nearly 29 feet by flowing into Glen Roy. The waters which formed No. 2 shelf in Glen Roy flowed in like manner into the valley of the Spey. The waters which formed No. 3 shelf were discharged over the head of Glen Glaster, down a slope of about 212 feet in vertical height, into Glen Spean. Lastly, the waters which formed shelf 4 in Glen Spean, issued out of Loch Laggan by the ancient river-course at Mukkul.

Now, any *one* of these cases is irreconcilable with the notion, that the shelves had been formed by arms of the sea. There is no such thing in nature as a river flowing out of an arm of the sea, to a lower level.

Mr DARWIN, as we have seen, admits that this coincidence of the shelves with water-sheds, must be in some way *connected* with their origin; and, accordingly, he endeavours to give an explanation of it consistently with his theory. He says that these water-sheds are *land straits*, with sea on each side of them, and that they consist of littoral deposits or accumulations of matter formed by the opposition of tides. This opinion, however, is altogether inconsistent with the actual circumstances of the case. In the first place, there is at these water-sheds, no accumulation of littoral deposits or detrital matter. They consist, generally, of bared rocks, forming sloping channels or water-courses. In the second place, there is no trace of water at the same level, on each side of these water-sheds. In the third place, when land straits are formed by the accumulation of matter from opposition of tides, it is not in situations like the heads of glens which narrow to a point, and

at that point are separated by a small neck of land,—it is where there is space for a considerable current on each side of the strait.

For these reasons I consider that Mr DARWIN's explanation of the coincidence of the shelves with the water-sheds before described, is quite inadmissible.

2. The second serious objection to Mr DARWIN's theory arises from the fact, that *the shelves* in the different glens *are not coincident in level*. If they had been formed by arms of the sea, as the land rose out of it, the sea should have formed lines in all the valleys which it entered, at precisely the same levels. But neither of the Glen Gluoy shelves is to be seen, in any of the other valleys. So also the No. 2, or highest shelf of Glen Roy, and the next lowest, or No. 3, do not occur in the lower part of that glen, or in the adjoining valleys of Glen Glaster, Glen Spean, and Glen Treig.

Mr DARWIN attempts to explain one, but one only, of these circumstances, viz., the difference of level between No. 1 and No. 2 shelves, by a theory of very questionable soundness. He says, that the tide in Glen Gluoy may have risen 20 feet higher at the head of the estuary, than at the head of Glen Turret. It would be necessary that it should rise 29 feet higher. But if this were the case, then the shelves, at all events, in Glen Gluoy, would not be horizontal, or nearly so;—they would have sloped upwards towards the head of Glen Gluoy, by 29 feet in the course of 6 or 7 miles,—the length of the glen. But this would be inconsistent with the great and well-established fact so characteristic of these Lochaber shelves; and moreover, though the beach-lines at the heads of the two glens might not be exactly coincident in level there, they ought, at all events, to be so at the mouths of the glens where the supposed arms of the sea joined the main body of the ocean,—which is not pretended.

This theory, however, would explain merely the non-appearance of shelf 1 in Glen Roy. The non-appearance of all the others is accounted for by Mr DARWIN, simply by supposing that something or other had prevented them being *marked* in the other glens.

In support of this view, Mr DARWIN refers to two intermediate shelves which are faintly traceable on Tombhran and elsewhere, in order to shew that the water did produce marks at some places, and not at others. But, from the faintness of those intermediate lines, it is manifest that the water had stood at their level for a much shorter period than at the levels of the principal shelves; and, therefore, no fair inference can be drawn from the former applicable to the latter.

3. These considerations suggest, however, a separate and even a more serious objection. Not only should the sea have made markings at the same levels in all the Glens of Lochaber, but it should have produced *similar appearances, and at the same levels respectively, on all the mountains of Scotland*, high enough for the purpose. Mr DARWIN says, “that it would be more proper to consider the *preservation* of these ancient beaches *as the anomaly*, and their obliteration from meteoric agency the ordinary course of nature.” (P. 60.) Supposing him right in

this, he ought to have shewn how circumstances caused that anomaly at Glen Roy and its adjoining valleys. But he has not shewn, and cannot shew, that the sides of the Glen Roy mountains, are in any respect different from those other highland mountains. Indeed, he has himself pointed out a similar beach line at Kilfinnin, in a glen towards Inverness. I take leave farther to doubt the soundness of Mr DARWIN's proposition, that the preservation of ancient beach lines is anomalous. The whole of Scotland, and I believe also of the British Islands, is begirt with lines of ancient sea beach.

4. The ancient sea beaches, now alluded to as existing along our coasts, present a very marked contrast with the Lochaber shelves. If these shelves had been formed by the sea, it will, I presume, be admitted that, considering their great altitude, they are of much older date than beach lines at a lower level. *If older, then they should be less perfect and entire.* But the contrary is the case. They are incomparably more perfect and entire than any of the lowest ancient sea terraces which occur along our coasts.

5. If the Lochaber roads were formed by the sea, the well-known actions of the tides, to which Mr DARWIN refers, would have *precluded* the formation of them *along lines absolutely horizontal.*

Mr DARWIN refers to a case in South America, where, in 18 miles, the tidal wave rises at one place 20 feet higher than another in the same estuary. Nearer home, in the Bristol Channel, the sea rises at its head about 50 feet higher than at its mouth.

The tide at Blackwall rises 12 feet higher than at Yarmouth. In the Firth of Tay, the tide rises at Perth 18 inches above the level at Newburgh. The tide at Alloa is said to rise 2 feet 9 inches above its level at Leith. At Glasgow, the tide rises 10 or 11 inches above its level at Greenock. On the Dee, the level of high water is, at Chester, 8 inches above what it is at Flint, near the mouth of the river, a distance of 11 miles.

On this principle, the beaches of Lochaber, if formed by arms of the sea, ought all gradually to rise to the head of the Glens—narrowing, as these glens do, towards the head. But this is negated by the fact.

6. On more narrowly considering the effect of tidal action, it will readily occur, that the beaches formed by the *sea* must be materially different from those of a *lake*, in which there is no movement of the water at the sides, except such as is caused by winds common to both. In the case of the sea, there is not only a vertical rise and fall of water (which, on the west coast of Scotland, is from 8 to 16 feet) twice in the 24 hours, but also a good deal of lateral current alternately in opposite directions. Hence the *sea*, whilst it will eat into the land more rapidly than a *lake*, will also spread out more completely the detritus washed down into it. In a lake, on the other hand, which has no movements of water either vertical or lateral, the detritus deposited on the sides of a valley occupied by it, will

be scarcely if at all removed, and will thus form projecting buttresses nearly flat in their upper surfaces, and presenting steep escarpments towards the lake.

Now, applying these two principles of tidal action to the shelves of Lochaber, we seek in vain for any actual *indentation* into the sides of the hills. The shelves consist entirely of *buttresses* which stand out from the sides of the mountains; and these buttresses, so far from sloping at an angle little less steep than that of the sides of the mountains (which would be the case with the sea), form flats or terraces which deviate in general very slightly from the horizontal.

7. If the shelves were formed by the action of the sea, *they should be most distinct at places where the hill sides had been most exposed.*

Thus, on the north and north-west sides of Craig Dhu, and on the west side of Bohuntine, where there must, on Mr DARWIN'S theory, have been an open expanse of ocean, the shelves should have been most distinct. But at *these places, the three highest shelves are entirely absent*; the fourth alone is visible, though, being the lowest, it must have been less exposed. It is quite anomalous, on the marine theory, that the shelves should not have been formed where the force of waves and of tidal currents must have been greatest, and that they should have been most distinctly formed in the higher and more sheltered parts of Glen Roy.

The hills at the mouth of Glen Roy seem rather to indicate that the highest shelves had not been formed on them,—the very reverse of what might have been anticipated if Mr DARWIN'S views are sound. If they had been formed, they would not have been obliterated, as is manifest from the perfect preservation of shelf 4 on Craig Dhu and Bohuntine.

8. Having stated these objections to the theory of Mr DARWIN, I proceed to consider his objections to the theory, that the shelves were formed by lakes.

These objections resolve entirely into the difficulty of explaining the disappearance of the barriers, which must have dammed back the water in the valleys. But it would be no good reason for rejecting an explanation founded on the existence of barriers, even though we could not very clearly account for the disappearance of them, provided that there is direct and conclusive evidence that such barriers existed. Now, I conceive that there is such evidence furnished by the considerations before referred to.

Let us examine, however, the alleged difficulty of explaining, how the waters could have been dammed up in the valleys to the height of the several shelves.

Shelf 2 is distinctly marked on both sides of Glen Roy, down to a certain point,—and also on both sides of Glen Collarig, down to a certain point. At this period, the water flowed from the east end of Glen Roy into the valley of the Spey. Something must have existed, therefore, in both glens at the points above referred to, to prevent the extension of the shelf westward.

Shelf 3, in both glens, extends a little more to the west than shelf 2. We

have seen that, whilst Glen Glaster is exempt from shelf 2, it is well marked on both sides by shelf 3.

To explain these facts, I assume that there was a blockage of some sort, in Glen Roy, which filled the lower part of the valley up to the level of shelf 2, and which blockage extended a little farther east than the mouth of Glen Glaster. I assume also a similar blockage in Glen Collarig, which filled the lower part of the valley, and as far eastward as the place where shelf 2 stops in that glen. This blockage may have been gravel, clay, or any other detrital matter.

Such is the supposed state of things, whilst the waters stood at shelf 2 in Glen Roy; at which period, it will be remembered, they were discharged to the eastward.

Former writers have assumed, that when the waters sunk from shelf 2, the amount of sinking must have been 82 feet, the distance of shelf 3 below shelf 2; and that this sinking had been one act, caused by an earthquake, or other violent operation, which all at once lowered the barrier by that number of feet. But this is a mistake. MACCULLOCH takes notice of a shelf faintly marked on Tomblhran hill, between shelf 2 and shelf 3, though he expresses afterwards some uncertainty about it. In fact, there are two intermediate shelves visible there; and they are also discernible, at precisely the same level on Ben Erin, and also more distinctly near Achavaddy, on the south side of Glen Roy; the one being about 14 feet below shelf 2, and the other about 36 feet lower down.* These two intermediate shelves clearly indicate, that the water which filled the valley, did not all at once sink from shelf 2 to shelf 3. They prove that the water first sunk down 14 feet, and was stationary at this level for some time; that it then sunk down other 36 feet, and continued at this level for some time; and that it again sunk other 32 feet, at which level it remained for a much longer period, till it formed shelf 3.

It is evident, from these facts, that the lowering of the barrier (of whatever material composed) which confined the water in Glen Roy, was a process of a more gradual and ordinary description than what former writers, and especially Mr DARWIN, suppose. It is plain, also, that the barrier which kept in the waters was less rapidly worn down, when they stood at shelves 2 and 3, than at either of the intermediate levels. We see that at shelves 2 and 3 the waters flowed over rocky ledges, in the one case into Spey valley, in the other case by Glen Glaster. Is it not fair from this to infer, that at the intermediate shelves, the water flowed over a blockage of such a nature as was capable of being more easily worn down and obliterated, such as detrital matter? It is, at all events, obvious, that when

* There are hummocks or knolls of stratified gravel and sand in Glen Glaster, the tops of which are all about 36 feet above shelf 3. It is probable that they were deposited when the lake stood at one or other of the intermediate points last mentioned.

the water sunk 14 feet, the discharge must have ceased at the east end ; and that it henceforward would go on at the west end, probably near the mouth of Glen Glaster. At every other place, the rocky mountain sides rise so high, as to preclude the possibility of overflow or attrition.

Keeping these principles in view, let us suppose that the detrital matter which blocked up the lower parts of Glen Roy extended a very little to the east of the mouth of Glen Glaster. How easy it is to suppose that this detritus was scooped away, so as to allow of the recession of the waters westward, and of their flowing round the east jaw of Glen Glaster, and on towards the head of that glen, from which they would descend to Glen Spean ? For this purpose, it is not necessary to suppose, that there was any *lowering* of the supposed barrier in level, even by a single foot. All that is required is the scooping or wearing away of the detritus, so as to allow of the extension of the lake a little to the westward ;—a few yards would be sufficient. As the discharge at this first sinking, must have been at the west end, it is fair to infer that the wearing away of detritus took place there ; and when once a flow of water was established through detrital matter, the process of removal would go on rapidly, so as to allow of repeated sinkings of the lake, till it reached the water shed at the head of Glen Glaster, the rocky nature of which would for a time stop any farther sinking, and thus allow of the formation of shelf 3.

According to the foregoing views, we see how the waters would, by successive steps, sink from shelf 2 to shelf 3, and, after entering Glen Glaster, form a marking on both of its sides. We see, also, that the same removal of detritus which allowed the formation of shelf 3 in that glen, would allow also the extension of it on Bohantine Hill, beyond the point where shelf 2 terminates.

Whilst this process of attrition was going on in Glen Roy, there need have been no contemporaneous change in the blockage of Glen Collarig. But there also, at some time or other, a similar scooping out of detritus must have taken place, to allow of the extension of shelf 3 beyond the point where shelf 2 terminates.

Nor is it difficult to conceive, how this removal of detritus was effected. Thus, in Glen Collarig, there are, on both sides of the glen, burns of considerable size and power (from the steepness of their channels) which flowed into the lake. There are three of them, which now descend in that part of the glen marked by shelves 2 and 3. If the detritus which formed the blockage in the lower part of the valley consisted of the same loose sand and gravel which now abounds there, forming cliffs from 70 to 80 feet high, nothing is more easy or natural than the scooping of it out, by such means.

The same observations apply to the blockage in Glen Roy, which, to prevent the waters when at shelf 2 flowing into Glen Glaster, must have been near the mouth of Glen Collarig, called Gap in the maps, out of which, from the number of streams in it, a considerable current had flowed.

So far with regard to the first depression to shelf 3, at which period I suppose the Collarig blockage to be still existing (scooped out a little towards the west), and the blockage in Glen Roy to have been, by a similar process, removed below the mouth of Glen Glaster. The next well marked shelf is No. 4, which is seen on Craig Dhu and Bohuntine, and on both sides of Glen Collarig, and which infers the necessity of removing the blockage entirely from both Glen Roy and Collarig.

This may have been, as in the case of the previous depression, a gradual operation. There is no improbability whatever in the ultimate removal by rivers and burns, of a blockage of the nature supposed. There flows into Glen Roy, from Bohuntine hill, and at or near the very place where the blockage must have existed, the Tundrun Burn, the sides of which shew mica-slate rocks cut through by it to the depth of about 70 feet, and detrital matter above these rocks cut through to the depth of 130 feet. If, since the drainage of the lake, it has thus cut through and removed blockage to the depth of 200 feet, of which one-third is solid rock, this rivulet must have had nearly equal power to wash away the more superficial blockage which existed at this place previously to that event.

The same observations apply to the detrital matter in Glen Collarig, which could easily be carried away by the numerous mountain torrents flowing into that glen.

The following is the manner in which Mr DARWIN alleges that the two depressions must have taken place, according to the lake theory. He says, that there are two barriers, one in Glen Collarig, and the other in Glen Roy: "Let one of the two barriers, we will say the smaller one in Glen Collarig, *give way from the effects of an earthquake*, or other cause, the lake will now stand at the level of the middle shelf, the barriers having *given way 82 feet vertically*. Again let it burst, and *this time rather more than 212 feet vertical must be swept away*. Let all this have taken place, but still a barrier nearly a mile long and 800 feet in height is left standing across the mouth of the Roy. Must we suppose that *each time* the barrier in *Glen Collarig* failed, the one in *Glen Roy* gave way the same number of feet, through some strange coincidence?" It is plain, from this representation, that Mr DARWIN had not in his view, the more simple and gradual process of removal which I have ventured to suggest. It is not in the least necessary to imagine, that there was any sudden sweeping away of barriers of the magnitude supposed; and which would certainly imply the existence and operation of some stupendous agent; but the effect of which would, as Mr DARWIN truly says, have also probably obliterated the shelves. The process which I have suggested, implies the continuous working of ordinary and natural agents,—agents which are now seen at this very place, producing results similar to those required.

Mr DARWIN says, that the barrier across the Roy must have been 800 feet high. This is on the assumption, that the valley of the Roy was then of its present depth and form. But is there to be no allowance made, for the removal by the

river Roy of detritus from the valley? It is manifest, from many appearances along its sides, that the river Roy has cut down at least 200 feet below what was the original bottom (whether of lake or estuary), formed when the waters stood at shelf 4; so that the height of the supposed barrier to retain the waters at shelf 2 would not exceed 600 feet above the bottom of the valley, and might be much less, if the valley were more filled up. Mr DARWIN considers it probable (p. 53), that the buttresses existing on the sides of Glen Roy indicate, that the valley, upwards from Bridge of Roy, had been filled with detrital matter to the very level of shelf 4; in which case the blockage or barrier requisite to form a lake at the level of shelf 2, would have been only about 300 feet above the bottom of the valley. My belief, however, is, that the whole not only of the lower part of Glen Roy, but also of the district about Unachan, High Bridge, and Fort-William, was blocked up with detrital matter, which, in the course of time was washed away by rivers; and that, when the blockage of Glen Roy was removed, the depressed waters standing at shelf 4 were dammed back by detrital accumulations near Unachan, so as to force a discharge by the Pass of Mukkul. This 4th, or lowest shelf, seems to me to stretch much farther to the north, on both sides of the Spean, than former observers have noticed. On the hills flanking the east side, this shelf can be traced to within nearly a mile of Spean Bridge. On the opposite side of the valley, it can be traced to within 6 or 7 miles of Fort-William. The width of the valley where this shelf on both sides ceases to be visible is about 4 miles. Across the mouth of this valley, a little beyond a line joining the extreme visible points of shelf 4, lies the high and elongated hill of Tomnempearaichin, the top of which I found, by the level, to be only 50 or 60 feet below shelf 4; and there is no great difficulty in imagining that the whole of this district, as far as Fort-William, where the enclosing hills are greatly higher, was filled by detritus. There are, even now, detrital remnants of enormous size, of which the well-known Hill of Tomnahurich at Inverness (about 180 feet high and half a mile long), and a hill to the west of it (240 feet high), are specimens indicating the prodigious accumulations once existing in the great glen.

To this point I shall revert. But, in the mean time, taking for granted that such detritus did fill the lower parts of the valleys, it is easy to understand how it should have dammed up the waters into lakes, and how, by a gradual and long-continued process of wearing down, this detrital blockage should have been lowered to the requisite extent.

I have endeavoured to explain the damming back and the depressing of the lakes to their successive levels, without imagining that the level of the sea was then different from what it is at present. If the sea stood at a higher level, then the difficulties of the explanation become less; because the valleys must then have been previously less excavated than they now are, by the operation of rivers. There are good reasons for believing, that since the period of the deposit of the

boulder-clay in Scotland, the sea has stood at least 1000 feet higher on the land than at present. Of course, it must have been after the land rose out of the sea to some extent, that the Lochaber shelves could have been formed by lakes; but the lowest of these might have existed when the sea stood 900 feet above its present level, in which case the depth of detrital matter required to dam up the valleys would be comparatively small.

I have attempted to explain how the valleys of Glen Roy, Glen Collarig, and Glen Spean, were blocked up. There still remains Glen Gluoy, which, as before mentioned, contains two shelves, one of which is about 29 feet above the highest of Glen Roy. Glen Gluoy being unconnected with the other valleys, requires a separate blockage. There would be no great difficulty in imagining the existence of detrital blockage in this glen, at the place where its shelves terminate towards the west, as it is generally, throughout its whole course, exceedingly narrow; and being unconnected with Glen Roy (though MACCULLOCH states the reverse), its blockage may have been worn down at periods, and in a way, independently of Glen Roy and Glen Collarig.

Before, however, forming a very decided opinion as to the position of the blockage applicable to Glen Gluoy, I should like to examine more particularly than I was able to do, some of the other Glens which open into the Caledonian valley on both sides, with the view of ascertaining whether they contain traces of horizontal shelves about the same height. Mr DARWIN takes notice of one in the valley of Kilfinnin,* about 10 miles to the eastward, and which he says is (by his barometric observations) about 40 feet above the highest shelf in Glen Roy; in which case it would be only 10 or 11 feet above that in Glen Gluoy, a difference quite within the limits of error.

I have observed several places along the Caledonian Canal, where there are traces of one or more horizontal terraces, at a height of from 650 to 690 feet above the sea. From these considerations, I infer the possibility of there having been a blockage which applied not merely to Glen Gluoy, but to other glens opening into the great Caledonian valley; and it would, therefore, be most important, that future observers should turn their attention to the adjoining districts.

My explanation of the Lochaber shelves depends entirely on the accuracy of the supposition, that the valleys were, in the lower parts of them, filled up with detrital matter, capable of being gradually worn down and washed away. This supposition is not only not improbable on general principles, but is verified to a great extent by the remains of such detrital matter at and above the heights required for it. Thus, in Glen Collarig, there are to be seen, near the east end,

* It is to be regretted that Mr DARWIN, when he visited Lochaber, was not provided with a spirit-level. His statement as to the horizontality of this shelf at Kilfinnin, depends entirely on ocular inspection and barometric measurements.

and within about half a mile of the place where the blockage must have existed, enormous heaps of boulder-clay, gravel, and sand. These detrital deposits must have existed in Glen Collarig before the shelves were formed, because shelves 2 and 3 are seen distinctly indented upon these deposits; and I was particularly struck with the fact, that these deposits reach to a height of more than 100 feet above shelf 2. Here is proof, that in Glen Collarig, before the formation of the lake which filled it, there was detrital matter of sufficient depth and consistency to have retained water at the required height. At the place where shelf 2 terminates in this glen, the valley, even at present, is only about 236 feet deep, and 300 yards wide, so that the depth of detrital matter does not exceed the limits of probability—nay, is exemplified by the occurrence of much larger accumulations of detritus in all parts of the Highlands.

It is here proper to explain, that there are in these valleys, as elsewhere in Scotland, two distinct sorts of superficial deposits,—the one consisting of the well-known boulder-clay, and the other of ordinary gravel and sand. This boulder-clay exhibits the same general characters, which it commonly possesses elsewhere; it is unstratified, exceedingly obdurate, of a dark-bluish colour, and filled with water-worn boulders. This boulder-clay I found at the following places;—Spean Bridge, where it is covered by sand; Bohuntine Hill, where it is covered with laminated clay, sloping to the centre of the valley, and about 250 feet below shelf 4; Bohina, on the south side of Glen Roy; Inverlair Bridge, near Loch Laggan; Glen Glaster (on the west side of the valley), from 50 to 80 feet *above* shelf 3; Glen Collarig (near the gap), where it rises *above* shelf 2; Glen Gluoy, as seen at the watershed between it and Glen Roy, and *on a level with shelf 1*. The deposit occurs also at Clenichan, at the river Roy, where the mica-slate rocks, through which the river now runs, are covered immediately by boulder-clay,—the boulder-clay being here covered by deposits of irregularly stratified beds of gravel and sand, from 150 to 200 feet thick. At this place, I observed among the boulders in the hill, granites (with red and grey varieties), old conglomerate, and red porphyry.—rocks, all of which must have come from a distance.

From the fact that this boulder-clay occupies alike the highest and lowest parts of the glens; and, more especially, that in several places it is seen distinctly covered over by laminated clay as well as by stratified gravel and sand, it may be inferred that the boulder-clay, with its imbedded blocks, was deposited, certainly not after the drainage of the lakes, but either before the valleys were occupied with water, or during that period.

In regard to gravel and sand, I do not remember having, in Glen Roy or its contiguous valleys, observed any considerable beds of it, so high up as the boulder-clay. But at lower levels, there are everywhere enormous cliffs of it to be seen, several of which I measured, and found to exceed 180 feet in height. These cliffs are formed out of the ancient bottom of the lake or estuary which filled the valleys, and are

composed of materials washed down from higher levels. The adjoining mountains of the district afford ample evidence, that gravel as well as boulder-clay had been, by some cause or other, brought and deposited over all this country, filling the valleys to heights exceeding the highest of the Glen Roy shelves. Thus, on the turnpike road between Tyndrum and Inverournan, near the summit level between the two valleys, which I estimated to be about 1030 feet above the sea, there is great abundance of sand and gravel. On the Black Mount, about 4 miles north of Inverournan, and at a height of 1300 feet above the sea, there is an immense accumulation of gravel and boulders, particularly on the south side of the summit. In the high ground north of Dalwhinnie, which I estimated at 1200 feet above the sea, there are great heaps of gravel, forming mounds and ridges. These facts, taken in connection with the undoubted fact, that detrital matter has been spread over the greater part of Scotland, to a height of at least 1500 feet above the sea, pretty clearly indicate, that detrital matter not only may have been, but actually was spread over the Lochaber district, and filled its several valleys, to the height of at least the highest of the Glen Roy shelves, thus affording ample blockage for its lakes.

I may mention that there are, in this part of the Highlands, several lakes of small size, at very high levels, the existence of which renders the lake theory of the Glen Roy shelves less improbable than to some it may appear. Thus, at the well-known pass of Rest-and-be-Thankful, there is a small lake, which is about 800 feet above the sea, and there are traces of its having stood formerly from 40 to 50 feet higher. To the south and west of Loch Treig about 3 miles, there are two considerable lakes, one called the Lake of Corry, and the other called Benofflap, which appear, from the accounts received of them, to be about 1200 to 1300 feet above the sea. There are several also on the Black Mount, at about the same high level.

Before concluding what I have to say regarding the parallel roads of Lochaber, I may briefly notice the theory, that the lakes which filled them may have been confined by glaciers, or by the moraines of glaciers.

This was one of the districts which, in the opinion of AGASSIZ and BUCKLAND, afforded undeniable proofs of the existence of glaciers. The former published a paper* on the subject, in which he says: "When I visited the parallel roads of Glen Roy with Dr BUCKLAND, we were convinced that the glacial theory alone satisfies all the exigencies of the phenomenon; and as this locality is the best known, I may limit myself to this example for the explanation of all others."

M. AGASSIZ, in the paper now alluded to, explains the grounds on which his theory rests; and it is accompanied by a plan of the locality.

It appears to me, (1.) That the facts on which M. AGASSIZ rests his theory

* Ed. Phil. Journal, vol. xxxiii., p. 236.

are incorrect. (2.) That, assuming as true the facts stated by him, they still afford no evidence that glaciers existed in the Lochaber valleys.

(1.) There are three main facts relied on by M. AGASSIZ. He states, *First*, That in Glen Roy, and in that part of Glen Spean, between Bridge of Roy and Loch Treig, there are 3 shelves visible; *Secondly*, That these shelves all terminate, on both sides of the valley at or near the Bridge of Roy; *Third*, That the bottom of Glen Spean, in front of Loch Treig, is not only polished with that polish characteristic of glaciers, but is, moreover, scratched transversely,—that is to say, at right angles to the direction of the valley, by a cause which evidently proceeded from Loch Treig.

To explain these appearances, it is suggested, that “the supposition of a great glacier descending from Ben Nevis, and shutting up the valley of the Spean, *by resting on Moeldhu*, which is opposite, combined with the influence of a glacier from Loch Treig, and which would bar the valley a second time at that height, *would explain all the facts.*”

These facts, for an explanation of which this theory was invented, appear to me not to have been accurately observed. In the first place, the *three* shelves do not occupy, as M. AGASSIZ asserts, “all the sinuosities of the lower part of Glen Spean, and of the whole of Glen Roy.” It is only the lowest of the three shelves, which occurs in Glen Spean and in the lower part of Glen Roy. The two uppermost shelves stop short of the mouth of Glen Roy, by about 2 miles; so that, if the Lake in Glen Roy was dammed back by a terminal moraine, that moraine could not have rested on Moeldhu, at the foot of Glen Roy; but must have been pushed up that valley, before the Ben Nevis glacier, 2 miles farther,—an operation which the levels, distance, and direction of the valley would have rendered impossible.

In the second place, the shelves do not, as M. AGASSIZ says, “terminate at the same point,”—viz., at Moeldhu, where he supposes the terminal moraine of the Nevis glacier to have been. The two uppermost shelves (as just stated) do not come within two miles of this point; and the lowermost shelf, instead of *terminating* there, runs, as formerly explained, several miles northwards, on both sides of the valley, towards Unachan, where they are 4 miles apart. It is scarcely necessary to say, that a moraine in this low district, which is not connected with any Ben Nevis valley, and considering its required height and length, is inconceivable.

In the third place, as to the existence of transverse scratches on the rocks in Glen Spean, which are said to indicate the movement of some body from Loch Treig, I could see no such scratches, though I twice surveyed the ground, and narrowly inspected the rocks, especially at the outlet from Loch Treig. Indeed, the supposition that any glacier flowed out of Loch Treig seems to be almost excluded by the fact, that a shelf, perfectly horizontal, exists on both sides of the

narrow outlet from Loch Treig, and continuously into Glen Spean. Such a shelf could not have been formed, and would have been obliterated by any glacier moving out of Loch Treig.

(2.) But assuming all these facts to be as M. AGASSIZ states them, do they present unequivocal proofs of the movement of glaciers, and the formation of moraines? Scratches on polished rocks, may be made by various causes; and if a moraine existed on Moeldhu, surely some trace of it, or of the great blocks which generally accompany moraines, would have been particularly observable there;—whereas there is scarcely a block or a patch of gravel to be seen in that part of the valley.

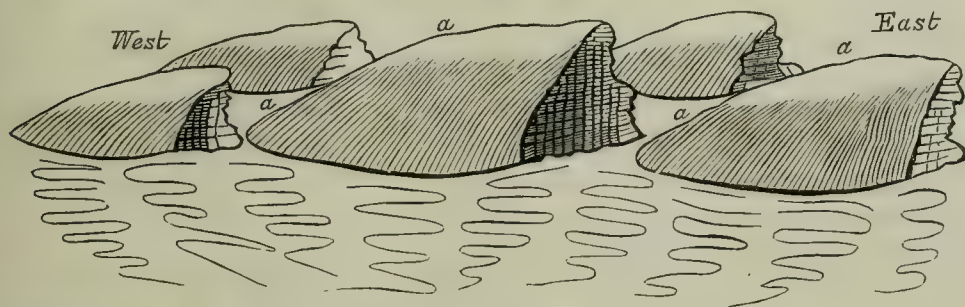
Farther, I would observe, that the valley supposed to have been the birth-place of the glacier, which produced this Moeldhu Moraine, is about two miles distant from Moeldhu, with an undulating country between them, which is most unlikely to have formed the channel or bed of a glacier. Dr BUCKLAND and M. AGASSIZ speak of this glen, as connected with Ben Nevis. But here, again, there is apparently some mistake. The valley in question is Larich Leachich, and runs up, not in a NW. direction towards Ben Nevis, but in a SW. direction towards the head of Loch Treig. It is an extremely short glen, and rises to no great height.

Finally, supposing, that if, in spite of all these objections, it were allowed that a glacier had moved down this little valley, and across the very uneven country to Moeldhu, so as to block up Glen Roy and Glen Spean, it would still remain to explain the blockage of Glen Gluoy, which, by no possibility, could be accounted for by a moraine at or near Moeldhu.

That there are certain appearances in the valleys of Lochaber, which must have been produced by attrition of some kind, I am free to admit. Water, accompanied by gravel and other detritus, appears, however, to have been the agent, and not ice. At the Monessie Falls, the valley is compressed to a narrow gorge, and the rocks forming the east side, present evident marks of attrition on a large scale, the rough faces of the rock being all down the valley. The rocks are here covered by sand and gravel, which indicate the flowing of water and of drift at that height, when these rocks were worn down. In like manner, at the outlet of Loch Treig, there are immense expanses of rock, all smoothed and rounded on the sides facing the SW. or WSW. by compass.* These smoothed rock-faces prevail to a height of about 786 feet above the lake, and 1680 feet above the sea, above which level they are no longer visible. There are many boulders lying on these smoothed surfaces, all of rounded forms. That these boulders have come from

* The general line or axis of the lake is north and south by compass, the upper part being towards the south, so that the motion of a glacier down this valley would have smoothed all the *south* faces of the rocks. It is also important to remark, that, on the west side of the lake, the rocks facing the lake are, as compared with those on the other side, exceedingly rough, shewing still more clearly that the smoothing agent had crossed the valley of Loch Treig, in a direction not parallel with its longer axis, but obliquely to it.

the west, is evident from the nature of them; several of a pink coloured felspar, having been traced by me to a dyke of the same peculiar rock a few hundred yards to the west, from which they had evidently been derived. Another circumstance proved this still more strikingly. In one place, a few hundred feet above Loch Treig, I observed a series of rocky knolls, in an east and west line. The parts of these knolls which were smoothed and worn down were uniformly to the west, whilst their rough faces were all to the east, thus—



It was clear, on an inspection of these knolls, that they had been worn down on their west sides; and the smoothed sides *a* were so close to the knolls respectively to the west of them, that nothing except some fluid, charged, it may have been, with drift, could have possibly reached and acted on them.

This last point was still more palpable, in several places, where there were narrow smooth-sided troughs, more or less steep, on the sides of hills. These troughs had apparently been natural fissures in the rocks, which had been smoothed by the long-continued action of water; for the notion that ice could have entered and rubbed them, was entirely precluded by their narrowness, situation, direction, and other circumstances.

M. AGASSIZ, in the paper before alluded to, says that he will never forget the impression he experienced “at the sight of the terraced mounds of blocks which occur at the mouth of the valley of Loch Treig, where it joins Glen Spean. It seemed to me (he adds) as if I were looking at the numerous moraines of the neighbourhood of Tines, in the valley of Chamounix.” These terraces of blocks, thus likened to moraines, are, I presume, the accumulations of blocks on the lowermost horizontal shelf, which is very conspicuous at the entrance to Loch Treig on both sides of the valley. On this shelf there are multitudes of blocks, just as in many other parts of the valleys, where this shelf and the others occur. But this fact is perfectly consistent with the theory, that these shelves were formed by water, and, indeed, can be explained on no other, when it is considered that they form at Loch Treig, as at every other place, a line absolutely horizontal,—a quality which, I presume, no moraine ever possesses.

The only place where I observed an accumulation of blocks, at all resembling a moraine, is on the east side of Glen Spean, near a place called the Rough Burn.

about three or four miles to the north of Loch Laggan. The accumulation is enormous. Blocks are piled over each other, to such a height as to render the general surface of the moor, over a wide extent, quite undistinguishable. This accumulation occurs not at the mouth of any valley. On the contrary, the hills near these blocks on the east side, are not much furrowed even by mountain torrents, and present a somewhat steep and high wall face to the west. On looking round for any possible explanation of the occurrence in this spot of so unusual a quantity of boulders, consisting almost entirely of grey granites, whilst the rocks on which they lie are different, I could not help noticing that the valley on the opposite or west side presented an opening or depression, though at the distance of 2 miles. This opening is the outlet of Loch Treig, and bearing about WSW. by compass. The appearance of the locality at once suggested the probability that the blocks had in some way issued through this opening, and had been transported across the valley to their present situation, where their farther progress was arrested by the lofty hills forming here the east side of Glen Spean.

I have already stated reasons for thinking that no glacier issued from Loch Treig. The only alternative seems to be the agency of water.

I proceed now to shew that the lake theory of the Lochaber shelves, and the principles on which I have endeavoured to account for the formation of lakes, and the eventual depression and drainage of them, are not inconsistent with any established geological truths,—but, on the contrary, receive support from collateral considerations.

1. The first circumstance which I shall notice, *is the occurrence of Parallel Roads in other valleys similar to those of Lochaber*, the formation of which can be attributed to no other cause than lakes.

I have the less hesitation in availing myself of this argument, when I find Mr DARWIN adverting to traces of shelves at Kilfinnin, and in the valley of the Spey, in support of his theory.

But if Mr DARWIN'S views are sound, traces of shelves should not be confined to the two localities just mentioned; they should be visible in other parts of the country of equal height as the Lochaber mountains.

On the other hand, if it should appear that there are in many valleys, distinct beach lines, all horizontal, and presenting no uniformity of height above the sea, the argument against a sea theory will be strengthened, whilst a strong analogy will arise to favour the lake theory,—if these beach lines, precisely similar in all essential features to those of Lochaber, can, from their inland situation, and other circumstances, be clearly shewn to have been produced by the waters of lakes.

I proceed therefore to mention a few localities out of many, where phenomena similar to those of Glen Roy are observable.

(1.) At Inverournan (about 40 miles SW. of Lochaber) there is a lake called

Loch Tulla, about 3 miles in length, and 1 in breadth. A stream enters from its east and west ends. Its surplus waters are discharged from its south side, by the river Urchay.

Two years ago, I discovered all round this lake indications of three levels at which its waters had stood, the lowest being about $183\frac{1}{2}$ feet, the second 277 feet, and the highest 474 feet, above their present level.* Loch Tulla I roughly estimated at 540 feet above the sea. This lake, therefore, extending originally to about 6 miles in length and half a mile in breadth, had sunk 197 feet,—at which level it had stood long enough to form the second shelf; it next sunk $93\frac{1}{2}$ feet,—when the third shelf was formed; after which it sunk $183\frac{1}{2}$ feet,—viz., to the present level of the lake.

It is unnecessary for me to enter into the proofs, that what I am now describing are really beach lines. Their perfect horizontality, which I ascertained by a spirit-level, looking at them from 12 or 15 different places along the banks of the lake,—their general conformity in sweeping round headlands, and retiring into valleys or burn-courses,—and the extent of flat surface at the levels of the different shelves, afford convincing and irrefragable proofs.

The difficulty here, as in other similar cases, is to discover, what could have dammed up the lake so much above its present level. The blockage, whatever it was, must have existed somewhere in the valley, through which the river Urchay flows. The country, on all other sides of Loch Tulla, rises much higher than 500 feet above its present level. The two lowest shelves are traceable for some distance down the valley of the Urchay,—the middle shelf for about half a mile, and the lowest considerably farther. My notion is, that this valley had been formerly filled with a great accumulation of gravel and diluvial debris, which was gradually eat away and lowered by the stream which issued from the loch. Accordingly, there exist still, at and near Urchay Bridge, great heaps of unstratified gravel, which clearly present only a remnant of what must have formerly existed. The valley at this place, is a quarter of a mile wide; and its sides rise far above the required level.

(2.) In the valley, at the head of which Tyndrum is situated, there are very manifest indications of the beaches of an ancient lake, although the valley is now occupied by only an insignificant stream. At Strathfillan church, the lowest terrace is about 50 or 60 feet above the stream, and may be traced continuously for at least a mile down the valley. The stream has cut through this old lake bottom, exhibiting beds of gravel, sand, and clay, which have been deposited and arranged by the water. About 237 feet above this flat, there are, on the sides of the hills on both sides of the valley, traces of a horizontal shelf, which can be distinctly followed with the spirit-level from above Tyndrum village, down the valley by Auchreach farm-houses, Enich farm-houses, and as far as Crianlarich

* These measurements were made by a mountain barometer, checked by the sympiesometer.

toll. At several places, boulders appear to have accumulated on this higher shelf. Tyndrum is about 740 feet above the sea.

(3.) Along the margin of Loch Awe, and particularly near Dalmally, there is a flat or terrace about 40 feet above the present level of the lake; and which manifestly indicates a subsidence of its waters to that depth.

(4.) Along the margin of Loch Lubnaig, in like manner, there is a flat or terrace about 40 feet above the lake, and which is very visible on both sides. Here as well as in the former case, the flat runs back from near the margin of the lake to the mountains forming one side of the valley; and the steep sides of which, contrast most significantly with the almost horizontal flatness of the ancient and exposed bottom.

At Loch Lubnaig, the flat can be traced for a considerable way on both sides of the valley, beyond the point where the lake now discharges itself, and, indeed, almost as far as Leny. At this place, as well as at Callendar, there exist indications of enormous quantities of gravel, which, before being cut down and carried away by rivers, afforded ample means of blocking up the waters of Loch Lubnaig to a higher level. The quantity of gravel which formerly existed hereabouts, may be inferred from the existence of the following remnants.

About $\frac{1}{4}$ mile west of Callendar, there is a ridge of gravel and sand about 100 yards long, and from 40 to 50 feet high. Near it, there is a conical mound of the same materials, and about the same height, bearing a thriving plantation. The ridge of gravel to the east of Callendar, designated in guide-books as the Roman Camp, is merely a remnant of the ancient gravel-bed with which the whole valley was filled; and when it contained a lake, of which there are abundant indications, it is probable, that, when Loch Lubnaig stood 40 feet above its present level, its waters were discharged into a lower lake, of which the eastern margin may be seen near the Lodge of Gart-House. Ultimately the gravel heaps which held in this Callendar lake on the east, had been cut through, so as to allow of its drainage; and, accordingly, there are, on each side of the river Teith at this place, gravel banks and cliffs from 70 to 80 feet high.

After the Callendar lake was drained, the waters which flowed out of Loch Lubnaig would acquire fresh cutting power, and would rapidly eat away the barrier which dammed back the lake to the higher level before referred to.

Callendar is about 270 feet above the sea.

(5.) In the valley in which the town of Huntly stands, there are two terraces, the one about 32 feet above the other, which are very clearly the beaches of a lake, which has sunk from the one to the other, and latterly been drained off.

(6.) A few miles south of Inverury, there are distinct traces of a lake which formerly filled the valley. The burgh of Kintore has been built in the ancient bottom of the lake. There are two well-marked beach-lines round the whole valley;

the one about 78 feet, and the other 50 feet, above the channel of the united streams of Don and Urie, which flow through the centre of the valley. The ancient bottom of the lake has been cut up by rivulets at the sides of the valley into separate fragments, some of them of so unusual a form as to have suggested a notion that they are artificial; and, accordingly, in the guide-books, and even in the recent statistical accounts of the parish, they are so described. Two of these alleged remains of antiquity are known by the names of Bass and Konin Hillock; and are variously conjectured to have been formed for sepulchral or judicial purposes. A similar mistake has been made with the hills of Dunipace, near Falkirk, which are represented by historians as formed to celebrate and record a peace between the Romans and the natives of Scotland. They are detrital remnants fashioned into conical shapes by the action of streams.

(7.) In the valley of the Leader (Berwickshire), there will be found terraces on the hill sides, which clearly shew the action of water. Three very distinct markings of this nature are traceable near Dodds' Mill, at Hounslow, at Carfrae Mill, and at Annfield near Channelkirk. The terraces at these different places, judging by the sympiesometer, seem to be all very nearly on a level; and if, on a more minute survey, they really prove to be so, it would follow, that the whole of Lauderdale had formerly been one vast lake, with a blockage at or near Chappel. The height of these shelves is about 800 feet above the sea.

It is scarcely necessary to advert to the inland situation, and other circumstances characteristic of the various beach-lines now mentioned, to shew that they could not have been formed by the sea, but must have been produced by lakes which filled the valleys, and which sunk at different periods,—in most cases, disappearing altogether.

If, then, the existence of lake-beaches be so common in the valleys of Scotland, there will be the less hesitation in ascribing the Lochaber shelves to the same cause,—established as that cause has been separately by local evidence.

That the occurrence of lake-beaches in the valleys of Scotland should be frequent, is only what every geologist must be prepared to expect, who considers the proofs which may be adduced, of the gradual emergence of the land out of the sea. Some of these proofs, in so far as afforded by Scotland, I shall immediately notice; but assuming that Scotland was, to the depth of 1300 feet or more, submerged beneath the waters of the ocean,—as it rose out, there would be lakes in every inland hollow, each, of course, having its river to carry off to the sea, the rain falling on its surface and that of the adjoining mountains. The stream thus issuing, would gradually wear down the detritus which formed a barrier at one end of the lake; and the cutting power of the stream would be gradually increased, as the elevation of the land proceeded; so that in most cases the blockage of lakes would, in the course of time, be extensively undermined

and worn down, and sudden depressions of lakes would take place, leaving marks of horizontal shelves along the sides of valleys.

The progress of these important changes is indicated, in many parts of the country, by the existence of haughs or river-flats, far above the present channels of the streams, and which evidently had been formed when they flowed at a much higher level.

Thus, from Perth up to Loch Tay, a number of isolated flats or terraces occur, forming a pretty uniform level, rising gently inland, and at a rate rather faster than the slope of the river. Near Perth, these old haughs are from 90 to 100 feet, and at Dunkeld about 110 feet, above the river. This old haugh at Dunkeld may be traced on both sides of the valley,—Dr Fisher's house being on it at the east side, and Claypotts farm-house on it at the west side. It may even be traced a considerable distance up both sides of the Braan, where it slopes a little to the eastward.

There is a low haugh at Dunkeld which is only about 20 feet above the present bed of the river, and is, therefore, quite distinct from the higher terrace above described. The ground is now cultivated and enclosed; so I suppose that the floods never rise to a level with it now.

On the Tweed, in like manner, the remains of ancient haughs can be traced in many part of its course. About half a mile above Berwick Bridge, one may be seen on the south side, from 30 to 32 feet above the sea. At Gainslaw, it is 44 feet; opposite to Finchie, it is 55 or 56 feet; opposite to Paxton, it is 58 feet; at Norham, it is 93 feet above the sea.

At New Rattray (in the parish of Blairgowrie) I observed an extensive flat, or ancient haugh, with its cliff or bank about 80 feet above the River Ericht.

On the Isla, above Airley Castle, there is haugh land, on both sides, about 30 feet above the present level of the river.

On the River Garry, about $3\frac{1}{2}$ miles north of Blair, there are on the east side two terraces, the one about 30 and the other about 50 feet above the river; but whether they are the remains of ancient haughs, or the beaches of a lake, it is difficult to determine.

XXVIII.—*Memoir of Dr THOMAS CHARLES HOPE, late Professor of Chemistry in the University of Edinburgh.* By THOMAS STEWART TRAILL, M.D., F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh.

(Read December 6, 1847.)

It is presumed that a notice of the life and labours of ONE, who was, for more than fifty years, a most skilful and successful teacher of chemistry in the Universities of Scotland, where he was the instructor of more than 15,500 pupils; who initiated in that interesting science many who now hear me; who long filled the office of vice-president amongst us, will not be unacceptable to the Royal Society of Edinburgh.

THOMAS CHARLES HOPE was a son of Dr JOHN HOPE, the first Regius Professor of Botany in the University of Edinburgh, and of JULIANA STEVENSON, daughter of an eminent physician in that city.

Professor JOHN HOPE was a grandson of Lord RANKEILLOR, an eminent Scottish judge in the early part of the last century, and son to Mr ROBERT HOPE, a respectable surgeon in Edinburgh. Professor HOPE died in 1786, at the age of 62. His family consisted of four sons and a daughter. ROBERT, the eldest, was bred to the bar, but died in early life; MARIANNE married JAMES WALKER, Esq., of Dalry, and died in 1837, leaving an only daughter, who became the wife of Sir JOHN WALL; JOHN, a Major in the army, who died in 1840; THOMAS CHARLES, the subject of this memoir, who was born on the 21st of July 1766, and died on the 13th of June 1844; JAMES, a writer to the Signet, who died in 1842, leaving several children.

THOMAS CHARLES, the third son of Dr JOHN HOPE, received the elements of his classical education in the High School of Edinburgh, to which he was sent in 1772; but in 1778 he was removed to a school at Dumfries, and was, 1779, entered as a student of general literature in this University, at the early age of thirteen; a practice still too common in this country. There he pursued the usual curriculum of general study, before he began to apply to medicine.

As was natural, he had devoted much attention to Botany, and, under his able father, had made such proficiency, that on the death of the latter in 1786, he aspired to the Botanical Chair; and, though supported by the influence of Sir JOSEPH BANKS, Sir GEORGE BAKER, and even by the favour of Royalty, the all-powerful influence of Mr DUNDAS prevailed, and he was unsuccessful. In June 1787, he obtained the degree of Doctor in Medicine at our University. Dr IRVINE, who held the Lectureship of Chemistry in the University of Glasgow, having died in the following month, Dr HOPE was appointed to fill the vacant chair, on

the 10th of October of the same year; and thus a new field was opened to his ambition.

It was no easy task with which young HOPE had to grapple. The Glasgow Lectureship had been successively held by men of consummate abilities, and high chemical acquirements. The immediate predecessors of Dr IRVINE, had been Dr ROBISON, Dr BLACK, and Dr CULLEN; men whose names will ever stand conspicuous in the science of Scotland. Instead of acting as a discouragement, this consideration only stimulated Dr HOPE to make every effort to prove himself no unworthy successor of these eminent teachers of chemical science. With the general doctrines of chemistry he was well acquainted; he possessed ingenuity in devising illustrative experiments, and a rare delicacy in chemical manipulation. Yet, as he has confessed to the writer of this memoir, from the shortness of the period for preparation, the scantiness of his apparatus, and the utter want of assistance in his laboratory, he regarded his first course of chemistry as very imperfect. But the novelty of his mode of teaching, and the neatness of his experiments, seem to have won the approbation of his auditory.

He was, at that period, a strenuous supporter of the then generally received doctrines of STAHL—that inflammable bodies owed that quality to the presence of a principle which was termed *phlogiston*, and of course taught that doctrine in this his first course of lectures. But his conversion to the Lavoisierian or French theory of chemistry was at hand.

It is generally known, that from 1777, LAVOISIER had doubted the existence of such a principle as *phlogiston*, and in 1785 proposed the antiphlogistic theory, supported by such facts and decisive experiments, that his views were speedily adopted by his own countrymen; though for a considerable time afterwards, they were not received in Britain.

The late Sir JAMES HALL happened to pass the winter of 1787 in Paris, and was much in the society of LAVOISIER, who showed great anxiety to make a convert of Sir JAMES, and, through him, to spread his doctrines among British chemists. For this purpose he not only gave Sir JAMES free access to his papers, but exhibited to him several very important experiments, even before they had been communicated to the Academy of Sciences, or made known to the chemical world in general. Sir JAMES HALL returned to Scotland in the autumn of 1787, well versed in the new doctrines, of which he became an able and zealous propagator. He had many long discussions on this subject with Dr HOPE, who was then a keen supporter of the phlogistic hypothesis, but was soon convinced by the arguments and facts communicated by his friend; and next winter he taught them to his class, the first occasion on which the Lavoisierian doctrines were introduced in a public course of lectures in Great Britain.

In the beginning of 1783 Dr HOPE was admitted as a Fellow of this Society; and soon after he resolved to pass the summer vacation in Paris.

In his way to the French capital, he made a short stay in London, where he was very kindly received by Sir JOSEPH BANKES ; and he had the high gratification of being introduced to CAVENDISH, BLAGDEN, HERSCHEL, and several other English philosophers.

At Paris he experienced marked attention from LAVOISIER and BERTHOLLET ; which he ascribed partly to his having been a pupil of Dr BLACK ; but was principally, I believe, owing to his having been the first chemist who had publicly taught the new French doctrines in Great Britain.

Dr HOPE considered this an important era in his life, as introducing him to men whose names were then becoming celebrated over Europe for their skill in a science to which he was ardently devoted. The amiable manners and great abilities of LAVOISIER made a deep and lasting impression on the Scottish professor ; and few persons more sincerely deplored the sad fate of that accomplished man, from whom he had received the most flattering attentions.

During his connection with the University of Glasgow, Dr HOPE enumerated as his colleagues and his friends, Dr THOMAS REID, the celebrated Professor of Moral Philosophy ; the eminent Mr JOHN MILLER, Professor of Law ; and Mr GEORGE JARDINE, Professor of Logic.

Dr HOPE, for some years, entertained the wish to join the practice of medicine with his chemical labours ; and in 1789, sought and obtained the appointment of assistant Professor of Medicine, and successor to his uncle Dr STEVENSON in the University of Glasgow. For two years he taught the Theory and Practice of Medicine, at the same time with Chemistry. On the death of his uncle, in 1791, Dr HOPE became the sole Professor of Practical Medicine, and then resigned the office of Lecturer on Chemistry ; but he continued his private researches in his favourite study, the first result of which was his masterly paper "*On a new mineral from Strontian.*"

This was communicated to the Royal Society of Edinburgh on the 4th Nov. 1793 ; and it proved, what indeed had been previously conjectured by others, that this mineral contained a new earth, differing decidedly in its qualities from Barytes, to which it bears the greatest affinity. To this earth Dr HOPE gave the name of *Strontites*, from the place at which it had then only been found. From the appearance of this mineral, which had been, I believe, first noticed, about six years before, by Dr WALKER, Professor of Natural History in Edinburgh, it was generally supposed to be a variety of heavy-spar, or perhaps might contain a new ingredient. Yet KIRWAN, in the Second Edition of his Mineralogy, published late in 1794, takes no notice of Strontites ; except we may consider such, his statement, "that he had heard of the discovery of *Barolite*, or aerated Barytes in Argyleshire ;" and SCHMEISSER, whose "Mineralogy" appeared in London two years after the reading of Dr HOPE's paper, disingenuously passing over his experiments, states, "by analysis I found it yielded 68 of Strontian earth, 30 of

carbonic acid, 1 of calcareous earth, and a little of phosphate of iron and manganese, which probably gives it colour." This would lead the unsuspecting reader to infer that SCHMEISSER had been the discoverer of the new earth, which is certainly not the case; but this is only one of the many plagiarisms of this writer. The only chemist who has the slightest claim to the merit of an original detector of Strontian earth, besides Dr HOPE, is M. KLAPROTH; who, in the *Chemische Annalen* for 1793-94, compared Strontianite with Witherite. In his first paper, KLAPROTH conjectured that the two minerals differed in composition, because the salts of Strontian colour the flame of combustibles red, while those of Barytes do not; and this conclusion was afterwards confirmed by some experiments of SULZER and BLUMENBACH. Neither KLAPROTH nor HOPE seem to have been aware of what the other had discovered, and both may therefore be considered as original discoverers, but the first full investigation of the subject is undoubtedly due to Dr HOPE.

The success of these investigations, and the popularity of Dr HOPE's Chemical Lectures at Glasgow, suggested to the celebrated Dr BLACK, then in declining health, the idea of having his promising pupil Dr HOPE associated with him, as his assistant and successor in the Chemical Chair. He accordingly made the proposal to Dr HOPE in 1795, obtained the concurrence of the Patrons, and on the 4th of November of that year, the latter body chose Dr HOPE in that capacity. In that session but a few of the lectures were delivered by Dr HOPE: But in the session of 1796-97, after Dr BLACK had concluded his admirable Lectures on Heat (as I find from M.S. notes of a friend who attended that course), the venerable Professor introduced Dr HOPE to the class in the following terms:—"After having, for between 30 and 40 years, believed and taught the chemical doctrines of STAHL, I have become a convert to the new views of chemical action; I subscribe to almost all M. LAVOISIER's doctrines; and scruple not to teach them. But they will be fully explained to you by my colleague and friend Dr HOPE, who has had the advantage of hearing them from the mouth of their ingenious author." Accordingly, Dr HOPE delivered a considerable portion of that wintercourse to a large audience; and in the summer of the year 1797, he also gave a three months' course of Chemistry.

The eminent men who were at that time the ornaments of our University, were Professors MONRO *secundus*, BLACK, GREGORY, ROBISON, DUGALD STEWART, and PLAYFAIR—and HOPE always remembered with much satisfaction his earlier intercourse with Principal ROBERTSON, ADAM SMITH, and especially with HUTTON the geologist—a constellation of names that shed a lustre on the society of Edinburgh at that period.

It would seem that the subject of our memoir still intended to conjoin the practice of medicine with his academical duties. For this purpose, he became a Fellow of the Royal College of Physicians in November 1796; and, until some

time after the beginning of this century, regularly took his share of the duties of Clinical Professor of Medicine.

Dr BLACK, who was always of a most delicate constitution, did not feel himself able to lecture after the session of 1796-97; but the life of this truly great philosopher and most accomplished teacher, was protracted to the 14th of November 1799. On his death Dr HOPE became the sole Professor of Chemistry in our University.

It was in the Session of 1798-99, that the writer of this memoir first became Dr HOPE's pupil; and he remembers, with gratitude, that it was from the clear and able prelections, and most happy experimental illustrations of the leading principles of the science by Professor HOPE, he imbibed that predilection for chemical pursuits, which long formed his chief relaxation from the severer duties of his professional life; and which, he hopes, will continue to afford interest and amusement to his declining years.

I may here remark, that Dr HOPE had become, from the variety and excellence of his illustrations, and dexterity in chemical manipulations, the most popular teacher of the science that had ever appeared in Great Britain. Not only was his lecture-room crowded with medical students from every part of the British dominions, but numerous foreigners resorted to Edinburgh, and became his pupils. Many of our nobility at that time were among his students. During one of the winters that I attended his class, among my fellow-students were the late Earl of LAUDERDALE, the present Earl (then Lord MAITLAND), Lord SEMPILL, and the late Lord ASHBURTON. The large class-room was filled to overflowing; and he who was not there before the commencement of the lecture had no chance of a seat.

The rage for chemistry continued for several years; and certainly no chemist ever had larger audiences than Dr HOPE. I find that the average number of chemical pupils here, during the six years preceding Dr HOPE's appointment as Dr BLACK's assistant and successor, was 225. When I attended him in the end of the last century and beginning of this, his annual pupils were above 400; in 1813 they had risen to 500, and in 1827 they had actually amounted to 575.

While HOPE lectured at Glasgow, the total number of his pupils amounted to about 300. After his removal to Edinburgh, his chemical lectures were attended by 15,500 persons, and the number of tickets issued for his chemical class was no less than 16,800.

His reputation as a lecturer induced a number of the Faculty of Advocates to request him to give a summer course of chemistry in 1800; which was also attended by many gentlemen engaged in other pursuits.

I shall now offer some remarks on the original investigations in which Dr HOPE at different times engaged, after his paper on Strontites.

It is well known that BOYLE, MARIOTTE, and other philosophers, ascertained

experimentally, that the diminution of bulk in atmospheric air is always proportional to the compressing force; or its volume is inversely as the pressure which it sustains; and philosophers had generally, from analogy, inferred the same of other gases.

I find, from some notes of Dr HOPE, that in 1803 he instituted a series of experiments to ascertain "whether the principal permanently elastic fluids, viz., oxygen, nitrogen, hydrogen, and carbonic acid, observe the same law of compressibility from pressure which air does."

In these experiments the compression was obtained by means of a column of mercury in a siphon tube, in the same manner as in the experiments of BOYLE, and of later experimentalists. The result was, that they all follow the same law of compression.

On the 9th of January 1804, Dr HOPE read a memoir to the Royal Society of Edinburgh, "*On the contraction of water by heat, at low temperatures,*" which appeared in the 5th volume of the *Transactions*, in 1805, p. 379.

The Florentine Academicians had published, in 1667, the singular fact, that water expands as it cools towards its freezing point; and in 1683, the same was stated to the Royal Society of London by Dr CROUNE, the Gresham lecturer. His experiments shewed that water, when cooling, begins to expand as its temperature sinks, from several degrees above the freezing point, until it begins to congeal. Several subsequent writers endeavoured to confirm these observations, but differed as to the point at which water attains its maximum density; some contending for the 40 or 41° of Fahrenheit; others for the 42° or 43°. All those experiments were made in tubes with large bulbs at one extremity, resembling in form the glass of a thermometer, but on a larger scale.

On the reading of CROUNE's paper, it was contended by Dr HOOKE, one of the most acute but most disputaceous philosophers of his age, that this expansion was apparent, not real; arising from the sudden contraction of the material of the bulb, on the application of cold. This opinion has since been maintained by several very eminent men; among whom we may mention DALTON, whose experiments on this subject are most ingenious, and who, in a private letter, drew Dr HOPE's attention to this curious phenomenon. It occurred to Dr HOPE, that this point might be decided by experiments, in which a change in the capacity of the containing vessels could have no influence on the result.

He took a cylindrical glass vessel, $8\frac{1}{2}$ inches deep and $4\frac{1}{2}$ inches wide, which was filled with water at the freezing point, 32°. Two delicate thermometers were suspended in the axis of the jar, so that the bulb of one was half an inch below the top of the liquid, and that of the other as far from its bottom. This apparatus was placed in a room at a temperature 60°, and the progressive temperature of the water was carefully noted, as indicated by both thermometers. The result was, that up to 38°, the lower thermometer was invariably one degree higher than the

upper; a proof that, as its temperature rose from 32° to 38° , the water had become more dense. On reversing this experiment, by placing water at 53° in a medium cooled to 32° , he found, while the temperature of the water descended to 40° , that the water at the bottom was always the coldest, and that this difference between upper and lower thermometers was sometimes as much as 7° or 8° ; but that in cooling from 40° to the freezing point, the thermometer at the bottom remained higher than that near the surface of the liquid.

The experiments of Dr HOPE, which were varied in different modes, led him to fix the point of greatest density of water at the temperature of $39^{\circ}5$ Fahrenheit.

These well-devised though simple experiments are perfectly conclusive on the question of the greatest density of water being several degrees *above* its freezing point; and Mr DALTON, the most able advocate of the opposite doctrine, afterwards admitted the general correctness of the observation, though he considered that the greatest density was not at so high a point as Dr HOPE supposed. There are, however, many facts which would lead us to *infer*, that the greatest density of water cannot be far from the point assigned by HOPE—as, for instance, the remarkable uniformity of temperature in deep alpine lakes, which is about 40° , according to the observations of PICTET and others.

From a long note attached to this paper of Dr HOPE* we also learn, that at an early period he had experimentally proved the fallacy of Count RUMFORD's assertion, that liquids were absolute non-conductors of heat. This philosopher had alleged, that when heat was applied to the upper surface of a fluid, the heat could only affect a thermometer placed *below* the surface of the liquid, by transmission downwards through the medium of the sides of the containing vessel; because, according to him, the particles of fluids communicate none of the caloric they receive to the contiguous particles (as takes place in solids), and that when heat is applied below, they become heated only by currents set in motion by the diminished gravity of the heated particles.

In these experiments, Dr HOPE employed a wide glass jar to contain the liquid to be the subject of trial, and applied heat to the surface of the liquid in a vessel 11 inches in diameter. The bulb of a delicate thermometer was placed half an inch below the surface of the liquid; and all conduction by the sides of the vessel was prevented, by keeping it immersed in water equally cold as high as the surface of the liquid within the vessel. Notwithstanding these precautions, the thermometer, in several experiments, slowly rose. The liquids subjected to such trials were water, olive-oil, and mercury.

Other experiments were conducted in a different manner. Equal portions of liquids, such as alcohol, were rapidly mixed together at different temperatures; and the mixture immediately indicated a mean temperature—which HOPE

* *Trans. R. Soc. Edin.* V., p. 394.

contended could not have happened, if liquids had been absolute non-conductors of caloric.

These experiments seem sufficiently conclusive; but Count RUMFORD still insisted, that the rise of the thermometer was only owing to the conduction by the sides of the containing vessel, in HOPE's experiments, as well as in the analogous investigations of THOMSON, NICHOLSON, and DALTON.

This objection suggested to the late Dr JOHN MURRAY the ingenious idea of employing a hollow cylinder of ice as the containing vessel; which, as its temperature could not rise above 32° , could not conduct or communicate any heat to the thermometer. Water could not be employed in this apparatus, on account of its anomaly in expanding by cold near its freezing point; but olive-oil, cooled to 32° , was used; and in experiments made by suspending the heating cause in contact with the surface of the oil, the thermometer rose, in a longer or shorter interval, in proportion to the greater or less depth of the instrument below the surface of the oil.—(*Nicholson's Journal*, 8vo series, I. 425.)

In considering these experiments and the objections stated, it occurred to me, that if the *same* apparatus were employed with different fluids, did the rise of the thermometer depend on the conduction of the sides of the vessel, that rise should be nearly equal, whichever liquid was employed. I tried this with ten different liquids; and though the apparatus was the same, and the distance between the source of heat and the thermometer similar, yet the time required to raise the thermometer to the same point, was very different with the different liquids: this I ascribed to the difference in the conducting power of each liquid.—(*Nicholson's Journal*, XII. 137, for 1805.)

All these investigations confirmed the view taken by HOPE, that though liquids were very slow conductors of caloric, they could not be considered, as was alleged by RUMFORD, absolute non-conductors.

Dr HOPE's reputation as a teacher of chemistry, arising from the causes already noticed, and his tact in exciting in his hearers his own enthusiasm for the study, long continued to attract vast crowds of pupils. His honours kept pace with his reputation.

In 1810 he was elected a Fellow of the Royal Society of London: in 1815 he was chosen President of the Royal College of Physicians of Edinburgh, an office which he continued to fill for four successive years; in 1820 he was admitted an honorary member of the Royal Irish Academy; and in 1823 he became one of the Vice-Presidents of this Society, an office which he held until his death. During his connection with the College of Physicians, he took an active part in the preparation of the ninth and tenth editions of their Pharmacopœia, especially in that published in 1817. For several years, besides his duties as a Professor of Chemistry, Dr HOPE gave an annual course of Clinical Medicine in this University, which was also numerously attended. But for many years before his death, he

resigned to his younger brethren the duties of the Infirmary, and of Clinical instruction.

Dr HOPE is the author of a decided improvement on the Eudiometer of SCHEELE, which, by permitting the convenient agitation of the included air with the liquid that absorbs the oxygen, expedites and simplifies that process; and is described in most elementary works on Chemistry.—(*See Nicholson's Journal, Vol. VI.*)

The establishment of Mechanics' Institutions, or Schools of Art for the instruction of the humbler classes, gave to that rank of society means of acquiring information beyond that usually obtained by many of the wealthier classes; and, in the opinion of some, diminished the respect of mechanics for individuals less knowing than themselves. The system of courses of popular lectures on scientific subjects for both sexes, which had prevailed for many years in various parts of England, was comparatively little practised in Scotland, when Dr HOPE delivered, in the spring of 1826, a short course of chemical lectures to Ladies and Gentlemen. His vast lecture-room was crowded with what he described to me as a "most brilliant audience;" and his example was soon followed by more than one of his colleagues in the University, and by several of the eminent men who then taught different branches of natural science in their private establishments; undoubtedly with no small benefit to the rising generation, and the more general diffusion, among all ranks, of interesting subjects of contemplation, and of conversation. Even admitting that the knowledge thus diffused is not deep, it has imparted to social intercourse, a vigour and variety that contrasts favourably with the former insipidities and frivolities of fashionable society.

Dr HOPE had always endeavoured to impress his pupils with the importance of Practical Chemistry, and introduced into the University classes for the cultivation of that branch of study; but, from increasing years, and love of ease, this department he soon almost wholly abandoned to his assistants.

In 1828, to encourage the study of chemistry among the students in the University, particularly in the practical department, he instituted a chemical prize; and, for this purpose, presented to the Senatus Academicus a sum of £800, as a fund, the interest of which should, annually or triennially, be given as a prize to the author of the best essay on a given chemical subject, illustrated by experiment. It should be observed, that money thus liberally bestowed, was the sum which Dr HOPE had received for his popular lectures on chemistry, which he appears from the beginning to have destined for this purpose.

For many years Dr HOPE appears to have abandoned the pursuit of original research, with which he had so auspiciously commenced his chemical career, and to have confined his efforts to the improvement of his lectures, and the devising of striking experimental illustrations.

I find no original paper of his, from the publication of his investigations on

the conducting power of fluids, until the 18th January 1836, when he read, to the Royal Society of Edinburgh, the first part of a paper entitled, "*Observations and Experiments on the coloured and colourable matters in leaves and flowers of plants, upon which acids and alkalis act in producing red, yellow, or green colours.*" A second part of this paper was laid before the Society on the 21st of the following March.

Although chemists have at all times used coloured vegetable infusions for indicating the presence of acids and alkalis, no researches appeared to have been made on the peculiar vegetable principle on which the acid and alkali acted; and it was generally taken for granted that both descriptions of agents acted on one and the same principle. Dr HOPE endeavoured to shew, by various experiments on the general colouring matter of plants, that vegetable infusions, which became red by the addition of an acid, and green or yellow by an alkali, contained two distinct principles, on one of which acids acted, and alkalis on the other. To the former he proposed the name of *Erythrogene*, and for the latter that of *Xanthogene*. DECAN-DOLLE had distinguished the colouring matter of flowers by the name of *Chromule*; and ELLIS speaks of the substance which may become green, red, or yellow, under different circumstances, as the *colourable matter* of plants. The object of Dr HOPE's researches was to prove, that this matter was not an individual substance, but consisted of two distinct vegetable principles, which exist either separate or combined in different plants. He illustrated this by many experiments on different sorts of plants, and gave the results in eight tables. He shewed that all green leaves, all white and yellow flowers, contain only one of these principles, viz., *Xanthogene*, that all red and blue flowers, also all leaves with red colours, contain both *Xanthogene* and *Erythrogene* (with the single exception of Litmus, which contains no *Xanthogene*), and that red flowers abound in *Erythrogene*. The distinct nature of these proximate principles of vegetables he inferred from the different modes in which they are affected by chemical re-agents.

In the same year Dr HOPE made a communication to the Society "*On the Chemical Nomenclature of Inorganic Compounds.*" He pointed out the disadvantages of the want of a discriminating and uniform nomenclature among teachers and writers on chemistry; and stated certain changes which he had for some time employed in his lectures.

The changes proposed were—

1. To discard the prefixes *proto*, *per*, *super*, *sub*, for compounds.
2. To adopt rigidly the happy suggestion of Dr THOMSON, viz., to employ the Greek numerals to denote the number of atoms or equivalents of the base of a compound, and the Latin numerals for the number of atoms of the oxygene or acid.
3. To avoid as much as possible the intermixture of Greek and Latin in numerical indications.

He added examples thus—

1	atom of base to	1	of oxygene, oxide of base.
1	...	2	... bis oxide.
1	...	3	... ter oxide.
2	atoms of base to	1	of oxygene, dis oxide.
3	...	1	... tris oxide.
2	...	3	... dis-ter oxide.

and so forth.

The general adoption of some such nomenclature, he conceived, would give a desirable accuracy to chemical language.

In a conversation with Dr HOPE in the early part of 1837, I noticed the discordant opinions held by various philosophers on the maximum density of sea-water, and asked whether he had applied to this fluid the same beautiful and simple investigations by which he had ascertained the point of greatest density in fresh water. He replied in the negative. I strongly recommended the subject to his notice; because, as it appeared to me, several geologists and hydrographers had deduced erroneous explanations of certain phenomena in the ocean from this undecided point. I added, that I should long ago have attempted to solve it, had I not considered that it would have been an interference with a subject he had already so ably discussed. He thanked me for the hint, and the consequence was, the completion of the series of experiments, which he communicated on the 2d of April 1838, to the Society, in an "*Inquiry whether sea-water has its maximum density at some degrees above its congealing point, after the manner of fresh water.*" Most philosophers seem to have assumed, that sea-water followed the same law in cooling as fresh water; and its greatest density was generally considered to be at temperature $36\frac{1}{2}^{\circ}$ F.

Dr HOPE first tried the effect of cooling sea-water from 40° in vessels shaped like large thermometers, and found that it continued to shrink, by a diminution of temperature, like other bodies. He afterwards employed the same apparatus with which he had examined the peculiarity in fresh water; and he found, that in cooling from 40° to its freezing point, the coldest water was invariably at the bottom of the vessel. Therefore, the striking anomaly which so remarkably distinguishes the cooling of *fresh water*, does not take place in *sea-water*. The importance of this conclusion will be manifest to those who have examined the theories of oceanic currents, and the remarkable fact, that the existence of banks or shoals in the ocean is marked by a fall in the temperature of the superincumbent water.

Dr HOPE reserved the examination of the precise point of the maximum density of sea-water for a future communication—which was never made.

In 1843, the Society had two communications from Dr HOPE. The first was—"Observations on the Flowers of the *Camellia Japonica*, *Magnolia Grandiflora*, and *Chrysanthemum Leucanthemum*." This paper was read on two evenings,

the first on the 23d of January, the last on the 3d of April. The author, from the action of different re-agents on infusions of these flowers, established the existence in each of a distinct proximate principle, which, however, he had been unable to exhibit in a separate state; to these he gave the name of *Camelline*, *Magnoline*, and *Chrysanthemine*. He shewed, also, that notwithstanding the fine white of the petals of *Camellia Japonica*, they contained much iron.

The second paper, his last communication, was read on the 1st of May 1843, the very last time that Dr HOPE was ever at the meetings of our Society. It is styled "*An Attempt to explain the Phenomena of the Freezing Cavern at Orenburg.*"

This cavern is described by Sir RODERICK MURCHISON, as one of several occurring in a low hill of Gypsum. In winter, the air of this cavern feels warm to those who enter it; but in summer an intensely cold air issues from it. This has been explained by Sir JOHN HERSCHEL, as being produced by the long time the waves of heat and of cold take to penetrate to the interior of the cavern—each requiring six months to penetrate to that depth; just as SAUSSURE found, that it required, at Geneva, six months for the heat of summer, or the cold of winter, to penetrate to the depth of $29\frac{1}{2}$ feet. While admitting this general explanation, Dr HOPE considered that it would require something more to explain the forcible issue of such cold air during the summer months; and he makes an ingenious conjecture, on the part performed by the air cooled in the fissures, described as existing in the inmost recesses of the cavern, in producing that phenomenon.

The subject is very interesting though obscure; but I may observe that such streams of cold air are not peculiar to the Orenburg cave. Streams of air, cooled from 15° to 34° below the external air in the shade, are known to issue from the crevices of the small artificial hill at Rome, named *Monte Testaccio*; from the limestone grottos of *Cesi*, in the Roman states, so well described by SAUSSURE, in *Journal de Physique* for 1776; from the caves in the sandstone hill, on which is perched the miniature republic of *San Marino*; from the *Cantines* in the potstone rock near Chiavenna; from the caverns of *Capriuo*, on the Lake of Lugano; and from the calcareous caves of *Hergisweil*, at the base of Mont Pilate, nearly opposite to Lucerne. What is still more extraordinary, such cold caves exist in countries the seats of not yet extinguished volcanic fire. Sir WILLIAM HAMILTON describes the cold winds issuing from the cave of *Ottajano*, at the base of Vesuvius; and in the Isle of Ischia, the air which issues from the *Ventarola* of *Funera* is as cold as 43° F., when a thermometer in the shade, without the cavern, is at 58° —(See Saussure, *Voyages dans les Alpes*, III. 1405.)

Such are the chief contributions of Dr HOPE to physical science.

It has been alleged that they are fewer and less important than we had reason to expect, from the long period during which he filled the Chemical Chair,

his acknowledged skill in experiment, and the brilliant path then opening for important discoveries in chemistry, which have immortalized the contemporary names of BLACK, PRIESTLEY, DAVY, WOLLASTON, DALTON, and FARADAY among ourselves—of LAVOISIER, BERTHOLLET, VAUQUELIN, GAY LUSSAC, VOLTA, KLAPROTH, BERZELIUS, and LIEBIG on the Continent. That there is foundation for this criticism, I will not attempt to deny : and, indeed, Dr HOPE seems to have anticipated it, by some observations he once made verbally to myself, and has stated in a paper now in my possession, as his apology. “Those,” says he, “who devote themselves to the science of chemistry, may be divided into two classes—1st, Those whose labours are employed in original researches, to extend our knowledge of the facts and principles of the science. 2^{dly}, Of those whose business it is, from university or other appointments, to collect the knowledge of all that has been discovered, or is going forward in the science, to digest and arrange that knowledge into lectures, to contrive appropriate and illustrative experiments, and devise suitable apparatus for the purpose of communicating a knowledge of chemistry to the rising generation, or others who may desire to obtain it. From my professional situation, I consider myself, as Dr BLACK had done before me, as belonging to the second class of chemists. I consider my vocation to be the teaching the science.”

It is true that it is the paramount duty of one appointed to teach a science to make that his principal object ; but this, I humbly conceive, is quite consistent with most extensive original research. It may be that the regular recurrence of the labour of teaching the elements of a science, requiring several hours of daily personal exertion, may sometimes indispose a lecturer to experimental investigations of a similar kind ; but such has not been its effects on DAVY, THOMSON, BERZELIUS, or LIEBIG ; all of whom have combined the business of teachers of chemistry with the most valuable and laborious original researches. Dr BLACK had certainly made all his great discoveries before he was Professor in the University of Edinburgh ; but his health was always very delicate, and his example can scarcely be pleaded for one who enjoyed such uninterrupted and vigorous health, that he never was a single day prevented from lecturing by indisposition, for a period of more than fifty years.

Dr HOPE undoubtedly fulfilled admirably the duty of a public teacher of chemistry, as we have already stated. His mode of lecturing was methodical and clear, though his style was occasionally too laboured ; he scarcely ever failed in the performance of the nicest and most difficult experiments, which he introduced to an extent previously never attempted in chemical prelections ; and he possessed the faculty of impressing his hearers with just notions of the importance and interest of the science. Still it is to be regretted, that one so well qualified to advance the boundaries of the study, had limited his ambition and his exertions

almost so exclusively to the business of methodizing and detailing the discoveries of others.

We may here remark, that besides the eminent philosophers already mentioned as his friends, HOPE was on terms of very friendly intercourse with WATT, DALTON, WOLLASTON, and DAVY. His acquaintance with the latter began in 1799, ere that illustrious man had yet risen to celebrity. In passing through Bristol, HOPE visited the Pneumatic Institution of Dr BEDDOES, and was much struck with the originality and inventive genius of young DAVY. Soon afterwards, a lecturer of talent was wanted to fill the Chemical Chair in the Royal Institution established in London, under the management of Count RUMFORD. Dr HOPE was consulted; he strongly recommended DAVY to the notice of the Count; and in 1801, the young chemist was established in the Royal Institution. This anecdote, which I have extracted from the original correspondence, once in my hands, is honourable to the discernment of HOPE, who thus early recognised that energetic genius, which was destined to win the proudest laurels in the career of physical discovery.

Among Dr HOPE's most intimate friends in Scotland, were Dr HUTTON, the geologist, and Sir JAMES HALL. From the intercourse with these eminent men, he had early imbibed their geological tenets; and for many years he was the only public teacher of science in this country, who inculcated the doctrines of the *Plutonic* theory of the earth. During the many years of my studies in this University, HOPE regularly gave several interesting lectures on geology in his chemical course, and was a strenuous assertor of the truth of the Huttonian theory, which he continued annually to teach in many subsequent years; while the rival Wernerian doctrines were most ably, and no less strenuously maintained, by my friends, Professor JAMESON, and the late most eminent and eloquent lecturer Dr JOHN MURRAY. At that time the chemical history of mineral bodies formed no inconsiderable part of a course of chemistry; and it was in introducing the mineral kingdom to the notice of his pupils, that Dr HOPE exhibited many of the proofs of the igneous formation of stony bodies; which was also illustrated by a well-selected series of rocks, chiefly collected by himself in different excursions in the Highlands and Western Isles, and in various other parts of the United Kingdom.

For many years Dr HOPE enjoyed uncommon health, and continued to discharge the duties of the Chemical Chair with his usual success, until within a year of his death.

A few years before that event, he complained to me of inability to read by candlelight, and of suffering severe pain in his eyes on making the attempt. On examining his eyes, I discovered on each cornea those minute depressions like the marks of the point of a pin, which have been described by some authors as abrasion,

or commencing ulceration of the cornea. The daily use of a weak solution of nitrate of silver gradually removed the disease; but, after some months, it recurred in a less violent degree, and again yielded to the same remedy.

In 1838, on completing the fiftieth year of his career as a Professor of Chemistry, Dr HOPE was invited to a public dinner by a numerous body of his former pupils. The meeting was attended by many philosophers from a distance, as well as by a great number of the inhabitants of Edinburgh. It was on this gratifying occasion that he stated, among other causes of thankfulness, that he never had been for a single day, either as a student or as a teacher, detained from the duties of his class.

Dr HOPE had continued his lectures in the University until the conclusion of the winter session in 1843. It was observed, that his voice was feeble, and although his experiments were, as usual, neatly performed and successful, that he had lost something of his wonted energy. Increasing debility induced him, in the autumn of that year, however, to resign his Professorship, rather unexpectedly, a short time before the commencement of the session of 1843-44; so that the Patrons had not sufficient time to deliberate on the choice of a successor in this important Chair. In the mean time, it was very necessary for the interest of the University, that a course of chemistry should be there delivered as usual. I was then in England; but, at the earnest request of the Senatus Academicus and the Patrons, after some hesitation, I undertook the duty, and taught the chemical class during the session of 1843-44. I know that Dr HOPE also was gratified by my undertaking the task. He not only freely gave me the use of his manuscript lectures, which were fairly and fully written out, and of his whole apparatus, but relinquished, in my favour, that portion of the emoluments of the class which had been secured to him as an annual retiring allowance, by the terms of his resignation.

It is but justice to Dr HOPE to state, that I found his lectures far more nearly written up to the advanced state of chemistry at that period, than I had been led to expect; and although it was necessary to make various alterations and additions, especially in the disquisitions on organic chemistry, these alterations and additions were less extensive than I had anticipated. Whether he had employed the interval between his last course and mine in improving his manuscript, I cannot tell; but the fact I have mentioned ought to be recorded. During that winter I had much intercourse with Dr HOPE. He was pleased to express a strong interest in my exertions, and said, that he had frequently enquired from others how I carried on the duties of the chemical class.

In the end of 1843 and beginning of 1844, he seemed rather more vigorous than in the preceding autumn; but as the spring advanced, his strength began very visibly to fail, and he spoke of his gradual decay with firmness and resignation.

During the month of May, he was much in bed ; yet even then he took an interest in general conversation, and warmly congratulated me on the termination of my chemical labours.

A few days before his death I saw him for the last time, and although apparently not in suffering, he took leave of me as if we should meet no more.

He quietly expired on the 13th of June 1844, in the 78th year of his age.

Dr HOPE was never married. An excellent portrait of him, by the late Sir HENRY RAEBURN, which has been engraved, is in the possession of his family : and a fine bust of him by our eminent artist STEELL was presented to the University.

XXIX.—*On the Colouring Matter of the Morinda citrifolia.* By THOMAS
ANDERSON, Esq., M.D.

(Read 17th April 1848.)

The chemistry of the colouring matters has, perhaps, scarcely as yet met with the extended and complete investigation which the importance of the subject, in a theoretical and practical point of view, appears to deserve. The attention of chemists has been almost exclusively directed to the study of a comparatively small number of these substances, such as indigo, logwood, and the colouring matters of the lichens, which have been well and completely investigated; while the remaining, and by far the more extensive class, has received only a very partial and imperfect examination. To the latter, however, belong some of the most important of our dyes; and among others, the most valuable and indispensable of all, madder namely, the chemistry of which forms a problem as yet very far from being solved, but which chemists have shewn little disposition to submit to a thorough and searching investigation; and this disinclination seems to continue, notwithstanding that ground has been broken on the subject by the important observations of ROBQUET, KUHLMANN, RUNGE, and others, which, though extremely incomplete, serve at least to indicate the importance of the results it is likely to afford, and to clear away the preliminary difficulties by which the commencement of such an investigation is surrounded.

But madder is only one member of an extensive class of dye-stuffs, each of which contains one or more colouring matters, the chemical constitution and relations of which are almost entirely unknown; and it may seem surprising that, with the important results obtained from those already examined before them, chemists should not have attempted to work out more completely than has yet been done, the rich mine of facts which they present, and the full exploration of which is equally important to theoretical chemistry and to its practical applications. The reason, however, in all probability is, that they have been deterred from the investigation by the small quantity of actual colouring matters which most of these substances contain, and the tedious and complicated processes requisite for their preparation. In indigo, and those which have been fully investigated, the colouring matters are supplied to us in the arts in any quantity, and in a state approaching, at least, to purity; but in roots and plants, the case is very different, as they there constitute only a minute fraction of the whole mass, from which their separation is always attended by difficulties, and necessitates

operations on a larger scale than is either convenient or customary in a chemical laboratory.

I have experienced this difficulty to a considerable extent in the investigation of the colouring matter to be treated of in the following communication, which has been somewhat restricted by the limited quantity of the substance at my disposal. I do not, therefore, present it to the Society as completely exhausting the subject, which I still leave open for further researches, but principally because the colouring matter in question differs in certain remarkable points from any hitherto described, and constitutes the type of an entirely new class, the existence of which is likely to throw light on some obscure points of Technical Chemistry.

The subject of these experiments was imported into Glasgow, some time since, under the name of Sooranjee, with the intention of introducing it as a substitute for madder in the art of dyeing. For this purpose it was, on its arrival, submitted for trial to some of the most experienced and skilful calico-printers in Glasgow, all of whom concurred in declaring it not to be a dye at all, and to be totally destitute of useful applications. My friend Professor BALFOUR happening to hear of this circumstance, was so good as to obtain for me a quantity of the root, which has enabled me to submit it to a chemical investigation. At the time I received the substance, no information could be got with regard to the plant from which it was obtained; but at the request of Dr BALFOUR, the importers took the trouble of writing to their correspondents in Bombay, for the purpose of obtaining specimens of the plant or its seeds. The result of this application was, that we soon received a small packet of seeds, the label of which bore that they were those of the sooranjee or soorinjee plant, the *Morinda citrifolia* of botanists. As this plant has been long and familiarly known as yielding one of the most extensively employed native Indian dyes, and as no authority was given with the seeds for the determination of the species, it was considered desirable to substantiate it by growing them, and examining the plant itself. They were accordingly sown, both in the Botanic Garden, and in the garden of Professor SYME at Milbank; unfortunately, however, not a single seed germinated, and we were compelled to content ourselves with the less satisfactory process of determining the plant by the characters of the seed itself. By a comparison with the plates of GLERTNER'S* work, they are found to agree very closely with his figures of the seeds of the *Morinda citrifolia*, and certainly belong to a species of the genus. Of this genus six or seven species are known to produce dyes,† but of these two only are important, the *M. citrifolia* of LINNÆUS, and the *M. tinctoria* of ROXBURGH, both of which are extensively cultivated in various parts of India, for the sake of

* De Fructibus et Seminibus Plantarum, vol. i., p. 144.

† In addition to those above mentioned, colouring matters are contained in *Morinda multifida*, *angustifolia*, *chachuca*, and *umbellata*.

the dye they contain. According to Dr BALFOUR, however, some confusion exists with regard to these two species, which differ so slightly, that it is doubtful whether they ought not to be conjoined, the sole difference consisting in the leaves, which are shining in the *citrifolia*, and not shining in the *tinctoria*, in the former oval, in the latter oblong. All things taken into consideration, Dr BALFOUR is of opinion that we are perfectly safe in referring the sooranjee to the *Morinda citrifolia*, which has been long described as the source of the native dye.

The *Morinda citrifolia* has been described by RHEEDE* under the name of *Cada pilava*, and is referred by botanists to the *Bancudus latifolia* of RUMPHIUS,† though it is curious that he expressly states that the roots of this species possess no dyeing properties, while he is very explicit regarding those of his *Bancudus angustifolia*, the *M. umbellata* of modern botanists, and the Wongkudu of the Javanese dyers, by whom it is employed to produce a beautiful scarlet. A detailed account of the cultivation of the *M. citrifolia*, and its employment as a dye, is given by Mr HUNTER,‡ who states that it is known by the name of Aal in Malawa, and of Atchy in Oude. No experiments, so far as I know, have been made on the chemistry of its colouring matter, unless we except some observations of Dr BANCROFT,§ on a root sent from India under the name of Aurtch, resembling madder in its external appearance, and which, from the analogy of the native names, he conjectures to be the *M. citrifolia*; no definite proof, however, is given, as Dr BANCROFT had not seen the plant, and I am inclined to doubt their identity, as the characters he ascribes to it do not agree with those of the substance I have examined. With regard to the term Sooranjee, I have been able to obtain no information in any of the works on the natural productions of India, nor is any one of whom I have had an opportunity of inquiring acquainted with the name.

Sooranjee is the root of the plant, and is imported cut up into pieces from one to four inches in length, and varying in diameter from half down to nearly an eighth of an inch. On the small pieces the bark is thick, and forms a large proportion of the whole root, but on the larger fragments it is much thinner. Its external colour is pale greyish-brown; but when broken across, it presents colours varying from fine yellow into brownish-red, and confined principally to the bark. The wood itself has only a slight yellowish shade, deepest in the centre, and scarcely apparent close to the bark;|| but it is coloured dark red by alkalies, in-

* RHEEDE, Hortus Malabaricus, vol. i., p. 97.

† RUMPHIUS, Herbarium Amboinense, lib. v., cap. 13.

‡ Asiatic Researches, vol. iv., p. 35.

§ Philosophy of Permanent Colours, vol. ii., p. 308.

|| This is also mentioned by RUMPHIUS as a character of the woody part of the roots and stem of his *Bancudus angustifolia*.

dicating the presence of a certain quantity of colouring matter in it. The bark is readily detached, and its inner surface, as well as that of the wood, has a peculiar silvery appearance, most apparent on the large pieces, and almost entirely absent in the smaller. Boiled with water, it gives a wine-yellow decoction, and with alcohol a deep red tincture.

Morindine.

For the preparation of the colouring matter of sooranjee, to which I give the name of Morindine, I at first attempted the use of boiling water, in which my preliminary experiments had shewn it to be pretty soluble; I found, however, that this method was inapplicable, as the decoction contains a quantity of mucilaginous matter which hinders the filtration of the fluid. The use of alkalies, in which the colouring matter is rapidly dissolved, likewise proved abortive, and I had finally recourse to alcohol, which succeeded perfectly. The bark of the root, separated from the woody portions and ground to fine powder, was boiled with six times its weight of rectified spirit, and the tincture filtered boiling hot. Its colour was deep brownish-red, and, on cooling, it let fall the greater quantity of the colouring matter as a brown flocculent precipitate, containing the morindine, contaminated by another red colouring matter which exists in the root in small quantity only. A second decoction, with an equal quantity of spirit, gave a paler solution, from which morindine was deposited with a much smaller quantity of the red colouring matter. This treatment was repeated over and over again, as long as the tincture deposited anything on cooling, and every successive boiling produced it purer than the preceding, until at length, from the final decoctions, it made its appearance in the form of minute radiated crystals of a yellow colour. By successive crystallizations from alcohol of 50 per cent, the red matter with which it was mixed was entirely removed, and the morindine obtained of a fine yellow colour. It was still, however, impure, and contained a quantity of ash, amounting, in one experiment, to 0.47, in another to 0.32 per cent. The separation of this could not be effected by crystallizations from alcohol alone; but after some trouble I succeeded in removing it completely by solution in alcohol slightly acidulated by hydrochloric acid, from which it crystallized in a state of purity.

Morindine is deposited from alcohol in minute needles, which, if the solution be dilute, make their appearance in radiated circles attached to the glass, and resembling, in their arrangement, the crystals of wavellite. They are extremely soft, and, on being detached, collected on a filter, and dried, mat together into a mass, presenting a rich sulphur-yellow colour, and satiny lustre. These crystals are sparingly soluble in cold alcohol, much more so in boiling spirit, especially if dilute; and the fluid on cooling is filled with a mass of bulky needles, which, when dried, shrink into a very small bulk. They are much less soluble in absolute al-

cohol, and totally insoluble in ether. Water dissolves morindine in the cold very slightly, although sufficiently to communicate a yellow colour to the fluid; at the boiling temperature, however, it is readily taken up, and again deposited, on cooling, as a gelatinous mass, destitute of all traces of crystallization, which stops up the pores of the filter, and prevents the separation of the mother liquor. It dissolves in solutions of the alkalies, with a fine orange-red colour. With concentrated sulphuric acid it gives a deep purple, which is violet in thin layers. After twenty-four hours' contact, the solution, on being diluted, deposited yellow flocks of the colouring matter in an altered condition, as it was now totally insoluble in cold water, and gave, with ammonia, a violet and not an orange solution. Nitric acid, sp. gr. 1.38, dissolves morindine slowly in the cold, with a deep brownish-red colour. The application of heat immediately produces violent action, the brown colour disappears, and nitrous acid fumes are evolved. The fluid, after long-continued boiling with the acid, and neutralisation with ammonia, gave no precipitate with salts of lime.

Solution of morindine gives, with subacetate of lead, a precipitate depositing itself in crimson flocks, which is extremely unstable, and cannot be washed without losing colouring matter. With solutions of baryta, strontia, and lime, it gives bulky red precipitates, sparingly soluble in water. Perchloride of iron produces a dark brown colour, but no precipitate. When its ammoniacal solution is added to that of alum, the alumina precipitated carries down with it the morindine as a reddish lake, and, when added to perchloride of iron, a brown precipitate is thrown down, which cannot be distinguished from pure peroxide of iron, but which contains morindine, as the supernatant fluid is colourless.

Heated in close vessels, morindine melts into a deep brown fluid, and boils at a high temperature, with the evolution of an exceedingly beautiful orange vapour resembling that of nitrous acid, which deposits itself on cold substances in fine red needles of considerable length. A bulky charcoal remains in the vessel.

The analyses of morindine were performed with oxide of copper, and upon substance which had been carefully dried for a long time at 212°. The results were as follows:

I.	{ 6.406 grains of morindine gave		
	13.028	...	carbonic acid, and
	2.990	...	water.
II.	{ 5.956 grains of morindine gave		
	12.100	...	carbonic acid, and
	2.699	...	water.
III.	{ 4.564 grains of morindine gave		
	9.270	...	carbonic acid.

	I.	II.	III.
Carbon, . . .	55.46	55.40	55.39
Hydrogen, . . .	5.19	5.03	...
Oxygen, . . .	39.35	39.57	...
	<hr/> 100.00	<hr/> 100.00	

These analyses give the formula $C_{24}H_{15}O_{15}$, which agrees perfectly with the mean of the experimental results, as is shewn by the following calculation.

			Calculation.		Mean of Experiment.
28	Equivalents	Carbon, . . .	2100.0	55.44	55.41
15	...	Hydrogen, . .	187.5	4.95	5.11
15	...	Oxygen, . . .	1500.0	39.61	39.48
			<hr/> 3787.5	<hr/> 100.00	<hr/> 100.00

The formula thus ascertained brings out an interesting relation between morindine and the colouring matters of madder, and more especially that one which is obtained by the sublimation of madder purple. From his analysis of this substance, SCHIEL* deduces the formula $C_7H_4O_4$. As this, however, is no more than the simplest expression of the analytical results, and as all the other madder colouring matters examined contain 28 equivalents of carbon, we are justified in supposing its real constitution to be represented by quadruple of that formula, or $C_{28}H_{16}O_{16}$, which differs from that of morindine by a single equivalent of water only. The unsublimed madder purple is also connected, though more remotely, with morindine, and differs only by containing 5 equivalents of hydrogen less, its formula, according to SCHIEL, being $C_{28}H_{10}O_{15}$.

Moreover, this similarity is not confined to their formulæ only, but extends itself over all their physical and chemical properties, which approximate very closely, although they are sufficiently distinct to preclude the possibility of their being confounded with one another. And this is a point well worthy of observation, as illustrating the similarity in chemical constitution of plants so nearly related in the botanical system; the morinda belonging to the natural family Cinchonaceæ, which, by many botanists, is considered as merely a section of the Rubiaceæ, of which madder is the type.

This similarity, however, does not extend itself to their properties as dyes, in which respect they differ in a very remarkable manner. I have already mentioned that the calico-printers had entirely failed in producing a colour by means of sooranjee; and this I have fully confirmed as regards the common mordants. I digested morindine for a long time, in a gradually increasing heat, with small pieces of cloth mordanted with alumina and iron, but nothing attached itself, and

* Annalen der Chemie und Pharmacie, vol. lx., p. 74.

the mordants, after boiling for a minute or two with soap, were found to be unchanged. Even with the root itself, alum mordant only acquired a slight reddish-grey shade, and iron became scarcely appreciably darker in colour. The case was different, however, when cloth mordanted for Turkey-red was employed. I obtained from Glasgow pieces of calico prepared for Turkey-red both by the old and new processes, and I found that both acquired, with morindine, in the course of a couple of hours, or even less, a dark brownish-red colour, devoid of beauty, but perfectly fixed. These observations agree with the account given by Mr HUNTER of the method of dyeing with the *M. citrifolia* employed by the Hindoos. The cloth is first soaked in an imperfect soap made by mixing the oil of the *Sesamum orientale* with soda ley. After rinsing and drying, it is treated with an infusion of myrobalans (the astringent fruit of the *Terminalia chebula*), and exposed for four or five days in the sun. It is then steeped in solution of alum, squeezed, and again exposed for four or five days. On the other hand, the powdered roots of the morinda are well rubbed with oil of sesamum, and mixed with the flowers of the *Lythrum fruticosum* (ROXBURGH), or a corresponding quantity of *purvas* (the nut-gall of a species of mimosa). The whole is introduced along with the cotton into a large quantity of water, and kept over a gentle fire for three hours, when the temperature is brought to the boiling point. The red colour so obtained is, according to Mr HUNTER, more prized for its durability than its beauty. This is simply a rude process of Turkey-red dyeing. He also mentions that, by means of iron mordant, a lasting purple or chocolate is obtained, but in this case the colour is probably affected by the tannin of the astringent matters employed in the process.

Morindone.

It has been already mentioned that morindine, when heated, is entirely altered, a quantity of carbonaceous matter being left, and a crystallizable principle sublimed, differing in its properties from the original substance. To it I give the name of Morindone.

Morindone is obtained by sublimation in the form of long needles, which, under the microscope, are found to be four-sided prisms, terminated by a single oblique face, and of an exceedingly rich and beautiful red colour. They are totally insoluble in water both hot and cold, but dissolve readily in alcohol and ether, and the solutions by slow evaporation deposit crystals. The alkalis dissolve it with a magnificent violet colour. In strong sulphuric acid it is also soluble, with the same intense violet colour, and it is precipitated on diluting the acid. Its ammoniacal solution gives a fine red lake when added to solution of alum, and a cobalt-blue precipitate with baryta water. The quantity of morindone which I was able to obtain for analysis was too small to admit of accurate results, or of all the precautions for its purification which would have been

resorted to had I possessed a larger quantity. The sublimed crystals were simply washed with ether, in order to remove empyreumatic matters, and then dried at 212°. Analysis gave the following results :

$$\left\{ \begin{array}{l} 1.629 \text{ grains of morindone gave} \\ 3.931 \quad \dots \quad \text{carbonic acid, and} \\ 0.614 \quad \dots \quad \text{water,} \end{array} \right.$$

which approximates most nearly to the formula $C_{28}H_{10}O_{10}$, as is shewn by the following comparison.

Experiment.		Calculation.		
Carbon,	. . . 65.81	65.11	C_{28}	2100.0
Hydrogen,	. . . 4.18	3.87	H_{10}	125.0
Oxygen,	. . . 30.01	31.02	O_{10}	1000.0
	100.00	100.00		3225.0

Of course, it is impossible to consider a formula as established by a single analysis upon so small a quantity. I think it probable, however, that that given above may be the true one, and that the excess of carbon was due to imperfect separation of empyreumatic matters, as, to avoid loss by solution, I washed with the smallest possible quantity of ether. That morindone is formed from morindine by the elimination of water, derives confirmation from the change which the latter substance undergoes in contact with sulphuric acid. As already mentioned, it then becomes insoluble in water, and gives a violet colour with alkalis similar to that produced by morindone, and as sulphuric acid in general acts by removing water, the probability is, that it has deprived the morindine of 5 equivalents, and converted it into morindone; at the same time this is a point which can only be determined by analysis, and the quantity which I obtained was not nearly sufficient for that purpose. Should further experiments establish $C_{28}H_{10}O_{10}$ as the true formula of morindone, we have another simple relation with the madder colouring matters, as it would differ from madder red by a single equivalent of water, the formula for that substance, according to the analysis of SCHIEL, being $C_{24}H_9O_9$. It would also be polymeric with gentianin, for which BAUMERT* has established the formula $C_{14}H_5O_5$.

Morindone is a true colouring matter, and is capable of attaching itself to common mordants. It gives with alumina a deep rose-red, and with iron violet and black; but the colours are not very stable, and it has a strong tendency to attach itself to the unmordanted parts of the cloth, and to degrade the white. Morindine, after treatment with sulphuric acid, is capable of attaching itself to ordinary mordants.

The discovery of a peculiar colouring matter capable of fixing itself exclu-

* *Annalen der Chemie und Pharmacie*, vol. lxii., p. 106.

sively on Turkey-red mordant, is of interest, as establishing the existence of a peculiar class of dyes hitherto totally unsuspected,—a class which may be extensive, and may yield important substances. It may serve also, in some respects, to clear up the *rationale* of the process of Turkey-red dyeing, which has long been a sort of opprobrium of chemistry. Although that process has been practised for a century in Europe, and has undergone a variety of improvements, no clear explanation of it was for a long time given, but it was supposed that, by the action of the dung, of which large quantities are employed, the cloth underwent a species of *animalisation*, as it was called, by which it acquired the property of receiving a finer and more brilliant colour than could be attached to it by purely mineral mordants. Recent experiments have, however, shewn that the oil, which is largely employed in the process, undergoes decomposition by long exposure to the air in contact with decomposing animal matter, and is converted into a sort of resinous matter, which constitutes the real mordant for Turkey-red. This has been pretty clearly made out by the experiments of WEISSGERBER.* He found that when cloth had been treated with oil, so as to give, when dyed, a fine rose-red colour, he could, by digestion with acetone, extract from it the altered oil; and as it was removed, the cloth gradually lost the power of attracting the colouring matter of madder, until, when it was entirely separated, the cloth passed through the dye without acquiring any colour. On the other hand, he found that, by applying the substance extracted by acetone in sufficient quantity to cloth, he could obtain the richest and deepest colours with madder, without the addition of any other substance whatsoever. These observations receive additional confirmation from the experiments detailed in the present paper, as it must be sufficiently obvious that the dark red colour obtained on Turkey-red mordant with morindine, must be entirely irrespective of the alumina, on which that substance is incapable of fixing.

I fully agree with the opinion expressed by PERSOZ that the use of alum mordant, which is at present always employed in Turkey-red dyeing, will be entirely abandoned so soon as calico-printers have learned the method of modifying at will the oil which they employ, so as to bring it at once into the state in which it acts as a mordant. Some steps have been made in this direction, by making use of some chemical agents, as nitric acid and chloride of lime, for the purpose of acting on the oil; but the improvements which have been effected stop far short of what I believe will eventually be effected, when the system of pure empiricism which has been all along employed in this particular process of dyeing is abandoned, and the subject submitted to really scientific investigation. It is understood that M. CHEVREUL has entered upon the inquiry, and in his hands there is little doubt but that it will meet with a satisfactory solution.

* PERSOZ, Sur l'Impression des Tissus, vol. iii., p. 176.

XXX.—*Notice of the Orbit of the Binary Star α Centauri, as recently determined by Captain W. S. JACOB, Bombay Engineers. By Professor C. PIAZZI SMYTH, F.R.S.E.*

(Read, April 5, 1848.)

The object of this short notice is merely to submit to the Society some astronomical results which were recently communicated to me in a letter from my friend Captain JACOB, as they appeared not only to be of a highly interesting nature in themselves, but imperatively to require being followed up farther, and as the observer has lately been obliged by bad health to resign his situation in India, it seemed advisable, for the purpose of procuring attention to the subject elsewhere, to make its peculiarly interesting features as generally known as possible amongst scientific men; and as α Centauri is already in a manner identified with Scotland, through the researches of the late Professor HENDERSON, and his determination of the parallax, no medium can be more appropriate than the Transactions of the Royal Society of Edinburgh.

The star α Centauri, situated in $14^{\text{h}} 29^{\text{m}}$ A.R., and $150^{\circ} 12'$ N.P.D., is in many respects a notable object, and though its greatest claims to attention have all arisen within the last few years, under the applications of the advanced astronomy of the present day, yet even to the naked eye it has much to raise it above the general crowd. It is a star of the first magnitude, and one of the brightest indeed of that class, and is situated in a peculiarly splendid region of the sky, the same as that occupied by the Southern Cross; a constellation, by the way, which, on account of its small dimensions, and the few stars it contains visible to the naked eye, is by no means entitled to the too warm encomiums so lavishly bestowed upon it so generally by the early Southern navigators and travellers. (And here I may be perhaps allowed to point out an error in PURDY'S Hydrography, where, amongst other fine qualities attributed to the Cross, he adds that of its forming always a sort of clock to the inhabitants of the Southern hemisphere; for the longer diameter of the Cross standing vertical, as he says, at midnight, persons may always judge by the inclination of the Cross to the one side or the other, when the middle of the night may have passed. Now the two stars at either end of the longer diameter having the same right ascension, will certainly stand in a vertical line when on the meridian, but will of course only be on the meridian above the pole at midnight, once in the course of the year.) The region of the Cross, however, abundantly compensates for the poverty of the constellation itself, for such is the general blaze of star-light from that part of the sky, that a person is immediately made aware of its having risen above the hori-

zon, though he should not be at the time looking at the heavens, by the increase of general illumination of the atmosphere, resembling the effect of the young moon.

This excessive splendour is caused not only by the profusion of first, second, and third magnitude stars in the neighbourhood, but by the extraordinary general breadth and brightness of the Milky Way thereabouts; for, separating into so many distinct luminous clouds, as it were, and exhibiting between them void black spaces unchequered by a single luminous object of any kind whatever, it forcibly impresses the idea of our being situated there near the confines of the sidereal system, or in the southern side of the vast ring in which the generality of the stars are arranged. The superior brightness of so large a proportion of the stars is then naturally accounted for by their greater proximity to us; and this fact was actually *proved* by my predecessor, who found from his own observations of α Centauri, an annual parallax of the large amount of $1''$, *i.e.*, that at the distance of this star, the radius of the earth's orbit, or 95 million of miles, subtended an angle of $1''$; the greatest quantity previously found for any star in the Northern hemisphere being only $0.23''$.

Professor HENDERSON'S results were fully confirmed by a very much longer series of observations subsequently made at the Cape Observatory by different observers, and with different instruments, and he then computed his old observations of the other principal stars in that region, and finding a considerable number* which shewed also indications of a sensible parallax, he immediately sent out a notice of the results to the present energetic Director of the Cape Observatory, for the purpose of procuring from him a greater number of observations of those suspicious stars. Such a series was accordingly commenced, and is still going on, and we may expect before long to hear of trustworthy results having been obtained, and there is little doubt that these labours will still more strongly tend to establish the proximity of that part of the sky.

On the application of the telescope to α Centauri, it proves to be composed of two stars, one very much brighter than the other, but still both may be placed in the list of first magnitude, the smaller occupying the lowest possible step in that grade. Early observers have indeed assigned it a much smaller rank, and in the British Association Catalogue published only two years ago, and intending to apply to the year 1850, it is actually made as low as the fourth magnitude; this, however, is manifestly an error, for the present epoch, as I can state from the experience derived from making the observations which served to confirm Pro-

* β Hydri.
 α Phœnicis.
 α Eridani.
 α Columbæ.
 ϵ Argus.

η Argus.
 α Crucis.
 γ Crucis.
 β Crucis.
 β Centauri.

ϵ Centauri.
 α Trianguli Austr.
 β Trianguli Austr.
 α Pavonis.
 α Gruis.

fessor HENDERSON'S parallax ; for, during the whole year, there was not a single day when, if the larger star was seen at all, the smaller one was not abundantly visible also ; and during that part of the year when they transited the meridian by daylight, they were even then invariably seen with the mural circle telescope, whatever the state of the atmosphere, unless actual clouds intervened. But that the smaller star was never in ages past as low as the fourth magnitude, the marvellous change which has occurred in the case of η Argus in our own times, would render a most hazardous assertion.

A proper motion of the large amount of $3.58''$ is participated in by both the stars, a fact which pretty clearly proves a physical connection between them ; for while they are now very nearly in the position they were in 100 years ago, when observed by the Abbé LACAILLE, they would have separated by this time upwards of five minutes, if one only was pursuing this anomalous path amongst the rest of the stars.

The first person to remark on this physical connection was Professor HENDERSON, who, in the concluding paragraph of his memoir on the parallax, says,

“ The two stars appear to be approaching each other. The earliest observations of α Centauri made with a telescope which I have found, are those of RICHER at Cayenne in 1673, but neither he nor HALLEY, who observed it at St Helena in 1677, mentions it as being double. Their telescopes were of course anachromatic, and probably not of much power. FEULLEÉ appears to have been the first person who observed the star to be double, as he mentions in the journal of his voyage in South America in July 1709. LA CONDAMINE next observed the star during the scientific expedition to Peru for measuring an arc of the meridian.” But neither of them made any observations of real service in determining the nature of the physical connection of the two stars. “ From LACAILLE'S observations in 1751–2, the distance of the two stars appears to have been then $22.5''$. MASKELYNE, who observed them at St Helena in 1761, says (*Philosophical Transactions*, 1764, p. 383), The bright star in the foot of the Centaur, marked α in the catalogues, when viewed through a telescope, becomes divided into two stars, one of which is about the second and the other the fourth magnitude. They were both observed by the Abbé DE LACAILLE. I found their distance by the divided object-glass micrometer, fitted to the reflecting telescope, to be $15''$ or $16''$. I have not found any observations,” continues Professor HENDERSON, “ of the distance of the two stars made between 1761, and the institution of the Paramatta Observatory : there, in the end of 1825 or the beginning of 1826, the distance was observed to be $23''$ (*Memoirs of Astronomical Society*, Vol. iii., p. 265), since which time it has been decreasing at the rate of more than half a second *per annum*. The angle of position scarcely appears to have changed since LACAILLE'S time, whence it may be inferred, that the relative orbit is seen projected into a straight line or very excentric ellipse ; that an apparent maximum of distance was attained in the end of

the last or the beginning of the present century; and that about twenty years hence the stars will probably be seen very near each other, or in apparent contact, but the data are at present insufficient to give even an approximation to the major axis of the orbit and time of revolution."

The next authority on the subject is Sir JOHN HERSCHEL, who specially applied himself to the subject of the Southern double stars when at the Cape, and had far superior instruments for such a purpose to any of his predecessors; he thus describes and sums up all that was known to him of this star, in his recently published work.

"This superb double star, beyond all comparison the most striking object of the kind in the heavens, and to which the discovery of its parallax by the late Professor HENDERSON has given a degree of astronomical importance no less conspicuous,—consists of two individuals, both of a high ruddy or orange colour, though that of the smaller is of a somewhat more sombre and brownish cast. They constitute together a star which to the naked eye is equal or somewhat superior to Arcturus in lustre." After describing the magnitude which he considered should be assigned to each, and which agrees more nearly with what I have already stated as being my own opinion, and after giving some optical and physiological reasons which may tend to explain the under-estimation of former observers,—Sir John then cites the fact of the remarkable amount of proper motion of the stars, and says, "This consideration alone suffices to decide us in admitting a binary connection between them, and it will therefore be interesting to see what evidence observation furnishes of orbital motion round their centre of gravity. For this, however, the data are somewhat precarious, as we have, until recently, only catalogued differences of A.R. and Polar distances, from which to calculate the angle of position and distance at the epochs of observation. This done, and the results tabulated, together with my own positions and distances, obtained by direct measurement with the equatorial, we have as follows:—"

Authority.	Epoch of Observation.	Position.		Distance.
Lacaille,	1750	218	44	20.51
(Maskelyne,	1761			15.5)
Fallowes,	1822	209	36	28.75
Brisbane,	1824	215	25	22.45
Dunlop,	1825	213	11	22.45
Johnson,	1830	215	2	19.95
Taylor,	1831	215	58	22.56
Herschel,	1834-68			17.43
	1834-79	218	30	
	1835-86	219	30	
	1837-34	220	42	
	1837-44			16.12

I have inserted here the observation of MASKELYNE in 1761, with which, probably, Sir J. HERSCHEL was unacquainted; it makes an apparently bad figure among the rest, but is by no means to be left out on that account merely, seeing the care and the superior means for that day with which the measures were made.

“Mr FALLOWES’ determinations,” continues Sir JOHN, “in this series, are open to objection, from the decidedly inadequate instrumental means by which they were furnished (a small altitude and azimuth circle). Mr TAYLOR’s results also rest on so few observations, as to entitle them to little weight.

“Though it is obviously impracticable to deduce any elliptic elements from such a series, there are some features which it is impossible not to recognise. There can be no doubt that the distance has gone on decreasing since 1822 at least; and the comparison of the measures least open to objection leads us to conclude that, for the ten years previous to 1838, the rate of decrease was $\frac{7}{13}$, or a little more than half a second per annum, which, if continued, will bring on an occultation, or exceedingly close appulse, about the year 1867. The small amount of variation in the angle of position shews that the plane of orbital motion passes nearly, but not quite through our system, while its actual tendency to increase exemplifies the general law of increase of angular velocity, with diminution of distance. Mr FALLOWES’ distance is probably too great by 3" or 4"; but in the long interval between 1750 and 1822 (at the former of which epochs the distance must have been on the increase), there is room for a very much greater excursion of the small star towards its apparent aphelion, so that, although we are sure that the major axis of the real orbit *must* materially exceed 24", it is impossible to say *how much* it may exceed that limit. Taking, therefore, the co-efficient of parallax for α Centauri, as determined by Professor HENDERSON, at 1", it will follow from what has been said, that the real orbit of one star about the other cannot be so small as that of the orbit of Saturn about the sun, and exceeds, in all probability, that of the orbit of Uranus.

“The plane of the orbit in the case of α Centauri, passing nearly through our system, my method of approximating to the elliptic elements becomes inapplicable, and for their determination, measures of the distance of the stars from each other can alone be relied on. No subject more worthy of continued and diligent inquiry can possibly be urged on the attention of southern astronomers.”

Thus the result arrived at, both by Professor HENDERSON and by Sir J. HERSCHEL, and which, though proved since to be erroneous, would have been probably concluded by any one else from the same data, seems to be, that the smaller star had been employed during the last century in gaining its aphelion, without any sensible change of angle of position; what the aphelion distance, the diameter of the orbit, and the period of revolution, might be, no guess could be attempted: but in his address, on the occasion of giving the gold medal to BESSET for his discovery of the parallax of 6 Cygni, Sir JOHN HERSCHEL stated,

that the orbit of the smaller star of α Centauri might subtend the large angle of about 1 minute. As it had been actually observed at an elongation of $28''$ on one side of the large star, the very reasonable supposition of a nearly circular orbit, seen in profile, would, in course of time, give the same distance on the opposite side. Both authorities also predicted the probability of an appulse of the same stars somewhere about the year 1867.

At the time of Sir JOHN HERSCHEL going to press, he knew of no micrometrical measures subsequent to 1838, but soon after that period, most fortunately for the interests of sidereal astronomy, Captain JACOB came into the field. On visiting the Cape from India, where he had been engaged in the great Trigonometrical Survey, he spent most of his time at the Observatory, and not only witnessed, but took part in the parallax observations of α Centauri. He then ordered a good achromatic telescope from Dollond, and on its arrival in India, after his return there, erected a small observatory, and devoted all his spare time with great perseverance and eminent success to that most difficult species of observation,—viz. the double stars.

About a year ago, he wrote to me to send him out all the old observations known of α Centauri, for the two stars were approaching more and more rapidly, and his own observations seemed to give a most unexpected orbit. The first document which reached him was Professor HENDERSON's memoir on the parallax, and then Captain JACOB found that he had been forestalled as to the actual facts of an appulse being shortly to be expected, though he indeed fixed the time as being very much closer at hand, bringing it from 1867 to 1851; but as to the idea that the small star had only been gaining its aphelion, without sensible alteration of angle of position since 1751,—he found, on computing the orbit, that within that interval it had made a whole revolution, or had altered its angle of position by 360° . The subsequent arrival of Sir J. HERSCHEL's observations fully confirmed Captain JACOB's views, who has now recomputed the orbit, including all the known observations up to the present time; and though this performance is to be considered but a first approximation, still it will probably not be very much altered by future observations in any of the important elements.

The difficulty that might be started at the first mention of this new opinion, would be, that supposing the small star, instead of having remained almost stationary in its orbit for the last 100 years, to have really made a whole revolution,—how came it to pass that every observer in the interval saw it always in about the same position on the west, and never on the east of the large star? This objection is fully met by the extraordinary nature of the orbit, which turns out much more nearly like that of a comet than of a planet, the greatest distance being $21.85''$ and the least $0.5''$, in consequence of which, the small star moves with such surpassing rapidity at its periaster, actually $2^\circ 40'$ per day; that it is but a very short space of time on the eastern side of its primary, and when at its

aphaster on the west, moves again with proportionate slowness, and so is seen there for a long period with hardly any sensible alteration of place. The time of revolution seems to be as short as 77 years; and LA CAILLE and MASKELYNE'S observations, which had before appeared somewhat anomalous, are fully reconciled, as belonging to a former revolution; indeed the small star seems to have been almost in precisely the same situation with respect to the large one when observed by MASKELYNE in 1761, as it appeared to SIR J. HERSCHEL in 1838; and had observations been continued for twelve years after MASKELYNE'S time, our knowledge of sidereal astronomy might have been almost a century in advance of its present position.

Captain W. S. Jacob's Observations of α^1 and α^2 Centauri (A.R. $14^h 29^m 5^s$, N.P.D. $150^\circ 12'$), made at Poonah, Lat. $18^\circ 31' N.$, Long. $4^h 55^m 42^s E.$, with a Five Feet Achromatic Telescope.

Angle of Position of the Two Stars.	Weight of Observations.	Number of Observations.	Magnifying Power.	Distance of the Two Stars.	Weight of Observations.	Number of Observations.	Magnifying Power.	Estimated Magnitudes.	Date.	Remarks.
230.1	8	4	87	11.03	9	4	87	1-2.5	1846, .17	flaring.
232.7	15	6	87	11.03	4	4	87	1-3	.20	do.
233.3	13	5	152	11.09	5	4	152	1-3	.20	do.
232.5	13	5	87	10.70	12	4	87	1-3	.26	definition tolerable.
232.2	12	5	87	10.12	8	4	87	1-3	.61	daylight; dawning.
234.1	21	7	87	9.47	19	6	87	1-3	.65	daylight; definition tolerable.
234.8	12	5	87	9.40	11	4	87	1-3	.97	do.
236.0	12	5	87	9.27	8	4	87	1-3	.99	do.
236.4	18	5	87	9.67	14	4	87	1-3	1847, .03	do. good.
236.0	17	5	87	9.30	18	4	87	1-3	.03	do. tolerable.
				9.35	14	4	87	1-3	.04	do. do.
234.7	12	5	152	9.00	10	4	152	1-3	.26	do.
233.6	12	5	87	9.47	11	4	87	1-3	.36	flaring.
234.0	14	5	152	9.64	11	4	152	1-3	.37	do.
235.9	16	5	152	9.81	13	4	152	1-3	.44	definition tolerable.
236.6	11	5	87	8.25	14	4	87	1-3	.93	daylight; definition tolerable.
237.1	12	5	87	8.25	8	4	87	1-3	.93	do. do.
238.0	18	5	152	7.96	13	4	152	1-3	.96	do. very good.
238.0	13	5	152	8.31	10	4	152	1-3	.97	do. do.
238.1	12	5	152	8.12	11	4	152	1-3	1848, .00	do. fair.
238.1	18	5	152	8.03	14	4	152	1-3	.00	do. excellent.
238.2	19	5	152						.01	do. do.
238.0	21	5	152	7.95	16	4	152	1-3	.01	do. do.
238.3	18	5	152	8.03	14	4	152	1-2.5	.02	do. good.
238.1	21	5	152	8.07	14	4	152	1-2.5	.04	do. do.
238.0	17	5	152	7.78	14	4	152	1-2.5	.05	do. do.
238.3	19	5	152	7.89	16	4	152	1-2.5	.05	do. slightly tremulous.

Captain W. S. Jacob's Orbit of α^1 and α^2 Centauri.

Position of perihelion,	.	.	.	π =	26° 24'
Inclination to the plane of projection,	.	.	.	γ =	47 56
Position of ascending node,	.	.	.	Ω =	86 07
Angular distance of perihelion from node on the plane of the orbit,	.	.	.	λ =	291 22
Eccentricity,	.	.	.	e =	0.950
Epoch of perihelion passage,	.	.	.	τ =	1851.50 year
Periodic time,	.	.	.	P =	77.0 years
Mean motion,	.	.	.	ν =	4°.675
Semiaxis major,	.	.	.	a =	15.50"
				Mass =	$\frac{3}{4}$ of the Solar

Apparent Orbit.

Maximum distance,	.	.	.	=	21.85" at 207.5°
Minimum,	=	0.50 ... 5.0
Greatest daily motion,	.	.	.	=	2° 40'

(See Plate XI.)

We thus have here altogether *one* of the most, if not *the* most, interesting and important sidereal system in the heavens; the only one which can compare with it is γ Virginis, and that has been looked upon as being amongst the double stars, what HALLEY's comet is amongst comets; but though so well and frequently observed of late years, it was not instrumentally measured so early as α Centauri, and it is a much smaller star, with an orbit of only one-fourth the apparent dimensions, and a period of time double the length of its southern rival; so that, while the actual observation for the purpose of carrying theory with fact would be eight times more difficult in the case of γ Virginis, and loaded with eight times the probable error of observation, there is the further objection, that on account of the greater length of the period, but a small portion of the orbit could be determined by one observer, or even by one instrument.

But the crowning importance of the binary system of α Centauri, is the accurate determination of its parallax or distance from us, by the late Professor HENDERSON, as we are thereby enabled to speak, not only of the proportions of the different parts of the orbit, but of their actual size, and the weight of the two bodies. Thus the least distance of these two suns is only half that of the earth from the sun, or a little less than that of Venus, while the greatest distance is a little more than that of Uranus; and the mass of the two stars comes out three-quarters of that of our sun, their distance from us being 226,100 times our distance from the sun.

Well, therefore, may Sir J. HERSCHEL have said, "that no subject more worthy of diligent and continued inquiry can possibly be urged on the attention of southern astronomers."

But the most interesting part of the orbit is still to come, viz., the periaster in 1851, and that this be well observed is indeed to be earnestly hoped, for the period will be an eminently crucial one; it proved so in the case of γ Virginis, imperatively requiring an excessive alteration in all the elements except one, as previously calculated; and in the case of α Centauri, the characteristic features of the orbit are of a much more violently marked nature, besides being represented altogether on a larger scale.

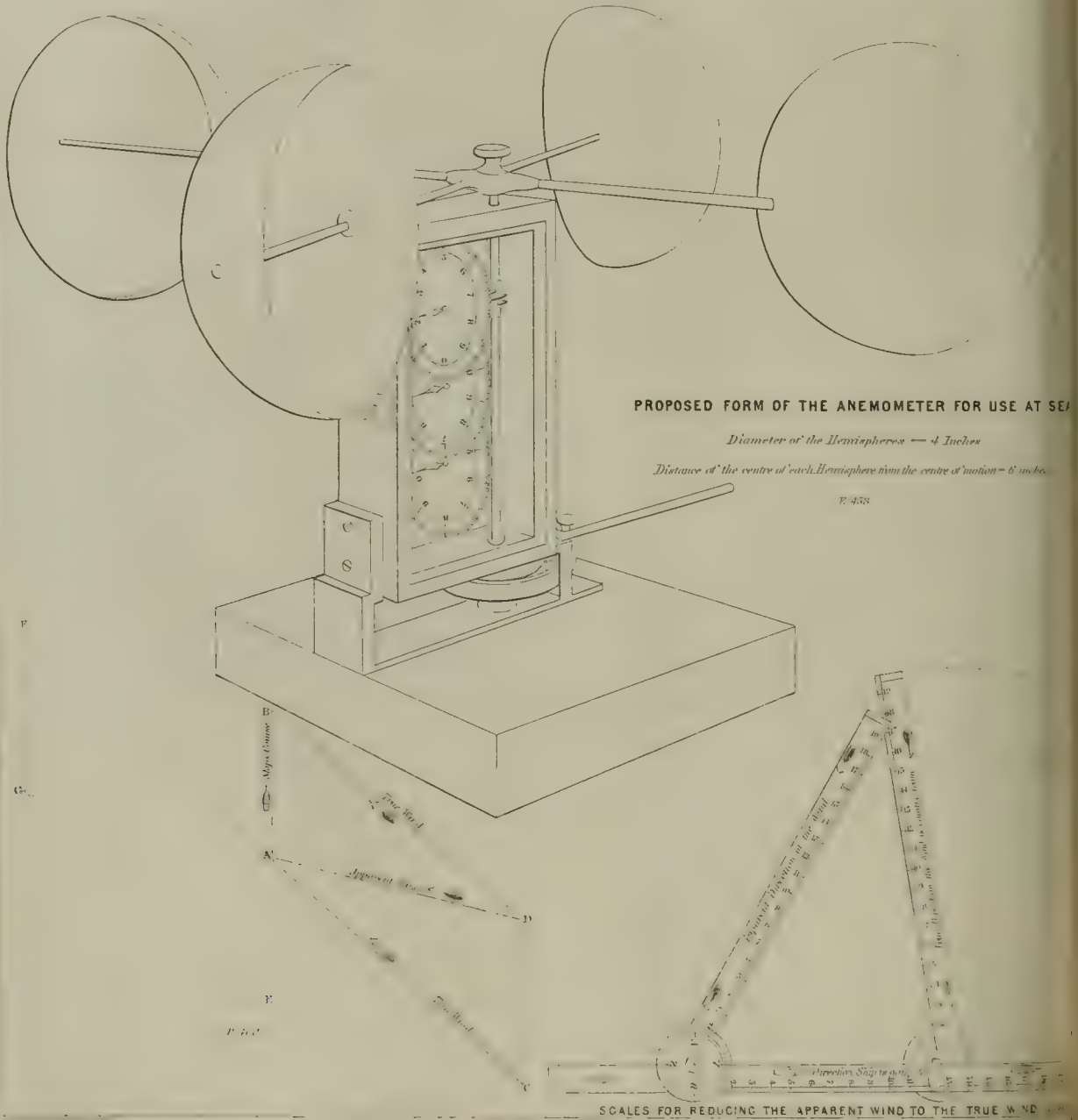
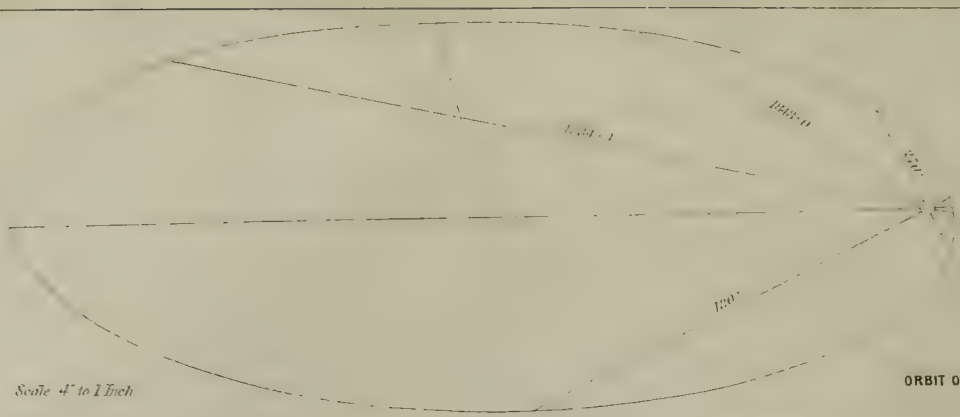
The extreme importance of obtaining an abundance of observations at that epoch may be further indicated by the mere statement, that it cannot yet be considered as fully *proven*, that the law of gravity extends absolutely unaltered to the most distant parts of the sky, and the only mode of proof open to us is by observing the double stars. It is true that most of the orbits yet computed on the theory of gravity have turned out very near the truth, but still not quite so near, it must also be confessed, as could have been desired; and in the luciferous case of γ Virginis, every orbit that has been computed for it yet, has persisted in giving a minimum distance of not less than $0.5''$, while observation at the time of the periastral passage made it certainly much smaller.

I do not, of course, by any manner of means, wish to express any doubt on these grounds as to the sufficiency of gravity to explain all the observed phenomena; a great part of the *onus*, or the whole of it, may rest on the excessive difficulty of the species of observation, and their inappropriateness for calculation in all ordinary manners, caused by the extreme roughness of even the very best procurable data; resembling, indeed, those of the comet of 1556, whose return, calculated on such wretched notices of its former perihelion passage, we have been looking out for in vain so long.

But whatever weight we may attach to the insufficiency of our observations and methods of calculation,* it is always proper to draw a distinct line of demarcation between those things which are proved and those which are merely inferred, and not seek to enjoy a triumph before the victory has been decidedly achieved.

* In a letter just received from Captain JACOB, he says that he thinks he has fallen on the right orbit of γ Virginis at last, having obtained one that expresses all the known observations very well, and gives a minimum distance of $0.23''$.





XXXI.—*An Attempt to Improve the present Methods of Determining the Strength and Direction of the Wind at Sea.* By C. P. SMYTH, Esq., F.R.S.E., Professor of Astronomy in the University of Edinburgh. (With a Plate.)

(Read April 3 and 17, 1848.)

Last year, my friend Captain COCKBURN, R. N., brought to my notice the very lax method which is usually pursued at sea in determining the strength and direction of the wind; and said, that he had for many years been trying to contrive some sort of anemometer that might be useful on board, as well as an easy method of eliminating the effect of the motion of the ship on the true character of the wind, but hitherto without success. I undertook, therefore, to endeavour to supply him with these two desiderata. He thought that they would be useful, in a practical point of view, in seamanship; and as I considered that they might be of importance in meteorology, I was the more ready to lend my assistance.

The foundation of meteorology as a science, may be considered to reside in a knowledge of the general motions of the atmosphere; and these may be far more correctly determined at sea than on any station on land, where local circumstances always produce more or less of an artificial climate, circumscribed, perhaps, to a few miles, or even less, and therefore of no moment to the world at large.

But although a ship, traversing the uniform surface of the ocean, of nearly unvarying temperature, day and night, and winter and summer, is thus naturally under highly favourable circumstances for advancing the science, still those opportunities seem never to have been turned to full account.

The usual mode of entering the wind in the log-book used to be, and may be still in the greater part of the merchant navy, “a hard breeze, or a stiff breeze, or strong breezes, and squally,” &c., &c.; each person judging by his feelings merely, and having a nomenclature of his own for those feelings; so that there is no way of reducing them all to any one uniform natural standard.

On account of the flagrant absurdity of this method, in a scientific point of view, Admiral BEAUFORT, of the Hydrographical Office, proposed and procured the general adoption of, in all Her Majesty's ships, a well-digested table of the names of different strengths of wind, and of the means of judging of them, in the terms of the numbers of which table all entries in the log-book were to be made.

Admiral Beaufort's Wind-Table.

0. Calm.		
1. Light air,	Or just sufficient to give steerage way.	
2. Light breeze, ...	Or that in which a well-conditioned man-of-war, with all sail set, and clean full, would go in smooth water, for	{ 1 to 2 knots. 3 to 4 do. 5 to 6 do.
3. Gentle breeze,...		
4. Moderate breeze,		

5. Fresh breeze, ...	} Or that to which she could just carry in chase, full and by	{ Royals, &c. Single-reefed topsails and top-gallant-sails. Double-reefed topsails, jib, &c. Triple-reefed topsails, &c. Close-reefed topsails and courses.	
6. Strong breeze,			
7. Moderate gale.			
8. Fresh gale,			
9. Strong gale, ...	} Or that in which she could scarcely bear close-reefed main-top-sail and reefed foresail.		
10. Whole gale, ...			
11. Storm,	Or that which would reduce her to storm-staysails.		
12. Hurricane,	Or that which no canvass could withstand.		

But although this was at the time a great improvement on the old system, it is by no means sufficient for the requirements of the present day ; for so much is still left to the feelings and experience of each observer, that one officer will say that the wind should be marked as No. 4 ; while his companion may say it is rather No. 5, and another may decide on 3 being the more appropriate expressive number.

But even supposing that they were all agreed on this point, and said the wind was 4 in strength, no one has ever attempted to determine what *that* number, or *any other* in the table, really means,—what natural strength of the wind, or what velocity of the air, it is equal to. A simple inspection of the table shews that the scale is by no means a uniform one ; for, between Nos. 1 and 2, there can hardly be a difference of $\frac{1}{2}$ lb pressure of the wind on the square foot ; while between 11 and 12, there may be 20 or 30lb difference.

Now, this is an imperfection in the system of the gravest kind, for if the strength of the apparent wind be not observed in such terms as are reducible to those in which the velocity of the ship is measured, the strength of the true wind, or that which a person would feel if perfectly at rest on the ocean, cannot be determined ; and the real motions of the atmosphere would be concealed to a great and unknown extent, by the effects of the movement of the ship.

These defects are of consequence, too, even in ordinary practical matters, as in trial squadrons ; for it is not the *absolute* speed of a ship which is wanted, but the *relative* speed of it, with regard to the wind ; and the winds, blowing at the same time on the various ships several miles asunder, may be of very different strengths ; as any one may prove to himself, by noting the capricious streaks, in which, after a calm, a change sets in on the surface of the water. It is true, that, by keeping the ships out a long time, a mean may be obtained of all these little atmospherical currents, but the result will not be satisfactory ; and, as in the celebrated case of the three trial brigs, there may be a total difference of opinion on the absolute merits of each vessel, by reason of the cause of the apparent superiority, now of one, and now of another, not being properly understood.

These difficulties are all of the character which would be removed by an instrumental method of determining the strength of the wind ; and this is not the first time that the use of an anemometer has been proposed on boardship ; but those which have hitherto been tried would seem to have failed, from not having been of the appropriate kinds ; and partly, indeed, because, in the case of those officers

with whom I have communicated, they seemed to look upon the effect of the motion of the ship as something insuperable, and in the face of which there was no need of aiming at any great accuracy.

On beginning to consider the best species of anemometer for the purpose in question, it appeared to me that something on the principle of the log-line used for determining the ship's speed through the water, would be appropriate; for, notwithstanding the very scientific and accurate character of numerous instruments invented for the same purpose in later years, still they have one by one disappeared, or been forgotten; and the old log-line has not only continued in existence from the earliest times to the present, but ninety-nine out of every hundred ships that now go to sea are furnished with it, and with it alone. This peculiar vitality and power of withstanding the changes of fashions and times, may perhaps depend partly on this, that the quantity to be observed is measured on so very large a scale, that the clumsiest person can read it off to sufficient accuracy; while, with the more modern methods, the accuracy of a person accustomed to delicate observation, is necessary for any trustworthy determination at all.

The case in anemometry, perfectly parallel to the log-line, would be,—to have a float of some sort suspended in the air, and to note how many feet of line it ran out in a certain length of time, under the combined influence of the movements of the air and the ship. But though so perfect an imitation as this is prevented by the rarity of the atmosphere, yet the vane of a horizontal windmill is an approach to the same thing; where the float is supported in the air on a horizontal arm fixed to a vertical axis; and the distance run out, is measured by noting the number of revolutions, and the magnitude of the circle described by the vane or float.

The small motive power, however, of a horizontal windmill, only one-twelfth, according to SMEATON, of the vertical construction, together with the necessity of having a moveable screen to cover up one-half of the wheel from the action of the wind, has prevented the adoption of such a machine as completely for scientific as for industrial purposes.

The vertical windmill, again, though it gains a far greater degree of mechanical power, is also inappropriate for our purpose, on account of the very different amounts of glancing off of the wind, at different velocities, from the inclined surface of the sail; the unavoidable twisting of the necessarily light arms, which prevents the angle of the sail being perfectly constant; and the impossibility of fixing one uniform standard for the shape, size, and angle of the sail; as well as the necessity of having the plane of the sail-wheel always turned toward the direction of the wind.

All these objections have, however, been very happily removed by a novel windmill, of the horizontal form, invented by Mr EDGEWORTH, which requires no screen, but revolves by virtue of the shape of the float-boards; which shape being a constant quantity in all strengths and directions of the wind, the revolution goes

on in the same direction from whatever quarter the wind may come, and increases in rapidity exactly in proportion to the strength of the wind.

Mr EDGEWORTH had proved many years ago the increased effect of wind on a concave surface to a flat one, by taking a sheet of tin, and shewing that, when bent into so very concave a form as to present considerably less area than in its flat condition, to a current of wind blowing straight upon it, that still it experienced a greater degree of pressure. But it was only very recently that he carried the principle farther into a practical form; and the first notice of this is found in Dr ROBINSON's communication to the British Association at Southampton, descriptive of the application to a Whewell's anemometer of one Mr EDGEWORTH's horizontal vane wheels, where each vane consisted of a hemisphere, and the concavities being all turned in one direction, and experiencing one-third more resistance than the convex sides, the whole revolves from the concave as it were to the convex sides, at one-third the rate of the wind.

This, then, seemed to be eminently the sort of anemometer for use on board-ship.

No particulars of size or construction were given, only the important principle involved was mentioned; and as the first instrument which I had made, did not prove sufficiently sensitive, I entered on a course of experiments to ascertain the best sizes of the machine, and shape of the floats; and being greatly assisted therein by the practical skill and ingenious methods of Mr MILNE, the artist employed, was at last enabled to fix definitively on the instrument figured in the accompanying plate as the one which combines the greatest number of advantages, and forms altogether the best standard, perhaps, that can be adopted.

There are 4 hemispherical floats, 4 inches each in diameter, and 1 foot apart from centre to centre: an endless screw, on the axis of the vane-wheel, gives motion to a train of wheels and pinions, which serve to measure the number of revolutions made; $1\frac{1}{2}$ grain, in the centre of one of the floats, is sufficient to produce motion. (See Plate XI.)

If the instrument be made on too small a scale, then it will have to overcome so much larger a proportion of the friction of surface than a larger one, that the floats will not move at one-third the velocity of the wind; but as much of this friction and resistance depends on the linear measure of the parts, while the motive power, which is as the area of the float, increases as the square of the dimensions, it is evident that increased velocity may be obtained by adopting a larger instrument. There is, however, a danger of passing the proper medium again in this way; for, as some of the sources of friction increase according to the *weight* of the moving parts, or as the *cube* of the linear dimensions, these may soon surpass the motive power, which increases as the *square* only. There is another objection also to having a vane-wheel with much mass, or *vis inertiae*; for although we wish in this inquiry to get a mean, or the total sum, of all the little separate gusts of

which any particular wind is made up ; and although, in a mere mechanical point of view, a wheel of great weight would tend to equalize and mean all the currents of different intensity, still it can only do so with a certain amount of loss, and with the total omission of all very light impulses ; and the only way accurately to sum up all those separate little quantities, is to employ an instrument which shall be as sensible as a feather, and take full and *immediate* account of the slightest motion of the atmosphere.

After trial of floats 2, 3, $3\frac{1}{2}$, 4, and 6, inches in diameter, the 4-inch ones were considered as being the best ; and the hemispherical shape was also preferred, as giving the greatest per centage of velocity with the least weight of material and smallest side resistance, as well as offering the shape, of all others, of the easiest and truest execution, and best understood everywhere.

Various experiments were tried, of making the floats more or less conical, in order to diminish the pressure of the wind on their backs ; but though that point was most eminently obtained, still the advantage was outweighed by the necessary increase of weight accompanying the increase of surface, the greater side resistance to the wind, and the diminished pressure on the concave side.

In the month of January this year, I had the opportunity of trying the value of the revolutions of this anemometer, in company with Captain COCKBURN. The instrument was mounted on the top of a cab, clear of the driver's head, and driven at a pretty uniform speed of above seven miles an hour, first forwards and then back, on two miles of the London Road ; the object being to measure the artificial wind produced by the motion of the vehicle, which would of course be equal to a natural wind blowing with the same velocity in the contrary direction. The first day there was a rather strong breeze, which would have completely vitiated the experiments, but that, as it was blowing almost exactly in the direction of that part of the road which was traversed, we expected to be able to eliminate its effects by taking a mean of the numbers given in going and returning.

When going, having the wind with us, the instrument, which measured then only the difference between the velocities of the wind and the cab, made 209 revolutions in one mile ; but in returning, measuring the sum, it gave 921 revolutions in the same distance. The mean of these, or 565, when multiplied by $3\cdot1415$ feet, or the space described by the centre of the float in one revolution, gives a velocity not exactly $\frac{1}{3}$, but $\frac{1}{2\cdot974}$ of that of the wind.

The second day was all but perfectly calm ; it was at the commencement of the long-continued frosty weather ; and a proof of the general stillness of the air was offered in the dense, unnatural manner in which the smoke was accumulated and remained suspended over the city. In going out, 558 revolutions were made in one mile ; and in coming back, 551. The mean of these, or 555, gives a velocity

of the floats of $\frac{1}{3.028}$; and the mean of both days makes it $\frac{1}{3.001}$, or almost exactly what Dr ROBINSON stated that it should be from theoretical investigation.

The result was, so far, perfectly satisfactory, and seemed to shew that the instrument was fully entitled to be tried at sea, as giving a good and convenient measure of the velocity of the wind. The particular proportion mentioned ($\frac{1}{3}$) might probably not obtain equally under all velocities of wind, but it has not been thought worth while to try the instrument at other velocities on shore; because there are much more powerfully modifying circumstances in the rolling motion of the ship, the full effect of which can only be determined by actual experiment at sea. But whatever alteration of the value of the revolutions takes place under such conditions, naval officers may be assured of this, that a certain physical connection between the velocity of the air and the revolutions of the anemometer exists, and its exact nature may be easily investigated and discovered, and the strength of the wind may then be entered in the log-book, in numbers expressive of the velocity of the air in knots per hour, or in the same terms as the motion of the vessel; and as the direction of the wind is already sufficiently well observed by the different vanes at present in use on board ship, all the elements of the *apparent* wind may, with the assistance of this anemometer, be looked upon as satisfactorily obtained.

This apparent wind being, however, the combined effect of the motions of the ship and of the air, may be very different from the true wind, both in direction and in strength. When the ship is going with the wind, the velocity of the true wind will be equal to the sum of the velocities of the ship and of the apparent wind; and when going against the wind it will be equal to their difference, without alteration of direction in either instance. But in almost every other possible case both the velocity and the direction will be changed, the problem being a particular application of the well known and important theorem in mechanics of the parallelogram of forces; the velocity of the ship observed forming one side, the velocity of the apparent wind—also obtained from observation—being the diagonal, and the true wind to be determined, another side.

This may be illustrated familiarly as follows:—Let the line A B (Plate XI.) represent the motion of the ship, the length of the line shewing the velocity in knots per hour as determined by the log-line, and the position of the line shewing the vessel's course, or the direction *to which* the vessel is proceeding, obtained by reference to the vanes and the compass. Similarly, let the lines A C or B D represent the true wind, or the wind which a person at rest would feel to be blowing over the sea during the time that the ship passed from A to B. The length of the lines A C and B D shew the velocity of the wind in knots per hour, given by the anemometer, and the position of the lines gives the direction *from which* the wind is coming.

Under such circumstances, what will be the apparent wind, or, in other words,

what will be the direction and the velocity of the wind which a person in the ship will feel?

Now, the motion of the ship from A to B being equivalent to a wind moving with equal velocity in a contrary direction, or from B to A, then any particle of air at rest at A will be driven, if acted on by the impact of this adventitious wind only, to a distance, equal to A B, beyond A, and in the same direction, or to E; but if acted on only by the natural wind, the particle will receive a velocity and a direction equal to that, and be driven from A to F; but as the particle is acted on instantaneously by both forces, it will neither go to E nor to F, but in an intermediate direction, and closer to one or other according to the relative strength to the two forces, or in the diagonal of the parallelogram of which A F and A E form two sides. A G, therefore, in the parallelogram A E F G, represents, by its length and position, the distance to which, and the direction in which, a particle at A will be driven under the united influence of the natural wind blowing over the sea, and the artificial wind caused by the motion of the ship; *i. e.*, A G represents the apparent wind, or that which a sailor would observe.

For convenience of illustration we may complete the parallelogram A B D C, which is similar to the parallelogram A E F G, and where A D is consequently equal to A G; and we may now, for the purposes of calculation, omit all but the triangle A B D, in which the sides A B, A D, being given by observation, and the included angle B A D being obtained from the difference of the observed directions of the ship and of the apparent wind, we have merely to compute by the usual rules for plane triangles, the length of the side B D, or the velocity of the true wind; and the angle A B D, which, being added on to the course of the ship, gives the direction of the true wind.

Considering, however, the necessary length of time which such a computation must occupy in the hands even of the most expert (the mere preparation of the angles for computation would of itself be no small matter, on account of the rough character of the subtraction of the course of the ship from the direction of the wind—both observed by compass points—the reduction of this to degrees, then the re-reduction of the resulting angle into points, and the addition of them to the course of the ship): considering, also, the almost infinite number of times the calculations would have to be gone through in any voyage, and the extreme improbability of any amateur undertaking so large a quantity of an unimproving sort of labour, I have had a set of scales made for solving the problem by inspection, and the entry being made with the velocities and *directions* observed, the velocity and *direction* of the true wind are immediately given.

These scales, which are represented in Plate XI., are formed of two ordinary jointed rulers, having moveable circles marked with the points of the compass on the joints, and three sets of divisions on the legs, to include all possible velocities

of the wind and ship, from 1 mile to 100 per hour, each mile being capable of subdivision into tenths.

A leg of each scale being made to slide one upon another, with the joint of each turned outwards, the centres of these joints are to be placed at such a distance apart as corresponds to the velocity of the ship in terms of any one of the three sets of divisions on the legs. Both circles are then turned, so as to shew on one edge of that double leg the observed direction of the ship's course; the single projecting leg of one scale being then placed in the observed direction of the apparent wind, and the projecting leg of the other moved until one edge of it cuts on the first, the divisions indicating the velocity of the apparent wind; then the length of the intercepted portion of that second leg shews the velocity of the true wind, and its reading on its own circle gives the direction.

The circles are graduated to degrees as well as to points of the compass, so as to be capable of solving all ordinary cases of plane triangles, as well as those immediately contemplated, and to suit the instances sometimes found of the more exact determination of the direction of the wind in degrees instead of points.

As the scales stand at present, they require, when the significant numbers of the anemometer shall have superseded the present unmeaning ones entered in a ship's log, no new data from observation; but by merely running down, scale in hand, the columns of "Directions and Velocities of the Ship, and Apparent Wind," as entered in the book for the ordinary purposes of navigation, the direction and velocity of the true wind may be rapidly entered in a couple of adjacent columns; and the importance of making this correction before beginning to reason, from the data at present afforded by ships, on the general character of the winds on the surface of the ocean, may be seen in the accompanying supposed extract from the journal of a man-of-war or a steamer; where, though the true wind was the same during the whole interval, yet, on account of the different courses pursued, and velocities attained, by the ship at the various hours, the wind did alter to those on board at times, almost of 90° in direction, and from 9 knots to 23 knots in velocity.

Date.		Ship's		Apparent Wind.		True Wind.	
1848.	Hour.	Course (to)	Velocity.	Direction (from)	Velocity.	Direction (from)	Velocity.
April 17	1	N.	Knots. 10·5	WSW.	Knots. 12·1	SW. by S.	Knots. 19·3
	2	NNE.	10·4	SW.	9·3	SW. by S.	19·3
	3	NE.	10·0	SSW.	9·7	SW. by S.	19·3
	4	E. by N.	11·5	S.	14·0	SW. by S.	19·3
	5	ESE.	11·8	S.	19·9	SW. by S.	19·3
	6	SSE.	5·2	SSW.	23·3	SW. by S.	19·3
	7	W.	4·1	SW.	22·6	SW. by S.	19·3
	8	WNW.	5·8	SW.	21·8	SW. by S.	19·3
	9	NW.	8·4	SW. by W.	19·3	SW. by S.	19·3
	10	NW. by N.	14·0	W. by S.	18·7	SW. by S.	19·3
	11	N. by W.	10·0	WSW.	14·1	SW. by S.	19·3
	12	WSW.	3·3	SW. by S.	22·8	SW. by S.	19·3

XXXII. *On the Products of the Destructive Distillation of Animal Substances.*

Part I. By THOMAS ANDERSON, Esq., M.D.

(Read 3d April 1848.)

In April 1846, I communicated to the Royal Society a paper on a new organic base, to which I gave the name of Picoline, and which occurs in coal-tar, associated with the Pyrrol, Kyanol, and Leukol of RUNGE. In that paper I pointed out that the properties of picoline resembled, in many respects, those of a base which UNVERDORBEN had previously extracted from DIPPEL's animal oil, and described under the name of Odorine; and more especially mentioned their solubility in water, and property of forming crystallisable salts with chloride of gold, as characters in which these substances approximated very closely to one another. And further, I detailed a few experiments on the odorine of UNVERDORBEN extracted from DIPPEL's oil, with the view of ascertaining whether or not they were actually identical, but on too small a scale to admit of a definite solution of the question.

These observations, coupled with the doubts which had been expressed by some chemists, and more especially by REICHENBACH, as to the existence of the bases described by UNVERDORBEN, induced me to take up the whole subject of the products of the destructive distillation of animal substances, which has not yet been investigated in a manner suited to the requirements of modern chemistry. In fact, UNVERDORBEN is the only person who has examined them at all, and his experiments, contained in the 8th and 11th volumes of POGGENDORF's *Annalen*, constitute the whole amount of our knowledge on the subject; and his observations, though valuable, and containing perhaps as much as could easily be determined at the time he wrote, are crude and imperfect, when we come to compare them with those which the present state of the science demands. Since his time, the methods of investigation in organic chemistry have undergone an entire change: the simplifications of the process of organic analysis had not then been made, or at least had not come into daily use as the auxiliary of investigation, and UNVERDORBEN, who belonged to the old school, and contented himself with the observation of reactions only, was necessarily led, as I shall afterwards more particularly shew, to confound together substances, the reactions of which approximate so closely that it is impossible, or at least very difficult, to distinguish them by such means alone. The errors, however, lay with the method, and not with the observer; for UNVERDORBEN's experiments, so far as they go, I have found to

be correct in the main, notwithstanding their having been called in question by REICHENBACH, whose numerous researches on the kindred subject of the products of the destructive distillation of vegetable substances, gave weight to his opinion, and have indeed been the principal cause of the doubts expressed by others on the subject.

The investigation of these products has occupied me pretty continuously since the publication of the paper before alluded to; and my researches have now extended themselves over a large part of the subject, although, from its branching off into so many subdivisions, and embracing the consideration of so large a number of substances belonging to almost every class of organic compounds, some time must still elapse before it is complete in all its parts. It is my intention, therefore, as the subject naturally divides itself into several sections, to take up the consideration of these in a succession of papers, of which the present is the first, and in which I propose to consider the general properties of the crude product employed in my experiments, and those of certain of the organic bases contained in it.

The products of the destructive distillation of animal matters were long since employed in medicine, and were obtained from all parts of the body, and from almost every section of the animal kingdom; but these afterwards entirely gave way to the *Oleum Cornu Cervi*, which, as hartshorn is entirely free from fatty matters, must necessarily be the pure product of decomposition of the gelatinous tissues. The more volatile portions of this oil, separated by distillation with water, and purified by numerous rectifications, constituted the *Oleum Animale Dippellii* of the older pharmacopœias. These substances would, in all probability, have been the most convenient crude materials for my experiments; but as they have long since ceased to be employed, and cannot now be obtained except by going through the tedious and disagreeable process for their preparation, I have made use of the bone-oil of commerce, which is prepared on the large scale by the distillation of bones in iron cylinders, and can be had in any quantity from the manufacturers of ivory-black. This oil appears to differ in no respect from the true *Oleum Cornu Cervi*, and, like it, is the product of decomposition of the gelatinous tissues only; for previous to distillation the bones are boiled in a large quantity of water, by which means both the fatty matters and also a certain proportion of the gelatine are separated. They are then dried, packed in the cylinders, and distilled at a heat which is gradually raised to redness. The oily product of this distillation is separated from the watery portion, and, after rectification, forms the bone-oil of commerce; though in some instances this latter distillation is dispensed with, and the product of the first made use of without any further purification.

Bone-oil, as supplied by the manufacturer, has a dark-brown, almost black colour, with a somewhat greenish shade, and perfectly opaque in the mass; but when

spread in a thin layer on a glass plate, it is seen to be brown by transmitted light. Its specific gravity is about 0.970. Its smell is peculiarly disagreeable, and is mixed with that of ammonia, which is always present, though sometimes in so small quantity that its odour is disguised by that of the oil itself, and is only rendered apparent by distillation. A piece of fir-wood moistened with hydrochloric acid, and held over the mouth of a vessel containing it, rapidly acquires the dark reddish-purple colour which is characteristic of the pyrrol of RUNGE. Acids agitated with the oil acquire a brown colour, especially on standing, and extract the bases contained in it; but if the quantity of the acid be large, and in a pretty concentrated state, a nonbasic oil is also dissolved, which, on standing for some time, and more rapidly if heated, undergoes decomposition, and the fluid becomes filled with orange-yellow flocks of a resinous substance, which acquires a dark colour by exposure to the air; this change is produced by the stronger vegetable as well as the mineral acids. Alkalies extract an acid oil, and a considerable quantity of hydrocyanic acid, which, on the addition of an acid to the alkaline solution, and distillation, can be distinguished in the product by its smell, as well as by its reaction with the salts of iron.

Previous to the separation of the bases, the crude oil was again rectified in portions of about fifteen pounds each, in an iron retort,—an operation attended with some trouble, as the fluid is apt to froth up and boil over in the early part of the process, so that the retort must not be more than half full, and the heat requires to be applied in a very gradual manner. At first a watery fluid distils, containing in solution ammonia, and a small quantity of the most volatile bases. This is accompanied by an oil of a pale yellow colour, limpid and very volatile, which after a time comes over without water, and with an increased though by no means dark colour. The distillation proceeds in a perfectly steady and gradual manner, until about two-fifths of the oil have passed over, when a point is attained at which the temperature requires to be considerably raised, in order that the distillation may continue uniformly, and the product becomes much thicker and more oily in its appearance. At this point the receiver was changed for the purpose of collecting the less volatile portion apart, and the distillation continued until the bottom of the retort reached a red heat. The latter portions of these products were obviously altered during the distillation, for a bulky porous charcoal remained in the retort; the oil which passed over smelt strongly of ammonia, crystals of carbonate of ammonia made their appearance in the neck of the retort, and a certain quantity of water collected in the receiver. The oil also became gradually darker in colour, and more viscid in its consistence. By collecting in a succession of receivers, I had an opportunity of observing a great number of curious optical phenomena at different epochs of the distillation. The oils frequently presented well-marked appearances of epipolic dispersion, and the very last portion exhibited a curious species of dichroism, its colour being dark reddish-

brown by transmitted, and green, with the effect of opacity, by reflected light. All these appearances, however, were very evanescent, and are only seen in the newly distilled oil, for after a few days it becomes very dark coloured, and they are then no longer visible.

Both the more and the less volatile oils contain a variety of bases, and were separately treated for their extraction. In neither, however, is the quantity large. I obtained from the more volatile portion of three hundred pounds of bone-oil less than two pounds of the mixed bases; but as, in the course of the various processes to which it was submitted, some small portions were lost, the whole may perhaps amount to about three-fourths per cent. of the total quantity of oil. The less volatile portion yields a larger quantity, which may be estimated at two or three per cent. of the crude oil. These, of course, are only rough estimates, but they may serve to give an idea of the quantity of the products.

Preparation of the Bases.

For the preparation of the bases precisely the same processes were followed throughout for both portions into which the oil was separated by distillation; and as the bases to be described in the present paper were contained in the more volatile portion, I shall detail the steps followed in reference to that quantity only. The oil was mixed in a cask with sulphuric acid diluted with about ten times its weight of water, and the fluids left in contact for a week or two, during which time they were frequently agitated. More water was then added, and the whole drawn off, and the process repeated with fresh quantities of sulphuric acid as long as any bases were extracted. The solution, which had a reddish and sometimes very dark brown colour, contained the bases, along with a quantity of nonbasic oil and of pyrrol. It was mixed with an additional quantity of sulphuric acid, introduced into a glass distilling apparatus, and heat applied. As the fluid approached the boiling point, a quantity of the red resinous matter before alluded to began to separate, and occasioned succussions of so violent a character as to endanger the safety of the vessel, and render it necessary to interrupt the process for the purpose of filtering it off, after which the distillation proceeded without difficulty. A small quantity of oil distilled over, and the water which accompanied it had exactly the smell of the water in a gas-meter, and contained pyrrol,* which continued to pass over for a long time, during the whole of which the distillation was continued. This distillation I had recourse to at first, from a suspicion that some of the bases were separated from the acid, and volatilised during the process; but so soon as I had ascertained that this was not the case it was

* These odours were so exactly alike, that I was induced to seek for pyrrol in the water of gas-meters, and I found that when mixed with sulphuric acid and distilled, the product gave the characteristic reaction of pyrrol with fir-wood. Ammonia remained in combination with the sulphuric acid.

dispensed with, and the fluid boiled down in porcelain or copper evaporating basins, water being added, and the distillation continued until, by taking a small quantity of the fluid and distilling off a few drops in a retort, they were found to be free from pyrrol. Even this precaution soon became unnecessary, for a little experience enabled me to know when the evaporation had been sufficiently prolonged.

The dark-brown fluid which remained in the basins was once more strained, in order to get rid of such resinous matters as might have separated during the evaporation, and then distilled in a large glass balloon connected with a condenser, after the acid had been previously supersaturated by a base. For this purpose, potass, soda, and lime were indifferently made use of: the latter answers extremely well, but, owing to the large quantity of sulphate of lime separated, the distillation requires to be carried on in the chloride of calcium or oil bath. When the alkali is added in sufficient quantity, an oil floats up to the surface of the fluid, and a strong pungent odour is given off, in which that of ammonia is apparent, along with another which can be compared to nothing but the smell of stinking lobsters. At the first part of the distillation a transparent and colourless watery fluid passed over, which contained the bases in solution; but after this had continued for some time, an oil made its appearance running in globules down the tube of the condenser, and dissolving immediately in the fluid which had already distilled. When the bases ceased to distil in quantity, the receiver was changed, and a small quantity of oil heavier than water was obtained by continuing the distillation for some time. At the end of the process an oil remained floating upon the concentrated fluid in the balloon, the quantity of which is very variable, and depends on the distillation of the crude bone-oil, having been continued too long before changing the receiver. In fact, it contains some of the bases of the less volatile oil, and will come to be considered in an after part of the investigation.

To the product of the distillation sticks of caustic potass were added, and, as these dissolved, the oily bases separated from the fluid in a manner exactly similar to that which was observed in the preparation of picoline, as detailed in the paper to which reference has already been made. The alkaline solution was drawn off by means of a siphon, and more potass added as long as water was separated. In this way the greater part of the base was obtained, but a small quantity of the most volatile of all still remains in the alkaline solution, and cannot be separated except by the addition of a very large quantity of potass. It was, however, readily obtained by distilling the fluid, and collecting only the first portion of the product, from which it was separated by a comparatively small expenditure of potass. The small quantity so obtained was preserved separately from the large mass.

The product of this operation was found to be extremely complex, and to consist of a mixture of four or five different bases, exclusive of ammonia. For the purpose of obtaining these in a separate state a great variety of processes was attempted, but none were found to answer so well as fractionated distillation, although it is an extremely tedious method of separation, and occasions a considerable loss of substance, which is very annoying when the quantities obtained are so small. When the mixed bases were distilled with a thermometer, ammonia began to escape at a very low temperature; but at 160° Fahr. the fluid entered into steady ebullition, and a perfectly transparent and limpid oil began to distil. A small quantity of oil passed over between this temperature and 212° , which was received by itself, and the after products collected in a succession of receivers, which were changed at every ten degrees which the thermometer rose. The fluid continued in steady and rapid ebullition, and the thermometer ascended rapidly to 240° ; and between that and 250° a considerable quantity was collected. It then again went up pretty rapidly, and another large quantity was obtained between 270 and 280 ; after which the distillation proceeded more slowly until the temperature rose to 305° , at which point the characters of the products underwent a complete change. All the substances obtained at lower temperatures dissolved instantaneously in water; but that which now distilled floated on the surface, and only dissolved on agitation with a considerable quantity of water. Distillation now continued with somewhat greater rapidity, till the thermometer rose to about 355 , when a drop of the product allowed to fall into a solution of chloride of lime immediately gave the reaction of aniline. When this was observed, the whole remaining products, which formed only a small fraction of the whole, were collected together. They consisted chiefly of aniline.

The products of these different distillations were repeatedly rectified, and by this means bases were obtained, corresponding to the points at which the thermometer was found to remain longest in the first distillation. Of these I have as yet examined only the most volatile, and that which boiled at about 270° .

Petinine.

The most volatile portion of the bases obtained by the fractionated distillation, was mixed with the small portion which was separated with difficulty from the potash solution, and had been kept separate from the large quantity. The mixed fluid still contained a large quantity of ammonia, for the separation of which it was again rectified several times in succession, and fractionated in a small retort, the receiver being kept carefully cool. After this process has been repeated until it is properly purified, it constitutes the base to which I give the name of Petinine (from *πετρεος*, *volutilis*), in allusion to its volatility, which is greater than that of any base yet known, with the exception, of course, of ammonia. The quantity of this substance contained in the bone-oil is excessively small, as I obtained from

three hundred pounds no more than was sufficient for the determination of its constitution, and the leading characters of a few of its compounds. It is probable, however, that some loss was incurred in the preliminary processes, as, from not anticipating the presence of so volatile a base, I did not take any precautions for producing complete condensation of the products, by means of freezing mixtures or otherwise; and a considerable quantity was also lost owing to my anxiety to expel completely the ammonia which it might retain.

Constitution of Petinine.

The petinine employed for analysis was very carefully dried over caustic potass, the fluid poured off after some days' contact, and distilled in the water-bath at a very gentle heat; a precaution which is rendered necessary by its dissolving a certain quantity of potass. I did not possess a sufficient quantity to make a determination of the nitrogen, but took it for granted that oxygen was absent; an assumption which is justified by the analogy of all the other volatile bases, as well as by the perfect coincidence of the experimental results with the calculated formulæ. It was analysed with oxide of copper in a very long tube, and gave the following results:

$$\left\{ \begin{array}{l} 6.663 \text{ grains of petinine gave} \\ 16.286 \quad \dots \quad \text{carbonic acid, and} \\ 8.382 \quad \dots \quad \text{water;} \end{array} \right.$$

corresponding exactly with the formula $C_8H_{10}N$, as is shewn by the following comparison:

Experiment.		Calculation.		
Carbon,	. . . 66.66	66.66	C_8	600.0
Hydrogen,	. . . 13.97	13.88	H_{10}	125.0
Nitrogen,	. . . 19.37	19.44	N	175.0
	<hr/> 100.00	<hr/> 100.00		<hr/> 900.0

In order to ascertain the atomic weight of petinine, I prepared its compound with chloride of platinum, and made the following determinations of the platinum contained in it:

- I. $\left\{ \begin{array}{l} 6.351 \text{ grains of chloride of platinum and petinine gave} \\ 2.245 \quad \dots \quad \text{platinum} = 35.34 \text{ per cent.} \end{array} \right.$
- II. $\left\{ \begin{array}{l} 3.860 \text{ grains of chloride of platinum and petinine gave} \\ 1.372 \quad \dots \quad \text{platinum} = 35.54 \text{ per cent.} \end{array} \right.$
- III. $\left\{ \begin{array}{l} 2.844 \text{ grains of chloride of platinum and petinine gave} \\ 1.010 \quad \dots \quad \text{platinum} = 35.51 \text{ per cent.} \end{array} \right.$

The atomic weights deduced from which agree very closely with the calculated results:

I. Atomic weight, by experiment,	.	.	.	910.3
II.	891.2
III.	894.2
Mean,	898.5
Calculation,	900.0

The mode in which this base is formed during the decomposition of gelatine, it is, of course, impossible at present to perceive. In its chemical relations it is, however, in all probability, related to the butyric series; and it is even possible that we may obtain it by artificial processes. Some time since, KOLBE* published some researches on the galvanic decomposition of valerianic acid, among the products of which he discovered a carbo-hydrogen, having the formula C_8H_9 . Now, by treating this substance in the same manner as benzine is acted upon for the preparation of aniline, we ought to obtain from it, if not petinine, at least an isomeric compound, as may be easily seen by comparing the formulæ of the different substances:

Benzine, . . .	$C_{12}H_6$	C_8H_9	KOLBE's carbo-hydrogen.
Nitro-benzide, .	$C_{12}H_5(NO_4)$	$C_8H_8(NO_4)$	Action of nitric acid.
Aniline, . . .	$C_{12}H_7N$	$C_8H_{10}N$	Petinine.

I have not yet had an opportunity of determining whether the change which theory would lead us to expect actually takes place, but there is every reason to suppose that it would.

Properties of Petinine.

Petinine is a transparent colourless fluid, limpid as ether, and possessing a high refracting power. It has an excessively pungent odour resembling that of ammonia, and yet quite distinct, for when the effect of its pungency has gone off, or it is smelt in a dilute state, its smell is disagreeable, and somewhat similar to that of decayed apples. Its taste is hot and very pungent. It boils at a temperature of about $175^\circ F.$; but the quantity which I possessed was too small to admit of an accurate determination either of this point, or of its specific gravity, although the latter is certainly less than that of water. Petinine is a very powerful base, and immediately restores the blue colour of reddened litmus, and gives abundant fumes, when a rod dipped in hydrochloric acid is held over it. It unites with the concentrated acids, with the evolution of much heat. It dissolves in all proportions in water, alcohol, ether, and the oils; and is also soluble in dilute solution of potass, but not in concentrated. Petinine gives double salts with bichloride of

* Memoirs of the Chemical Society of London, Part xxi.

platinum and corrosive sublimate, both of which are soluble in water. With chloride of gold it gives a pale yellow precipitate, which does not dissolve on boiling the solution, and is not deposited in crystals. Petinine throws down peroxide of iron from its compounds. It also precipitates salts of copper, and the oxide thrown down dissolves in excess of the base with a fine blue colour.

These properties agree with those of none of the bases described by UNVERDORPEN; and, in fact, it is certain that petinine could not have been present in the mixture to which he applies the name of odorine, for he expressly states that it commenced boiling at 212° . And it is easy to see why he did not obtain it, because, in separating the bases from the acid by which they were extracted from the crude oil, he took care to add a quantity of potass just so great that the oily bases were liberated, and not the ammonia; and as his object in doing so was to get rid of the latter substance, and there being no means of doing this exactly, it is probable that he did not fully separate the bases, but the most volatile, which is also the most powerful, remained in combination with the acid along with the ammonia.

Compounds of Petinine.

The minute quantity of petinine which I obtained has necessitated a very cursory examination of its salts, which are interesting, both from the facility with which they crystallise, and their great stability. None of them undergo change in the air, but may be left exposed for any length of time without acquiring colour. They are all soluble in water, and those with the volatile acids sublime without decomposing, and are deposited in crystals upon cold surfaces.

Sulphate of Petinine, is obtained by adding petinine to dilute sulphuric acid until the fluid is neutral. On evaporating, petinine is given off, and the solution, when concentrated to a syrup, concretes on cooling into a foliated mass of crystals of an acid sulphate. These crystals are strongly acid to test-paper, extremely soluble in water, and slightly deliquescent in moist air.

Nitrate of Petinine.—The solution of petinine in nitric acid, evaporated to dryness, and gently heated on the sand-bath, gives a sublimate of the nitrate in fine woolly crystals.

Hydrochlorate of Petinine.—Hydrochloric acid combines with dry petinine, with the evolution of much heat, and the formation of a salt which is extremely soluble in water, and sublimes in fine needle-shaped crystals.

Chloride of Platinum and Petinine.—If bichloride of platinum be added to a dilute solution of hydrochlorate of petinine, the salt formed remains in solution; but when both substances are concentrated, it falls as a pale yellow precipitate, which was purified by crystallisation from hot water. On cooling, the fluid, if sufficiently concentrated, becomes entirely filled with exceedingly beautiful golden-yellow plates, resembling those of crystallised iodide of lead. It is pretty soluble

in cold water, extremely so in hot, and is not decomposed by boiling the solution. It is also soluble in alcohol.

$$\left\{ \begin{array}{l} 9.552 \text{ grains of chloride of platinum and petinine gave} \\ 5.930 \quad \dots \quad \text{carbonic acid, and} \\ 3.593 \quad \dots \quad \text{water.} \end{array} \right.$$

By three determinations of platinum, the details of which have been already given, the mean per-centage of platinum was found to be = 35.46.

These results correspond with the formula $C_8 H_{10} N, H Cl, Pt Cl_2$.

Experiment.		Calculation.			
Carbon,	16.93	17.26	C_8	600.0	
Hydrogen, . . .	4.17	3.96	H_{11}	137.5	
Nitrogen,	5.04	N	175.0	
Chlorine,	38.29	Cl_3	1330.4	
Platinum, . . .	35.46	35.45	Pt	1232.0	
		100.0		3474.9	

Chloride of Mercury and Petinine.—A solution of petinine in water, added to a solution of corrosive sublimate, gives a white precipitate, which dissolves in a considerable quantity of hot water, from which it is again deposited in crystals. It is much more soluble in alcohol; and the boiling solution gives a deposit of beautiful silvery plates on cooling. It is decomposed by boiling its watery solution, petinine being driven off, and a white powder deposited. It is readily soluble in the cold in dilute hydrochloric acid, probably with the formation of another double salt.

Products of Decomposition of Petinine.

The want of substance, which prevented the full investigation of the salts, has likewise curtailed this branch of the subject to a very few observations, which is the more to be regretted, as the general properties and low atomic weight of petinine give promise of definite products, which might enable us fully to determine its position in the chemical system.

When treated with concentrated nitric acid, it dissolves without any remarkable phenomena, and, on boiling, a feeble evolution of nitrous fumes takes place; but the petinine is attacked only to a very small extent, for, after being kept boiling for a long time, and then supersaturated with potass, it evolved the smell of the base apparently unchanged. Solution of chloride of lime immediately acts upon it in the cold, and develops a highly irritating odour, and some compound is manifestly produced; the solution remains colourless. Bromine water dropped into an aqueous solution of petinine occasions the precipitation of a yellow oil

heavier than water, and insoluble in acids,—the solution contained hydrobromate of petinine. From the analogy of the other volatile bases, we should expect this to be tribromopetinine, $C_8H_7Br_3N$. My material being exhausted, I was not able to extend these observations further.

Picoline.

Having determined the properties of petinine, I next turned my attention to that portion of the mixed bases which boiled between 270° and 280° , where I had every reason to expect the presence of picoline. After several rectifications, in each of which the first and last portions of the product were separated, I obtained a fine colourless transparent oil, possessed of all the properties of that substance. It dissolved readily in water: gave, with chloride of gold, a fine yellow compound depositing in needles from the hot solution, and with bichloride of platinum, a salt crystallizing in orange-yellow needles, analogous in all its properties to that of picoline. This identity was confirmed by analysis, which gave the following results:

$\left\{ \begin{array}{l} 5.648 \text{ grains of picoline from bone-oil gave} \\ 15.990 \quad \dots \quad \text{carbonic acid, and} \\ 3.998 \quad \dots \quad \text{water.} \end{array} \right.$					
Carbon,	77.21	77.41	C_{12}	900.0	
Hydrogen,	7.86	7.53	H_7	87.5	
Nitrogen,	14.93	15.06	N	175.0	
	<hr/> 100.00	<hr/> 100.00		<hr/> 1162.5	

For still further security, a determination of the platinum in its double salt with the chloride was made:

$\left\{ \begin{array}{l} 12.784 \text{ grains of chloride of platinum and picoline gave} \\ 4.204 \quad \dots \quad \text{platinum.} \end{array} \right.$	
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This corresponds to 32.88 per cent., and the calculation gives 32.94.

The suspicion, then, of the occurrence of picoline in the odorine of UNVERDORBEN turns out to be perfectly correct; at the same time my experiments have clearly shewn, that odorine is a mixture of picoline, with at least one other base, the properties of which will be detailed in the second part of this investigation. The quantity of picoline contained in bone-oil is considerable, and it can be more readily prepared from that substance than from coal-tar naphtha; in fact, I obtained from three hundred pounds of bone-oil a larger quantity of picoline than that employed in my examination of it, which was obtained from some hundred gallons of coal-tar naphtha; and by means of it, I shall be enabled to trace out the products of its decomposition, which I was unable to pursue in my former communication

The presence of aniline in bone-oil I have already alluded to, and its quantity, though small, is by no means inconsiderable, when compared with that of the other bases. I did not think it necessary to take any further means for its identification than its highly characteristic reactions with chloride of lime and nitric acid.

The investigation of the other bases is not yet in a sufficiently advanced state for publication. The sparingly soluble one has been especially troublesome, and its purification is attended by difficulties which I have not yet fully overcome. The consideration of these will be taken up in the second part of this investigation.

XXXIII.—*On the Action of the Dry Gases on Organic Colouring Matters, and its relation to the Theory of Bleaching.* By GEORGE WILSON, M.D., Lecturer on Chemistry, Edinburgh.

(Read, April 17, 1848.)

I. *Preliminary Remarks.*

The remarkable power which chlorine possesses, of destroying all colours of organic origin, has long been an object alike of speculative and practical interest. The theory of bleaching, however, has hitherto remained imperfect, in consequence chiefly of the observation of Sir H. DAVY, that chlorine loses the power of decolorising when deprived of water. So striking is the difference, in this respect, between wet and dry chlorine, that it led the distinguished chemist mentioned, to deny to this gas the character of being essentially, or *per se*, a bleacher. He regarded as the true decolorising agent the oxygen of the water, which must be associated with chlorine before the latter can bleach. The late Dr TURNER has stated DAVY's view so shortly and clearly, that I quote his abstract in preference to any statement of my own. "DAVY," says he, "proved that chlorine cannot bleach, unless water is present. Thus dry litmus suffers no change in dry chlorine; when water is admitted the colour speedily disappears. It is well known also, that hydrochloric acid is always generated when chlorine bleaches. From these facts, it is inferred that water is decomposed during the process; that its hydrogen unites with the chlorine, and that decomposition of the colouring matter is occasioned by the oxygen which is liberated. The bleaching property of bin oxide of hydrogen, and of chromic and permanganic acids, of which oxygen is certainly the decolorising principle, leaves little doubt of the accuracy of the foregoing explanation."*

The opinions of chemists on this point have now entirely changed. Chlorine is reinstated in the place it formerly held as the possessor of positive and intrinsic bleaching powers, and is looked upon as exerting these even when water is present. I quote Sir ROBERT KANE's views on this subject, as more fully expressed than the statements of most of our chemical authorities, and because he has demonstrated experimentally the falsity of DAVY's conclusion. "The theory," says he, in allusion to bleaching, "of this action of chlorine, which had been formerly thought to depend upon a mere oxidation of the colouring matter, water being decomposed, has been shewn by my results with orceine, and confirmed by those of ERDMAN on indigo, to consist in the formation of new substances con-

* Elements of Chemistry, 7th edit., p. 275.

taining chlorine. The chlorine in some cases replaces hydrogen; in others, it combines directly with the colouring matter; in others, again, water is decomposed, and the product, besides containing chlorine, is also more highly oxidised.*

Beautiful, however, and satisfactory as the investigations of KANE, ERDMAN, DUMAS, LAURENT, and others are, they leave unconsidered the question, why has chlorine, which so rapidly and permanently destroys the colour of organic compounds when moist, no action on them when dry?

The object of the researches I am about to detail, is to supply, in part at least, this defect in the theory of bleaching, by endeavouring to shew how the removal of water from chlorine arrests or suspends its decolorising power. Before entering at length into this question, however, I would observe, that DAVY's conclusion, that oxygen is the efficacious bleaching constituent of moist chlorine, cannot be regarded as marked by the felicity which generally characterised that great chemist's interpretations of obscure or complicated phenomena.

1. His view assumed, against all probability, that the acknowledged great affinity of chlorine for hydrogen would be exerted solely towards the hydrogen of water, but not towards that present in a colouring matter; or at all events, that the affinity in question would be efficacious only in relation to the elements of water. We certainly must set aside, as entirely arbitrary, the notion, if that were implied in DAVY's statement, that chlorine in the presence of two bodies—water and a colouring matter—each containing hydrogen, would be indifferent to that element, as a constituent of the one, but eager to unite with it as present in the other. Although free chlorine, however, must be regarded as equally ready to unite with the hydrogen of every compound which comes within the sphere of its affinity, it does not follow that it will obtain that element with equal ease from every substance containing it. On the other hand, we may be certain, that those more unstable compounds which part most readily with their elements, will be the first to have the hydrogen removed from them by chlorine, whilst less easily decomposed substances may resist its action altogether. DAVY's view, however, gains nothing from this acknowledgment; for it represents water, an enduring compound of but single equivalents of two elements, as compelled to abandon its hydrogen to chlorine, whilst the proverbially fading colouring principle of a flower or an insect—a frail combination of many equivalents of three or more elements—is assumed to be able to retain its hydrogen unaffected by chlorine. In the justice of such a conclusion, no chemist could concur.

2. Again, DAVY's argument proved too much, and was in truth, self-destructive; for if chlorine be denied the character of a positive bleaching agent, because it does not bleach when dry, then oxygen, judged by the same rule, must

* Elements of Chemistry, p. 1054.

be refused that character also, for when the latter gas is deprived of moisture, it is more indifferent to colouring matter than even chlorine. DAVY'S reasoning, then, if pushed to its logical consequences, conducts us to the strange conclusion that since, when moist chlorine bleaches, the chlorine and oxygen are not the active agents, and the only other body present except the colouring matter is hydrogen, which certainly does not decolorise when dry, bleaching must be regarded as an inexplicable phenomenon, an effect without a cause.

It would not be difficult to point out other objections to the consistency of DAVY'S opinion; but those given may suffice to prove that, before the analysis of the products of chlorine-bleaching shewed the erroneousness of his conclusion, its inherent untenableness admitted of easy illustration.

II. *Influence of Sunlight on the Bleaching Action of Dry Chlorine.*

The chief object of the experiments which this paper details was, to ascertain the cause of the indifference of chlorine when free from moisture to anhydrous organic colouring matters. But before entering on this inquiry, it occurred to me to doubt, whether DAVY'S original proposition, that dry chlorine does not bleach dry colours (which seems to have been universally assented to by his successors), could be admitted without limitation.

A repetition of his experiments appears, at first sight, to justify unconditionally his conclusion. Among the specimens which accompany this communication are two sealed tubes, containing blue and red litmus-paper shut up in an atmosphere of chlorine. The paper was first dried in a current of air previously passed through chloride of calcium. A stream of carefully desiccated chlorine was then sent over the paper for five minutes, and the tube, whilst full of gas, sealed at the blow-pipe. The coloured papers were thus exposed, in the first place, to the bleaching action of some sixty cubic inches of chlorine; and have, in addition, remained in contact with that gas since the 28th of July 1847, a period of more than eight months, yet they still retain their original tints, though somewhat faded. Had water been present in these experiments, the colours would have been irrecoverably destroyed in a few seconds, or minutes at the farthest.

Striking as these results are in supplying confirmation of DAVY'S views, they are curiously contradicted, or rather qualified, by other experiments, differing from those just mentioned, as to mode of trial, only in one particular.

The affinity of chlorine for hydrogen, when both gases are free, is greatly modified by the action of sunlight, so that whilst in perfect darkness they may be kept mingled without combining, they unite with explosion if exposed to the direct rays of the sun, and more or less rapidly in diffuse daylight, according to its intensity. So faithfully, indeed, do free chlorine and hydrogen obey what natural

philosophers and chemists have agreed to call the actinic influence of the sun-beam, that the mixed gases contained in a graduated tube over water are found to form a delicate actinometer, the intensity of the actinism being measured by the rapidity with which the water rises in the tube, as it dissolves the hydrochloric acid produced by the union of the gases.* This actinic exaltation of affinity, so striking when both gases are free, continues to manifest itself, though less powerfully, when chlorine is in contact with substances containing hydrogen, although the latter is in a state of combination.

Chlorine water remains unchanged in the dark, but is rapidly converted by sunlight into hydrochloric acid, and free oxygen. Dutch liquid, chloroform, and chloric ether, besides various other bodies, are known to give up their hydrogen to chlorine much more swiftly when exposed to the direct rays of the sun than if shaded from them. It seemed in the highest degree probable that the hydrogen of organic colouring matters would, in like manner, resist the action of dry chlorine for a much shorter period in sunlight than in diffuse daylight, or in darkness. To determine this point, the following experiment was tried. A wide glass-tube, open at both ends, was constricted in the middle so as to present a narrow central canal, like that of an hour-glass. Pieces of blue and of red litmus-paper were then placed on either side of the constricted portion, and the open ends of the tube drawn out at the blow-pipe, so as to admit of their being put in communication, by means of caoutchouc connectors, with an arrangement for drying the paper, and furnishing chlorine. After the paper had been exposed to a current of dried air at the temperature of 220° Fahr. for three hours, washed chlorine, transmitted through Nordhausen sulphuric acid, and a tube three feet long containing fused chloride of calcium, was sent along the double tube containing the papers, for five minutes. The ends of the tube were then sealed whilst it remained full of gas, and the constricted middle portion closed and divided at the blow-pipe, so that the double tube was converted into two single hermetically sealed ones, each containing dried litmus-paper in an atmosphere of chlorine. In this way two tubes were procured, each containing portions of the same coloured paper, which had been dried in the same current of air, and exposed in exactly similar circumstances to the same stream and the same amount of dry chlorine.

The one of these twin tubes was hung up inside a window, with a western exposure, on or about the 31st of July 1847. The other was laid aside in a cupboard, out of reach of the direct rays of the sun, but not protected from the influence of dull daylight. It was frequently brought out, moreover, to be examined, and was at no time during the day in absolute darkness. I shew the Society this tube after remaining in the circumstances described for more than eight

* Lond. and Edin. Phil. Mag., 1844, vol. xxv., pp. 2-3.

months. The inclosed papers still retain their original colours little altered; and in perfect darkness would, in all probability, have retained them still better. Side by side with this tube I have placed its twin, which was exposed to full sunshine, and the papers in which are bleached to the purest white. In how short a time his change occurred I cannot precisely say, as absence from town between the 1st of August and the 16th of September 1847 prevented me from watching the progress of the actinic bleaching. But, on the last-mentioned date, I found the paper completely decolorised, so that six weeks of sunshine sufficed totally to bleach paper in dry chlorine, whilst that gas excluded from direct sunlight has failed to produce the same effect in eight months and a half.* In another quite similar experiment, the results were much less striking. A tube with dry chlorine and litmus-paper has hung since 1st August 1847 in a western exposure, yet, at the date of my writing, (April 13, 1848), the litmus-paper, though much faded, as appears when it is contrasted with the contents of the twin tube which was kept out of sunshine, is far from being entirely bleached. This difference in result leads to the suspicion, that in the experiment first recorded, the chlorine or the paper may not have been so dry as both were in the second trial. Great precautions (the same in both cases) were taken to secure absence of moisture from the gas and the paper, but I know of no test of perfect dryness applicable to gases, and I cannot affirm that, in either case, the chlorine or the colouring matter was absolutely anhydrous. Nor does it admit of doubt that the presence of even a trace of water would sensibly quicken bleaching under sunlight, which rapidly decomposes chlorine-water. Yet every chemist will acknowledge that chlorine, which could be retained over litmus without bleaching it for nearly nine months, must have made a close approximation to perfect dryness. We are as yet, moreover, too ignorant of the laws and conditions of actinic action, to know well how to dispose of apparent discrepancies in its effects.

I could not try more than the two experiments recorded, last summer, and I did not think it desirable to attempt a repetition of them during the clouded season of the year. Meanwhile, different as is the testimony these experiments afford, as to the rapidity of actinic chlorine-bleaching, they agree in proving that darkness, as well as dryness, is essential to the preservation of organic colours from destruction by chlorine, and that this gas, at least when assisted by sunlight, is a positive bleacher. DAVY'S original proposition must be accepted with this qualification.

I close my remarks on this subject, with the observation, that in bleaching on the large scale it should make a sensible difference on the rapidity of the process, whether it be carried on in open sunlight, or in exclusion from it. Our present bleaching process is as rapid as could well be wished, so that it is not in the direc-

* The papers shut up with chlorine, and kept in darkness, have not become bleached by two months' longer retention in the gas June 19, 1848

tion of quickening his methods, that the practical bleacher probably desires improvement. One may expect, however, that the same amount of chlorine, especially if moist, should be more efficacious in bleaching, if assisted by sunlight, than if debarred from it; or what comes to the same thing, that a small amount of chlorine should, in practice on the great scale, bleach as powerfully in sunshine, as a larger one in darkness. It might be possible, accordingly, to economise chlorine, or chloride of lime, in this country, in the brighter seasons of the year, and at all times in sunny climates, if the bleaching operations were carried on in the open air.

III. *Influence of Water over the Bleaching Action of Oxygen, Sulphurous Acid, and Sulphuretted Hydrogen.*

The fact that actinised chlorine bleaches, though dry, supplies no explanation of the function which water performs, when it invests that gas with decolorising power. With a view to solve this problem, I made two series of experiments; 1st, The object of the one was to observe to what extent other bleaching gases resemble chlorine in being dependent for bleaching power on the presence of water, and likewise to ascertain whether the acid gases and the volatile alkali, when made anhydrous, lose that power of changing the tints of dry organic colouring matters, which characterises them when moist. These experiments promised to shew whether the action of dry chlorine on colours is exceptional and anomalous, so as to demand a special explanation, such as DAVY gave, or but a particular case of a general law, to which all elastic fluids are obedient.

2d, The object of the other set of trials was to determine, whether bleaching power can be conferred upon dry chlorine, by dissolving it and anhydrous colouring matters in liquids containing no oxygen. I begin with the experiments first referred to.

Five gases besides chlorine have marked bleaching powers when in the condition of perfect elastic fluidity, and not anhydrous, viz., chlorous acid, hypochlorous acid, sulphurous acid, sulphuretted hydrogen, and oxygen.* To these may be added provisionally, the curious body *ozone*, which BERZELIUS regards as an allotropic form of oxygen, and SCHOENBEIN as a volatile peroxide of hydrogen. I have made no experiments with this substance, because, in the present state of our knowledge concerning it, it could not supply crucial results. Chlorous acid is too explosive to admit of satisfactory researches being made with it. The same remark applies with limitation to hypochlorous acid, a substance so interesting, from its high bleaching power, and its containing, like chlorous acid, the two most important bleaching agents, chlorine and oxygen. I have made no experiments with this substance, but PELOUZE has quite recently supplied us with a new and much

* I omit from this list hydrogen, because, although it bleaches powerfully in the nascent state, it has no sensible bleaching action, whether moist or dry, after it has attained the condition of perfect gaseity.

more manageable process for preparing it, by means of which we may hope to make researches as to its action on colours.* My experiments have been limited to sulphurous acid, sulphuretted hydrogen, and oxygen. I begin with the last as the chief rival in bleaching power of chlorine.

Oxygen.

I have not thought it necessary to make many experiments with oxygen, as to its relative bleaching power when moist and dry. Test papers can be preserved for years, without sensibly changing tint in air, *i. e.*, diluted oxygen, only moderately dry, especially if free exposure to light be avoided. The general experience of mankind has led to the same conclusion, in reference to the comparative permanence of tint, of dyed tissues kept in the shade. I have exposed coloured papers for four and five hours to a current of dry air, without permanently altering their hue. The paper in such trials always exhibits a duller tint at the end than at the beginning of the experiment; but that this is the result merely of its loss of water, is evident from the fact that, on moistening the paper, the original brightness of tint is restored. No one, probably, will dispute the conclusion, that dry oxygen does not, at least in darkness, bleach more than dry chlorine.

The effect, on the other hand, of the addition of water to oxygen in increasing its decolorising power, is so strikingly demonstrated by the practical experience of the domestic bleacher, that experiments on the small scale did not seem necessary to prove the fact. No point is more attended to, in the familiar practice of bleaching cloth by free exposure to rain, wind, and sun, than the constant keeping of the tissue wet. I am far from affirming that other important agencies, such as the actinic, concerned in the bleaching, are not affected by the presence of water; yet I think no one will doubt, that one important function it serves, is the increasing (I do not at present say how) the bleaching action of the oxygen of the atmosphere.

The unquestionable decolorising power of peroxide of hydrogen, chromic, and permanganic acids, to which Dr TURNER refers as confirming DAVY'S view regarding the bleaching action of moist chlorine, only demonstrates that *nascent* oxygen bleaches, and is of no service in proving that that gas, when in its state of perfect elastic fluidity, possesses bleaching powers. The nascent hydrogen of decomposing water bleaches readily; so that, if Dr TURNER'S view were accepted as valid in relation to oxygen, a theory of chlorine-bleaching might, with some plausibility, be defended, in which hydrogen, instead of, or as well as, oxygen, should be represented as the positive bleaching agent in chlorine-water.

* It would be peculiarly interesting to observe the effect of drying this gas in modifying its action on colouring matters. Should it lose its bleaching power when dry, it would be curious to watch the effect of exploding it in the presence of an anhydrous colouring matter. The result would shew whether *nascent* oxygen and chlorine bleach as powerfully when *dry* as moist.

Sulphurous Acid.

The bleaching power of moist sulphurous acid is so well known, and has so long been turned to account in the arts, that I need enter into no details in proof of the gas possessing this property. With a view to determine whether it loses its bleaching power when made anhydrous, I passed washed sulphurous acid, obtained by the action of mercury on oil of vitriol, through strong sulphuric acid, and over chloride of calcium, so as to deprive it of moisture. The gas was then made to stream for five minutes through a tube containing blue litmus-paper, carefully dried, and the tube hermetically sealed while full of the gas. This experiment was made on March 31, 1848. The paper was not altered in tint during the passage of the gas, and at the present date (April 17) it remains unchanged.* I made a similar experiment on the 10th of July 1847, only the sulphurous acid, being obtained by the action of charcoal on oil of vitriol, was mingled with carbonic acid. The mixed gases were passed dry over blue litmus-paper for seven minutes, but did not change its tint in the slightest; the tube was then sealed, and is included among the specimens laid before the Society. After the lapse of nearly nine months, the paper continues not appreciably altered. Gaseous sulphurous acid, then, is no better bleacher when dry than chlorine.

Sulphuretted Hydrogen.

The other remarkable properties of sulphuretted hydrogen have prevented its bleaching power from attracting very much attention; nevertheless, it has long been recognised. Nascent sulphuretted hydrogen bleaches powerfully. An acidulated infusion of litmus has its colour rapidly destroyed by the addition to it of a metallic sulphuret, such as that of calcium, barium, or iron. The free gas bleaches much less distinctly, yet its action is tolerably rapid. In proof of this, I have sent with this communication a tube, which, after the blue litmus-paper contained in it had been dried, was opened for a few seconds to the atmosphere, in consequence of a derangement of the apparatus. The paper was thus exposed for a very short period to the amount of vapour which is diffused through air at the temperature of about 60° F. Immediately after this accident, carefully-dried sulphuretted hydrogen was passed over the litmus for five minutes, and the tube sealed. The paper was distinctly, though faintly, reddened during the passage of the gas, and after the lapse of about twenty hours the colour was found almost completely gone. In contrast with this result obtained with slightly moist sulphuretted hydrogen, I shew the Society a tube containing dry blue litmus-paper, and brown-red rhubarb paper, which were exposed to the action of equally dry sulphuretted hydrogen for six minutes. The papers were not altered in tint.

* The colours are still unaltered; a remark which applies also to the experiment next recorded, June 19, 1848.

They have remained sealed up in the tube, in an atmosphere of the gas in question, since July 9, 1847, when the experiment was made. No decided change was observed in the tints of the papers up to July 30, when I ceased to make notes of their appearance. The rhubarb paper is now little altered, but the blue has, here and there, a few small red spots, some with white borders upon them. Both tints, however, are still, after so many months' exposure to the gas, very slightly affected.* I shew the Society also tubes containing blue paper, which were exposed for five minutes to dry sulphuretted hydrogen on the 25th March 1848, and have since remained shut up in the gas. They exhibit, at the present date (April 17), no sensible change in tint.† Sulphuretted hydrogen, then, has its bleaching power arrested by depriving it of water.

IV. *Action of the Acid Gases and of Ammonia on Organic Colours.*

I pass now to the acid gases and ammonia, which so characteristically alter the tints of organic colouring matters, when water is present. The gases I tried were, sulphurous acid, carbonic acid, sulphuretted hydrogen, and hydrochloric acid, in addition to the volatile alkali.

Sulphurous Acid.

I need not say anything further concerning sulphurous acid, as it is implied in what was stated as to its negative action on vegetable blues, that its reddening action is as much arrested as its bleaching one by depriving it of water. Dry sulphurous acid, I also find, does not change alkalised turmeric or rhubarb paper to yellow.

Carbonic Acid.

I have already referred to the retention of its full blue tint, by dry litmus-paper, exposed for many months to a mixture of anhydrous carbonic and sul-

* The papers have not sensibly altered after the lapse of two additional months; nor is there any change in the litmus-paper referred to in the next experiment, June 19, 1848.

† One of the arguments in favour of the "Binary Theory of Salts" is the fact, that the so-called oxygen acids do not affect vegetable colours unless associated with water, which they are assumed to decompose, so as to become by appropriation of its elements hydracids of new radicals. The experiments recorded in the text, however, shew that one hydracid, at least, has its action on colouring matter as much negated by the withdrawal of water as any oxyacid. It does not follow that the rationale of the change is the same in both cases; but the fact that anhydrous gaseous hydrosulphuric acid does not redden vegetable blues, lessens the value of the argument alluded to. In the great majority of cases, the rendering of an oxyacid anhydrous implies its alteration from the liquid state to the solid or gaseous one. This change in condition is of itself sufficient to alter most materially the influence of a reagent. It appears, however, to have been altogether overlooked in explaining the indifference of a dry oxyacid to organic colours. The phenomena recorded further on, as observed with gaseous hydrochloric acid and liquefied sulphurous acid, bear upon this point, but it cannot be discussed at length here.

phurous acid gases. Pure carbonic acid is equally negative in its action on vegetable blues and browns.

Sulphuretted Hydrogen.

Sulphuretted hydrogen, even when moist, does not change organic colours to so great an extent as the stronger acids do. Solutions of litmus, *ex. gr.*, become, under its action, only of a purple-red tint, like that which carbonic and boracic acids give them, whilst the more powerful acids destroy all shade of blue. If I may judge from the few experiments I have made on this subject, the reddening power of sulphuretted hydrogen is more dependent than its bleaching action on the presence of water. At all events, it is equally dependent on moisture, for blue litmus has been reddened very slightly by eight months' exposure to the dry gas, neither has brown rhubarb paper become yellow, or appreciably grown paler.

Hydrochloric Acid.

No acid excels hydrochloric in full and rapid action on organic colours; nor is any one, according to the prevailing opinions of chemists, less likely to be indebted to association with water for its characteristic properties. It is the simplest type of a perfect acid, and as such, might be expected to exhibit, even when gaseous and anhydrous, the same relation to organic colours which it does when moist. I looked upon hydrochloric acid, therefore, as the most interesting of the acid gases with which experiments could be made.

I have not hitherto referred particularly to the method followed for drying the gases, because none of those I have yet mentioned present great difficulties in the way of rendering them,—I will not say certainly anhydrous,—but at least sufficiently dry not to affect colours. It is otherwise with hydrochloric acid. I have failed more frequently than I have succeeded, in rendering this gas, by drying, indifferent to colours; nor have I been able to preserve blue litmus for any length of time unchanged in an atmosphere of the dry gas. It is necessary, therefore, to be more particular in describing the process for drying, which was followed with hydrochloric acid; although it differed in no respect from that pursued with the majority of the other gases.

The general arrangement, especially in the later and more perfect trials, was the following:—The thinnest India letter-paper was stained with an infusion or tincture of the colouring matter intended to be used, and afterwards dried at the temperature of the air. Slips of the paper were introduced into a tube, varying in different cases from half an inch to one inch in diameter, and from six to eighteen inches in length. The tube was then hermetically sealed at one extremity, and drawn out at the other into a narrow canal, which was left open. A

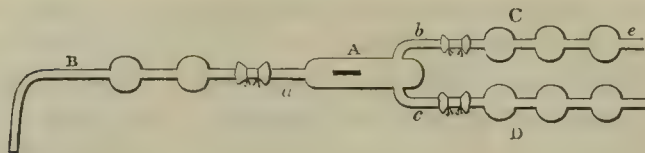
smaller open tube was afterwards attached to each side of the larger one, near to its shut end, so as to communicate with its cavity. These lateral tubes projected for a short distance at right angles to the long axis of the larger tube, and then ran parallel to it, with their open mouths pointing in the opposite direction from that of the single canal at the other end of the tube. To prevent confusion, I shall call the lateral appendages which I have described, the horns of the tube.

When the latter was arranged for an experiment, the narrowed termination at one end was placed in communication with a short tube filled with chloride of calcium, by means of a caoutchouc connector. The free extremity of this tube was bent at right angles, and dipped into oil, so as to cut off communication with the outer air. To each of the open horns, also, a chloride of calcium tube, three feet in length, was attached. The one of these tubes was intended to convey, and render anhydrous, a current of air, which should dry the paper. The other, in like manner, was to carry and dry the gas, which should be brought in contact with the paper, when the latter was deprived of moisture. At the beginning of an experiment, the tube through which the gas subsequently passed, after being connected with one of the horns, was sealed at the end furthest from the paper. Its presence from the first was essential, because otherwise the dried paper must have been put in communication with the moist outer air, when a fresh chloride of calcium tube to carry the gas was substituted for that which previously conveyed the air: for if the same tube in whole, or in part, had been employed, first to dry the air, and then the gas, the freedom of the latter from moisture could not have been counted upon.

I found, after trying various devices, a pair of common bellows the most convenient instrument for furnishing a current of air. The air was first passed through a bulbed tube, immersed in a freezing mixture, and then through the chloride of calcium tube into the one containing the paper; from which it escaped through the smaller drying tube that dipped into oil, as already mentioned. The paper was maintained by gas lights at a temperature of about 220° Fahr., and the air was kept passing over it, for at least two, generally for three, hours.

When the paper appeared perfectly free from moisture, the lateral horn, by which the air reached it, was sealed at the blow-pipe, and the drying apparatus detached. The shut end of the other long chloride of calcium tube was then opened, and connected with an apparatus for furnishing the gas to be used in the experiment.

The woodcut on the following page will make the description more intelligible. Only the more essential parts of the arrangement are represented in the diagram; the bellows and connecting flexible tube on the one hand, and the retort in which the gas was generated on the other, as well as the washing bottles, &c., being omitted.



A, Tube in which coloured paper was placed.

a, Narrow canal, communicating by a caoutchouc collar, with small tube containing chloride of calcium.

b and *c*, Lateral appendages or horns, communicating with long drying tubes, C and D.

B, Small drying tube, dipping into oil, through which the elastic fluids, after passing over the paper, escaped into the air.

C, Long drying tube, which was sealed at *e*, at the commencement of an experiment, but opened when the paper had been dried, and employed to convey the gas which was to act on it.

D, Second long drying tube employed to carry air, to render the paper in A anhydrous. When the paper was dried, the horn *c* was sealed at the blow-pipe, and D detached. Gas was then transmitted through C.

The arrangement I have described was followed with all the gases except ammonia. Chlorine and sulphurous acid were sent through oil of vitriol before reaching the chloride of calcium tube. The other gases were generally passed simply through a bulb immersed in a freezing mixture, before being transmitted through the drying tube. I found it essential to success in the experiments recorded to dispense with corks, asbestos, and cotton, in connecting or loosely stopping the tubes, as these bodies retained moisture with the greatest obstinacy. Caoutchouc collars were used in every case to unite the detached portions of the apparatus. The special devices followed in particular cases are mentioned under the gases which called for them.

I procured hydrochloric acid gas by the action of Nordhausen sulphuric acid on common salt previously fused. Theoretically, the gas should carry with it a mere trace of moisture, yet in spite of the apparently effectual drying apparatus made use of, the acid, in the majority of trials, changed the tint of blue litmus as soon as it came in contact with it, giving it a dark lilac or deep wine-red colour. Nevertheless, on three occasions I was able to pass a current of dried hydrochloric acid for five minutes over blue litmus-paper, without sensibly altering its tint, according to the judgment of four persons besides myself, who were witnesses of the experiments. These positive results outweigh the negative ones already referred to. The majority of the latter, moreover, were only partial failures, and went the length, at least, of proving that the removal of moisture from hydrochloric acid gas delays, if it does not prevent, its characteristic action on organic colours. In the successful experiments mentioned above, the negative action of the acid was only transient; for when the tubes containing the gas and paper were sealed and set aside, the colour invariably passed from its original blue tint to a more or less decided red. In several trials, however, the ultimate effect of the hydrochloric acid fell far short of the full reddening which

even the slightly moist gas produces. I shew the Society a tube, containing litmus-paper, which withstood, without change, a current of hydrochloric acid passed over it for five minutes. The experiment was made on 23d February 1848; and the paper is at this date not a bright red, but only a dark lilac.

Three explanations suggest themselves as to the ultimate though imperfect reddening of the blue litmus. 1. The gas may not have been quite dry. 2. Hydrogen acids, though perfectly dry, may, unlike the so-called oxygen acids, be able to modify the tints of colours. 3. Hydrochloric acid, which has a great affinity for water, can compel its elements to unite to form it, so that the gas may combine with the liquid. Hydrogen and oxygen are present, both in the colouring matter and in the paper. In the paper, indeed, they are present in the proportion to form water. This solvent, therefore, may be slowly generated within the sealed tube, and be the cause of the gradual reddening of the blue paper.

As to the first of these views, I can neither disprove nor confirm it. It is probably as difficult to render a gas absolutely anhydrous, as it is to produce a perfect vacuum. Moreover, as I have stated already, we have no test of absolute dryness, applicable to a gas. It would interrupt the argument, however, to consider this question at length here; I have devoted, accordingly, the section which succeeds this to its discussion.

Of the second explanation, I would say much the same as I have said of the first. It is highly probable that a powerful hydracid like the hydrochloric should retain, more or less, as a gas, its characteristic action on organic colours.

I would speak most positively of the third view. The power of bodies which have a great affinity for water, to compel its formation and separation, is so great, that I ventured to predict, that it would be impossible to preserve, for any length of time, blue litmus unchanged in tint in the driest hydrochloric acid. I do not dwell at length, however, on the cause of the ultimate reddening, as the fact that the paper was only slowly and imperfectly reddened, is sufficient for my present purpose. At lowest, the experiments I have detailed demonstrate that the removal of water from hydrochloric acid gas delays its action on colours.

Ammonia.

The last of the gases I tried was ammonia. According to KANE,* when dry it has no action on organic colours, although no body, when moist, affects these substances more powerfully. I might content myself with adducing this distinguished chemist's statement as to the negative action of dry ammonia, and add it to the list of gases which have their action on colouring matter arrested by the removal of moisture. My own experiments, however, have been so much less decisive than KANE's statement led me to expect they would be, that I cannot, without comment, avail myself of his evidence. The difference between his results and

* Elements of Chemistry, p. 352.

mine is probably sufficiently accounted for by the supposition, that I did not thoroughly dry the ammonia. This gas is more difficult to render anhydrous than even hydrochloric acid; not so much, perhaps, because it has a greater affinity for water, but because our most powerful desiccating agents, such as the deliquescent chlorides and oil of vitriol, cannot be employed to dry it. We are restricted, accordingly, to substances much less hygrometric, such as unslaked lime, hydrate of potass, and its fused carbonate.

It would serve no purpose to record a series of unsuccessful experiments: I merely mention, therefore, that I have never been able to obtain ammonia in a condition in which it did not change the tint of reddened litmus and of yellow turmeric paper as soon as it came in contact with them. I have found its action on colouring matter, however, sensibly reduced by passing it over the hygrometrics last referred to. Reddened litmus, for example, became only purple when it first encountered dried ammonia, and did not acquire a bright-blue tint, when left in the gas, till after the lapse of some hours.* For the reasons mentioned above, I do not, in the meanwhile, feel myself at liberty to say more than that the presence of water greatly quickens the action of ammonia on colours.

It would appear, then, from the results I have detailed, that there is little, if anything, anomalous or exceptional in the negative bleaching of dry chlorine. Oxygen, sulphurous acid, and sulphuretted hydrogen, are equally powerless as bleachers, when deprived of moisture, as that gas. Sulphurous and carbonic acids are probably more indebted than chlorine to water for their power of modifying colouring matters, both as regards changing and destroying their tints. Hydrochloric acid and ammonia have their influence on colours at least temporarily arrested by the absence of water; and, after all, it is a question with the whole of the gases referred to, only of degree. It is not likely that even in absolute darkness chlorine has no action on anhydrous colouring matter. If this be conceded, the whole of the gases referred to may be included in one category, as having their modifying action on organic colours accelerated by the presence and retarded by the absence of water. I trust to supply an additional datum towards the settlement of this question, by observing the difference which exposure to sunlight makes, in relation to the action of all the gases with which experiments were tried.

* In the experiment which yielded the most successful result, the ammonia was first passed through a bulb immersed in a freezing mixture, and afterwards through long tubes containing lime, caustic potass, and its fused carbonate. The gas was then allowed to flow through a tube for some minutes till it had expelled the air, and the tube was sealed. The one end of this tube had been previously expanded into a large ball, which was filled with fragments of the hygrometrics just mentioned: in the other end of the tube a small sealed bulb was placed, containing a piece of carefully dried red litmus-paper. The ammonia was left in contact with the drying agents for a week, when the tube was shaken till the bulb broke, and allowed the gas and the paper to meet. The latter, as mentioned in the text, immediately became purple, and after some hours bright blue.

Before proceeding to detail the second series of researches, I have thought it desirable to offer some observations on the processes employed for drying gases.

V. *On the Methods applicable to the Drying of Gases.*

The methods at present in use for drying gases cannot be considered as yielding more than an approximation to absolute dryness in the case of any elastic fluid. The processes employed are inherently defective, both mechanically and chemically. When chloride of calcium and pumice-stone, steeped in oil of vitriol, are employed as the desiccating agents, they cannot be made use of except in fragments of considerable size, otherwise the containing tubes become choked, and the gas does not pass. Interstices, accordingly, comparatively speaking large, occur between the separate fragments of the drying agent; and the gas, in moving along, has a certain portion of its mass not in physical contact with the hygrometrics, or directly exposed to their desiccating action. In like manner, when a gas is sent through a column of oil of vitriol, only the surface of each bubble is in contact with the liquid, and the gas-bells rise very rapidly through so dense a fluid, so that they can be dried only imperfectly during their ascent.

Those defects admit only of partial remedy, by extending the surface of chloride of calcium or pumice-stone, or by multiplying the columns of oil of vitriol through which the gas shall pass. A practical limit is set to such devices by the obstruction which they offer to the passage of elastic fluids. This can be overcome only by increasing the pressure at which the gas is delivered, and it is not easy to regulate this, so that the gas shall not flow in too swift a current, and so neutralise, by the rapidity of its passage, the benefit which would otherwise result from its coming in contact with an extended hygrometric surface.

The imperfections just alluded to are not, perhaps, beyond the reach of suitable mechanical contrivances; but even if they were all remedied, the important question still remains, will the protracted and complete contact of a gas containing water-vapour with the most powerful hygrometrics, suffice entirely to deprive the gas of moisture? With a view to determine this point, I shut up muriatic acid gas, previously passed through a freezing mixture, and over chloride calcium, within a glass tube containing fragments of the same salt. It was left for a week in contact with the chloride, and then allowed to meet carefully dried blue litmus-paper, enclosed along with it at the commencement of the experiment, in a small sealed bulb of thin glass, which was readily broken by shaking the tube. The paper began to change tint as soon as it met the gas; and if this alteration in colour be accepted as an evidence of moisture being present in the muriatic acid, then the latter was not dry. A similar experiment, with a like result, has already been related in reference to ammonia.

These results, however, are not decisive of the point whether the gas was anhydrous or not, for the change in tint of the litmus may possibly be accounted

for otherwise than by assuming the presence of water; and the fault, moreover, even if water were present, may have lain with the coloured paper, not with the gas. The same objection does not apply to the following observation. If air at the temperature of 60° F. be sent through a long tube filled with fused chloride of calcium, it parts with moisture, which the chloride absorbs and combines with. If this dried air be thereafter transmitted over moist chloride of calcium, the latter becomes, to appearance, speedily dry. Here we have the apparently contradictory results of chloride of calcium drying air, and air drying chloride of calcium. The inference seems unavoidable, that there must be a neutral point where the chloride of calcium and air will be mutually indifferent, so that neither shall be able to deprive the other of moisture. This point will vary in reference to, 1. the relative quantities of the hygrometric salt and air acting on each other; 2. the relative dryness of the gas and solid; and, 3. the temperature at which the trial is conducted. Experiments on gases are generally made in apartments having an average temperature of or about 60° F., at which the tension of water-vapour is probably great enough to resist, so far as complete condensation is concerned, the absorbing power, and affinity for it, of all hygrometrics. This remark leads directly to the observation, that reduction of temperature is probably the most effectual of all processes for drying a gas. It has been employed with great success by FARADAY, in his later researches on the liquefaction of the gases;* and I was induced, in consequence, to make use of it in my experiments. The value of the method admits of easy demonstration. The great obstacle to rendering a gas anhydrous, is the tension which heat confers on the water-vapour diffused through it. We generally endeavour to overcome this tension by opposing to it the condensing force of porous hygrometrics, and the chemical affinity of substances which combine readily with water; yet it is not at all certain that these forces have the maximum condensing power attributed to them. On the other hand, it is certain, that the tension of water-vapour is exceedingly small at zero, and rapidly decreases as we descend the thermometric scale.

FARADAY'S discovery, moreover, of the existence of a limit to vaporisation, teaches that there must be a temperature at which ice abruptly ceases to give off vapour. If this point be within reach of our frigorific appliances, and were ascertained, we should possess, in the reduction of gases to this temperature of no vapour, a theoretically perfect process for rendering gases anhydrous. It would be applicable, however, only to the less condensible elastic fluids, for the more easily liquefied ones would become liquid before the temperature of no-water vapour had been attained. It is further to be noticed, and the remark is important, that all volatile bodies have their vaporising point lowered in the presence of bodies more volatile than they are. The fact is familiar to every chemist. The

* Phil. Trans., 1845. Part I., p. 155.

essential oils, *ex. gr.*, whose boiling points are much above that of water, are entirely dissipated in vapour if mingled with water raised only to the temperature of 212° F. Even bodies ranked, when anhydrous, among fixed substances, such as common salt, nitre, and boracic acid, rise with the vapour of water below its boiling point. It cannot be doubted that, in like manner, a temperature sufficiently low to hinder ice from volatilising *in vacuo*, or in still air, would not prevent it yielding a continuous stream of vapour in a current of gas. This power, indeed, of gases, as the more volatile bodies, to solicit and compel water-vapour to accompany them, is, at all temperatures, but especially at high ones, a formidable obstacle to rendering elastic fluids anhydrous. On the other hand, this diffusive power greatly increases the desiccating effect of gases, even not absolutely dry, when sent in currents over moist solids.

How near an approximation may be made to perfect dryness in the case of gases, cannot be determined till we have a test of the anhydrous state applicable to elastic fluids. A criterion of some value would be the passage of a considerable volume of the gas (ammonia excepted), through a weighed tube containing chloride of calcium, which should not increase in weight if the gas were anhydrous; but, if the preceding observations are well founded, this test would cease to act before the gas was quite dry.

I have tried whether the change of tint which the so-called sympathetic inks (solutions of the salts of cobalt and nickel) undergo when deprived of water, would serve as an indication of dryness on the part of gases; but I find that it is a test of no delicacy.

Indifference to colouring matter will certainly be found a negative indication of some value. Chlorine, *ex. gr.*, which immediately bleaches, and sulphurous or carbonic acid, which reddens litmus, cannot be dry. This test, of necessity, is limited to the gases which affect organic colours, and would be useless in the case of oxygen, nitrogen, hydrogen, the carburetted hydrogens, &c. &c.

A convenient way of examining the dryness of gases by means of colouring matters is to prepare, by blowing at short distances along a thin glass-tube, a series of small bulbs, in each of which a piece of litmus-paper may be placed. The papers are then to be dried in a current of air, passed through oil of vitriol and over chloride of calcium, and each bulb sealed off separately. In this way, a large number of bulbs can be prepared at the same time, and kept ready for use when required. One of these is to be placed in a tube forming part of the arrangement employed in the particular experiment, so that it shall be enveloped in the gas whose dryness is to be tested. By a sharp tap on the tube, the enclosed bulb is easily broken, and the gas and paper allowed to meet. There is no difficulty in making the bulbs thick enough to bear handling, and yet sufficiently thin to give way when required.

In the preceding remarks, I have chiefly referred to the difficulty experienced

in drying gases. But organic solids, such as litmus and colouring matter, though probably more readily dried than gases, are certainly with difficulty rendered anhydrous. The difficulty is too familiar to every chemist who has made organic analyses, to call for any illustration or proof. Reference has already been made to the desiccating power of currents of air, and to the likelihood of a solid being more effectually dried by a gas than a gas by a solid. If, however, it is impossible to supply a current of perfectly dry air, it may be doubted whether it is possible to render a solid anhydrous by passing air over it. It is difficult to imagine that air, containing *ex hypothesi* some moisture, should make a solid absolutely dry, drier than the air itself is; yet it is not impossible that it should. The tension which heat gives to water-vapour; its great dilatation when present in small quantity; and its diffusion through a large volume of gas, may more than balance any power on the part of the solid to attract or condense it. There may be a limit to condensation, as well as to vaporisation.

Notwithstanding all that has been urged in this section, in reference to the difficulty of rendering gases *absolutely* dry, it will not, I think, be questioned, that in the experiments I have recorded, a close approximation to actual dryness was attained in many of the trials. And, conceding that traces of moisture may have been present, I may, nevertheless, with some justice, argue, that if the removal of a certain amount of water from gases arrests for months their action on colours, *a fortiori*, the total abstraction of moisture would still more decisively negative that action.

VI. *On the Action on Dry Organic Colouring Matters of the liquefied Anhydrous Gases, and of Chlorine dissolved in liquids containing no Oxygen.*

The second series of experiments, as I have already mentioned, was made with the view of ascertaining in what way water acts, when it accelerates the action on colouring matter of the gases referred to, but particularly of chlorine. According to the prevailing theories of chemists, when water meets dry carbonic and sulphurous acid, or dry ammonia, it does not merely dissolve them, but allows its elements to be appropriated by each of these gases, which become, in consequence, compounds possessed of new relations to bases, acids, colouring matters, and the like. I shall therefore set these gases aside, as not admitting of direct comparison with chlorine, which chiefly concerns us. There is no reason, on the other hand, for supposing that water does more than merely dissolve oxygen, sulphuretted hydrogen, and hydrochloric acid, so that that liquid may be supposed to change their relation to colours in the same way as it does that of chlorine.

The older chemists held by the axiom, "*Corpora non agunt nisi soluta,*" and by means of it could fully have accounted for the difference in action on colours

of the dry and moist gases. In our own day, however, the problem takes a somewhat different shape, for we have learned to liquefy the gases without the intervention of a solvent. Three of the four gases last referred to, which simply dissolve in water, viz., chlorine, sulphurous acid, and sulphuretted hydrogen, admit of liquefaction, although quite anhydrous. It has been held, accordingly, that the liquefaction of a gas changes its properties, in the same way as dissolving it in water would.

With a view, so far at least, to examine this point, I exposed carefully dried blue litmus-paper to the action of liquid bromine (which is equivalent to a liquefied gas), repeatedly rectified from chloride of calcium, and supposed to be anhydrous. Ultimately the paper was quite bleached; but the decolorising action was slow, certainly much slower than that of hydrated bromine. Specimens accompany this paper. The dark colour, however, of that element makes it an unsatisfactory substance to work with, in relation to changes of tint in the bodies upon which it acts. From experiments such as I have described, as well as from theoretical observations, it has been inferred that the function of water in relation to the gases I have been considering is simply to effect their mediate liquefaction, and thereby to bring them into closer physical contact with the colouring matters than their elastic condition permits. So general a conclusion, however, as this, which would imply that a liquefied gas has the same properties as a dissolved one, is certainly in the meanwhile without proof, and is probably untenable. So far as they have been examined, the liquefied gases present properties very different from those exhibited by the same bodies when in aqueous solution, although their action on colouring matters has been less inquired into than might have been expected.

There is, moreover, this manifest distinction between the action on a colouring matter of a liquefied gas, and of an aqueously dissolved one, that in the former case the gas only is in the liquid form, the colouring matter remaining solid, whilst in the latter the water dissolves alike the colouring principle and the gas, and brings both into a condition far more favourable to chemical action than where the one only is liquid.

It is further certain that much must depend on the force of the adhesive attraction of the liquidised gas for the dry colour. A liquid which cannot wet a solid will exert little, perhaps no chemical action upon it, although it may produce a marked effect when both are dissolved in water.

Again, if the liquidised gas can dissolve the colouring matter, we may be certain that, sooner or later, it will affect it; but if it cannot dissolve it, the latter may be totally unaltered by its presence. The slow action of dry bromine is probably related, either to incapacity of quickly wetting, or of dissolving litmus; perhaps to both.

In connection with this subject, I tried an experiment with liquefied anhydrous

sulphurous acid, which yielded a result so interesting, that I mention it particularly. A piece of blue litmus-paper was exposed for three hours to a current of dry air, and then sealed up in the narrow tube in which it had been dried. The sealed bulb, containing the paper, was placed in a tube immersed in a mixture of pounded ice and salt, and carefully dried sulphurous acid transmitted through the arrangement. As soon as a sufficient quantity of the gas had assumed the liquid form, at the low temperature to which it was exposed, the open ends of the tube were sealed, and it was shaken till the bulb within broke, and allowed the paper and the liquefied gas to come in contact with each other. The paper was instantaneously soaked through, and completely wetted, but its blue colour remained totally unaltered, whilst an aqueous solution of sulphurous acid would have instantly reddened it. The liquidised gas acquired no colour itself, even after a fortnight's contact with the litmus-paper. It appeared to wet it without dissolving anything from it.

The retention of the blue tint on the part of the paper was, however, only temporary. In an hour and a quarter it had become dark purple, and the blue slowly faded, till, in twenty hours, the paper was bright red. No indication of bleaching action appeared.

I attribute the final reddening to the production of water, generated out of its elements in the litmus or paper, or both, by the influence of the sulphurous acid. For, if anhydrous liquid sulphurous acid possessed the power, *per se*, of reddening vegetable blues, there seems no reason why its action should be so long delayed, when it wetted the coloured paper so readily. And it could not owe its reddening power to water present in it, ready formed from the first, otherwise it would have reddened instantaneously.

I set aside, therefore, as at least unproven, and, further, as not probable, the dogma, that the mere passage of an elastic fluid, such as chlorine, from the state of gaseity to that of liquidity, is the whole cause of its accelerated action on colours, when dissolved in water. It seemed to me, indeed, that the acceleration of action was as much owing to the water liquefying the colouring matter as to its liquefying the gas, and that one might venture, in the spirit of the elder chemists' motto already quoted, to infer, that any liquid which dissolves alike the gas and the colouring matter, would be as efficacious as water in determining the destruction or modification of the colour. But, I have learned by experiment, that this also is too general a conclusion, and that it is quite possible for a liquid to dissolve simultaneously a colouring matter and a gas, and yet not exhibit the results which it would present if water were the solvent of both.

So far as this branch of the inquiry is concerned, I have been compelled, by want of leisure and opportunity, to limit myself almost entirely to chlorine. This gas is dissolved by chloroform, by bisulphuret of carbon, and by the volatile oils of the type of spirit of turpentine (C^3H^1). None of these liquids, when pure, contain

oxygen, and all of them dissolve several colouring matters. Yet, not only dry, but moist chlorine may be passed through solutions of the colouring principle of false alkanet root (*anchusa tinctoria*), in the solvents mentioned, without bleaching occurring.

On the other hand, solutions of blue litmus, in chloroform and bisulphuret of carbon, are bleached instantaneously by dry chlorine. I took the greatest precautions in these trials to exclude moisture. Paper was dispensed with. A solution of blue litmus was dried up in a glass tube, and desiccated in a current of air. The chloroform, or sulphuret of carbon, was repeatedly rectified over chloride of calcium, and finally distilled into a bulb communicating with the outer air, through a narrow tube filled with the same hygrometric salt. The bulb was then sealed, and placed within the tube containing the litmus at the commencement of the experiment. Chlorine was ultimately passed over the colouring matter for some minutes, in order to make certain that the gas was too dry to act unaided on the colour. The tube was then sealed, full of chlorine, and shaken till the bulb broke. The blue colour immediately disappeared, and the liquid became of a pale yellow tint.

The tincture of alkanet in chloroform or sulphuret of carbon retained its bright red colour, if kept in darkness; but less than an hour's exposure in the open air, though the sky was clouded, sufficed to turn the scale in favour of bleaching, and the colour disappeared.

From these results it appears that, contrary to DAVY'S view, chlorine can bleach though oxygen be absent, for chloroform contains none; and that neither of the elements of water is essential to its bleaching action, for sulphuret of carbon is devoid of both. The further conclusion seems unavoidable, that neither water nor any other liquid is essential to the decolorising action of chlorine, otherwise than as enabling the gas and the colour to come within the sphere of chemical action, by dissolving both. This function, water probably performs better than any other liquid, in virtue of its solvent power for most substances exceeding that of almost all other fluids.

A similar conclusion, *mutatis mutandis*, may be extended to oxygen, sulphurous, hydrosulphuric, and hydrochloric acids, but with this qualification, that specific differences may be expected to occur with all the gases named, as to their action on any one colouring matter, and with different colouring matters, as to their deportment with any one of the gases.

XXXIV.—*A Biographical Notice of the late THOMAS CHALMERS, D.D. & LL.D.*
By the Very Reverend E. B. RAMSAY, M.A., F.R.S.E.

(Read 4th March 1849.)

MR PRESIDENT,—It has been a practice from the foundation of the Royal Society of Edinburgh, to commemorate its deceased distinguished members by memoirs or biographical notices, read at the ordinary meetings of the Society. Some of these have been printed in the Transactions; and our published volumes are enriched by papers of DUGALD STEWART, Professor PLAYFAIR, Sir JOHN MACNEIL, and Dr TRAILL, on the characters and writings of ADAM SMITH, Dr HUTTON, Professor ROBISON, Sir CHARLES BELL, and Dr HOPE. A biographical notice is now due to the memory of a distinguished countryman, late Vice-President of the Royal Society; and the following remarks will, in attempting that object, make a deviation from those more severe discussions with which the time of the Society is usually occupied, in connection either with pure mathematics, natural philosophy, or natural history.

I consider it scarcely becoming for the reader of a paper to occupy the time of the Society, by details or explanations which are merely personal. I would, however, ask permission to state, that I did not enter upon this office till I knew that it had been declined by one far better qualified for its performance; one who, if named, would, I am confident, be recognised as the individual of our body best calculated to do justice to the subject.

I feel assured, however, that, from those whom I have the honour to address, I shall receive every sympathy and indulgence in the few observations which I propose to offer in attempting to delineate those literary characteristics—those efforts of practical benevolence—by which the subject of this brief notice was distinguished during the many years which, as a public man, he came before his contemporaries.

THOMAS CHALMERS was born at Anstruther, 17th March 1780, and at its parochial school received his early education. He studied at the University of St Andrews the usual course of eight years, from 1791 to 1799. He received licence from the Presbytery of St Andrews, 31st July 1799. During the sessions 1799-1800, 1800-1801, he studied at Edinburgh under Professors ROBISON, STEWART, and HOPE. He commenced his clerical life as assistant at Cavers, December 1801—was instituted to the Parish of Kilmany, Fife, 12th May 1803—removed to Glasgow, 1815—to St Andrews, as Professor of Moral Philosophy, 1823. He came to Edinburgh as Professor of Divinity, 1828, and filled that chair till the Disruption in 1843. In February 1834 he was elected a Fellow of the Royal Society, Edinburgh—in 1835 a Vice-President. In January 1834, he was elected a corresponding member of the Institute of France, before which distinguished body he read, in 1838, a paper, in

English, on the distinction between legal charity for the relief of indigence, and legal charity for the relief of disease. At the annual commemoration of Oxford, 1st July 1835, he received the honorary degree of LL.D. In that ancient and Episcopal seat of learning this degree was conferred upon the Presbyterian Professor amidst enthusiastic acclamations, without one dissenting voice. His death took place, 31st May 1847, at the age of 67: He was buried, 4th June 1847.

Of a life so long extended, and embracing so many subjects of active exertion, it is evident such a paper as the present can include only a very abridged and limited notice. It is not intended to embrace those points which belong to mere personal and private biography, or to details of questions on which there existed special and peculiar relation to his own religious communion. There is, I believe, in preparation a full Life of Dr CHALMERS, which will include a publication of his private memoirs, of his correspondence, and other personal biographical expositions. We have now to consider Dr CHALMERS as he came before the world, as he occupied a distinguished place in the observation of mankind; for his reputation was not merely Scottish, or merely British,—it was European. In this view, then, I think we may at once, for the sake of preserving something like method and order in our remarks, consider his public character under three heads:

1. As an Author.
2. As a Political Economist.
3. As a Speaker.

First, One thing strikes us at first approaching the subject of Dr CHALMERS' writings, and that is, the great *industry* which must have marked his literary labours. When we look at the array of volumes published during his lifetime; when we consider the manuscripts which he left behind; and, in addition to all this, take into account that these volumes were not written in the retired cloisters of a college, or the quiet of a country parsonage, but that he wrote in the bustle of numerous engagements, of meetings to be attended, of lectures and examinations for his classes, of correspondence to be maintained, and perhaps, above all, amidst lavish encroachments made upon his time by strangers; we must be struck with his economy of time, and with the *perseverance* of his mental efforts. How many might say of him, as the Younger PLINY wrote of his uncle, the Elder PLINY, "*Erat incredibile studium summa vigilantia. Itaque soleo ridere, cum me quidam studiosum vocant; qui si comparer illi sum desidiosissimus.*"* Dr CHALMERS was far from being, in the classical or scholastic sense of the term, a *learned* man, or a great scholar. His early education, his habits, and pursuits through life, prevented it.† But it is a pleasing

* Plin. Epist. iii. 5.

† In his Lectures on the Romans, he makes no reference to an exegetical or critical view of the passages, though in that Epistle there is a great temptation to do so. He takes the statements of the Apostle in their broadest and most general acceptance. His mind did not rest on the niceties of philological distinctions.

view of his character to find how much he admired and respected learning in others. He never undervalued an attainment because he did not possess it himself. He impressed his students with the value and importance of learning in Theology, and revered what he called the "massive erudition" of divines of the English Church. In describing the peculiarities of his mental constitution, we are at once led to the conclusion of a remarkable predominance of one, and that is an extraordinary abundance of the *imaginative* faculty,—the power of illustrating his ideas, and of setting forth his subjects of discussion with never-ending variety of imagery, comparison, and analogy. In some of his works it seems as if he could not tear himself away from the pleasure of reproducing some great truth, which he enforces under all the different garbs and attitudes with which he can invest it. There is no question that this is a very effective and important method of handling subjects, when the particular bent of the author's genius enables him to pursue it effectually, and is specially adapted for leaving a clear, distinct, and vivid impression upon the mind. In the case of Dr CHALMERS, attachment to science, and early pursuits in astronomy, chemistry, and other branches of physical science, gave him a great advantage in furnishing types for analogy and illustration. These he used on some occasions with happy effect. Indeed, he never lost his interest in the exact sciences; and, had the circumstances of life been favourable to their pursuit, would, no doubt, have been distinguished in the branches of mathematical pursuits. His mind was always alive to scientific subjects. In 1838, when introduced to the present Bishop of Nova Scotia, he heard, with much interest, the Bishop's description of the Bay of Fundy (which is in his diocese), and the enormous roll of tide coming in with a front 70 feet in height; next day Dr CHALMERS wrote a letter to the Bishop, proposing the experiment of having a delicate pendulum placed on the shore, and to watch the effect of the mass of water upon it, as they came into the bay, similar to Dr MASKELYNE's celebrated experiment at Schehalion, to test the effect of gravity, but, with the advantage over Dr MASKELYNE, that the waters would form a homogeneous mass of matter, and the result be more striking, from marking the effect of the mass *approaching* the pendulum.* When I said, therefore, that, in Dr CHALMERS, the faculty of imagination was an abounding and prominent endowment, I was far from meaning that this implied a poverty of the reasoning faculties, or defect in other mental qualities. On the contrary, he had a mind remarkably adapted for the apprehension of great principles, of broad and profound truths. He delighted to grasp primary and fundamental elements. He expatiated, with the fullest enjoyment, on reasonings of such authors as Bishop BUTLER, BACON, NEWTON. His admiration of BUTLER was intense: as an expounder of great elementary truths, he placed him in the first and highest class

* This experiment, I find, had been suggested by Professor ROBISON, in his *Elements of Mechanical Philosophy*, § 474.

of human intellects. In the dedication of his Bridgewater Treatise to the BISHOP of LONDON, he thus expressed his admiration: "I have derived greater aid from the views and reasonings of Bishop BUTLER, than I have been able to find besides in the whole range of our existent authorship." On one occasion, when some person present was animadverting upon the wealth of the Church of England, and gave, as an example of its over-abundance, the revenues of the See of Durham, the Doctor exclaimed, with characteristic eagerness, "Sir, if all that has been received for the Bishopric of Durham since the foundation of the See, were set down as payment for BUTLER'S Analogy, I should esteem it a cheap purchase." We are not to consider his admiration of BUTLER'S works as proceeding from the sameness or resemblance of their mode of reasoning, but rather from the difference. BUTLER excogitated masses of profound thought, and left them nearly as raw material, costly indeed, but not elaborated for use, except for the purpose of furnishing him with examples of *analogy* between natural and revealed theology. CHALMERS found, in this storehouse, abundant substance for practical application to the business and improvement of life. He polished and carved, and adjusted the stone which he had dug from the quarry. And thus, both as an able quarryman, and as an accomplished dresser, he has erected graceful, durable, and useful edifices for mankind. His method of exhibiting truths, in so many and in such attractive positions, has deeply impressed the minds of thousands, not only of those who were amongst his stated hearers as pupils, but amongst readers of his works generally. Although Dr CHALMERS' mode of treating his subjects was such as I have described, and though his usual mode of handling was to exhibit *one* great and leading topic, illustrated and enforced with all the profusion and imagery of a rich fancy and a powerful imagination, we should, at the same time, observe that the method is frequently applied with great ability, and with great effect in bringing forward *two ideas* where one is required to check or modify an exclusive attention to the other. Thus, for instance, in his Sermons, though he dwells upon the doctrine of the corruption of human nature, and the utter insufficiency of all mere natural efforts to merit the Divine favour, and to claim a reward at God's hand, he runs, as it were, parallel with this great truth another truth, equally important and equally authoritative, viz., that virtue in itself is beautiful, that the generous affections and good feelings must not be undervalued or depreciated, but are, in fact, desirable and estimable in their own place and their own character, and require only the right *motives* to render them acceptable. I know no writer who has more successfully elaborated this important subject. He has shewn the harmony and consistency of the two doctrines. He has upheld and vindicated the dignity and the loveliness of virtue. He has cut away all ground of merit and of human desert before God. In the same manner, as a predestinarian, he has ably and powerfully (in some instances sternly) put forward the proofs of God's pre-science and omnipotence over all his works; but, in conjunction with that great truth, he has upheld, with unflinching fidelity, the necessity of human exertion,

and he has illustrated the agency of man's own endeavours as fully and as freely as if he had been the champion of a free will entire and uncontrolled. Thus it is always in his writings. He is urging and reiterating, with all the fervency of an ardent eloquence, a great and important principle, or he is running the parallel between *two* essential truths. He is sustaining, singly and conjunctly, the position of two considerations, both of which are to be of supreme authority. The action of both is requisite for man's moral and spiritual wellbeing; at times they may, in theory, appear to be incompatible, but in action are never inconsistent. He is not, therefore, a writer of subdivisions or details. He is copious, but copious in illustrating great propositions. He offers, in this respect, a remarkable contrast to a great writer, Dr ISAAC BARROW, whose strength is in *division*. Of him it was said, that he "*exhausted* his subject." CHALMERS also exhausted his subject. But then one exhausted the practical application and minute enforcement of a truth, in all its results and consequences; the other exhausted the various forms and illustrations by which that truth itself could be enforced upon the human mind. There is nothing of the analytical method in his treatment of a subject. It is almost purely *deductive*. He sets out with a great principle, and shews, in a thousand shapes, its application and appropriation. One remark, however, we would make on this subject. Although the handling is so copious and diffusive, it is seldom deficient in strength and pungency. It would frequently be difficult to abbreviate without injury; and we find expressions constantly occurring of great force and point. It was said of Dr CHALMERS by ROBERT HALL, after hearing him preach, that his sermon went on hinges, not on wheels. Images are sometimes dangerous coadjutors. A discourse on wheels may run off the course; but a discourse on hinges must, at any rate, retain the speaker in his place, and make him exhibit the various forms and phases of his subject, by turning it in every direction to his audience.

The style of Dr CHALMERS' writing partakes of the character of his mind. It is copious and overflowing; cumbrous, perhaps, at times, for the more minute detail of a subject; but the phraseology (though occasionally somewhat eccentric) is often powerful and beautiful in the highest degree. It is impossible to illustrate these peculiarities without examples. I shall only select a few. Thus, to express the quick passage of time: "Time, with its mighty strides, will soon reach a future generation, and leave the present in death and in forgetfulness behind it." To express that the world occupies our thoughts: "Its cares and its interests are plying us every hour with their urgency." A man of shallow views in religion is a "man whose threadbare orthodoxy is made up of meagre and unfruitful positions." The external marks of piety: "A beauty of holiness, which effloresces on the countenance, and the manner, and the outward path." To say that the repentance of a sinner interests the angels, is thus worded: "His repentance would, at this moment, send forth a wave of delighted sensibility throughout the mighty throng of their innumerable legions." Persons who take

their opinions from a *partial* adoption of Scripture truth, are persons who, “retiring within the entrenchment of a few verses of the Bible, will defy all the truth and all the thunder of its warning denunciations.”

His style, with all its peculiarities, was HIS OWN. It may be called mannerism; but it is the mannerism of a powerful mind striving to express its own conceptions without regard to rules of rhetoric or the discipline of schools. It is the mannerism of genius,—one leading characteristic of which is to invest known truths and ordinary objects with new and untiring interest, and with constantly-fresh attraction; and, on this ground, it is characteristic and becoming, because it is his own; and, accordingly, these peculiarities of style pervaded his ordinary conversation and his familiar letters, just as much as they marked his more elaborate compositions; and in the ordinary intercourse of life, expressions constantly recurred to remind one of his writings. In fact, his language is merely the vehicle or medium of expressing and communicating his ideas; and we may almost say *he could not help it*. There is a danger with him (as there is with all imaginative writers) of his style being considered imaginative *only*. To many minds declamation is irksome and wearisome in the highest degree,—to them it conceals rather than develops the mental power which lies below the surface; and, not unfrequently, practical wisdom and sound argument are not duly estimated, simply *because* there is a play of imagination around them,—the lustre and richness of the setting obscures the pearls. Such authors are not unfrequently a snare to their admirers. Mannerism in authors may be easily caught by those who have no inspiration of their genius. Hence, of all writers and speakers Dr CHALMERS was one most dangerous for imitators (and amongst young and injudicious students he had imitators). What was natural to him was constraint or affectation in them. In fact, they became copyists more than imitators. Their taking his style and manner becomes a literary larceny, rather than an honourable and fair obligation. It is miserable to see men borrowing fine clothes which they know not how to wear,—affecting a glow of eloquence to cover a vapid and commonplace conception of their subject.

Secondly, As affecting the happiness of mankind, and as bearing upon their best and highest interests for time and for eternity, Dr CHALMERS was, during the whole of his public career, much occupied with the theories of Political Economy. In all ages of the world, how much of the misery of mankind may be traced to the errors and mistakes of erroneous legislation. Bad laws on excise,—on poor management,—on taxation,—on police or criminal jurisprudence, proceeding from false views of political economy, have been the most fruitful sources of crime, of misery, and degradation. The energetic and benevolent spirit of CHALMERS saw and felt the connection between a *well-doing* and a *well-living* population. He felt how much, under the Divine blessing, might be done by rulers and statesmen

to make or mar human happiness, and he took a very prominent position amongst the Christian economists of the day. Into the general question of political economy as a theory, whether of population, free trade, balance of trade, capital, taxes or tithes, I do not pretend to enter. On these points Dr CHALMERS wrote with much power and acuteness. His views on most points generally coincided with ADAM SMITH, MALTHUS, TOOKE, and authors of that school. But in one department of political economy, he took that position which has added lustre to his name, and which exhibits him to the world as the true Christian philanthropist, and the best friend of human nature. Speculations on theory and doctrine in political economy were not sufficient for one who constantly sought to do good to those who most needed the help and guidance of their fellow-Christians. We have to consider CHALMERS, then, as a practical economist; as one who, not satisfied to reason and to speculate in his study upon the best methods of improving the conditions of mankind, went forth into the cottages, the hovels, and crowded habitations of the poor, to improve their temporal, moral, and religious condition. The agencies on which he depended for improving mankind were the school, the Bible, the visitor, the pastor. Hence the titles of his works and articles on this subject, indicate what were the objects and purposes he had in view: for instance, we have "The Civic and Christian Economy of Great Towns;" "The Christian and Economic Polity of a Nation;" "Sabbath Schools;" "Bearing of Christian Economy upon Pauperism," &c. In his "Civic and Christian Economy of Large Towns," he lays down some of the most valuable and practical principles of useful charity. It is a dreary and heart-sickening prospect which the Christian philanthropist encounters when he enters upon the charity of great cities; and not only did Dr CHALMERS zealously promote amendment in that field of our erring, and destitute, and suffering countrymen, by suggesting sound principles of management, but he threw his whole energy, his persuasive eloquence, and his personal superintendence into the work.*

In 1815 he had been called to take the pastoral charge of a parish in Glasgow, a city where he knew there would be abundant opportunities for verifying his opinions and employing his resources. He commenced the publication of *The Civic and Christian Economy*, as a small periodical, and took the lead in directing the attention of the nation to the absolute necessity of extending, in our city population, means of education, of pastoral superintendence, and spiritual instruction, *similar* to what prevailed through the country parishes

* It is pleasing to remember how the last mortal days of such a man were engaged with plans of instruction for the benefit of this very class. He had for some time been entirely taken up with a School and Church, in the worst locality of the Old Town of Edinburgh. The man of high speculation became a teacher of ragged children. The Professor of Theology descended from his chair to impress the first rudiments of Christian truth upon the rude minds of a congregation the most ignorant and most neglected.

of Scotland. However ample and effective had been the supply of these elements of human improvement in the agricultural parishes and districts, the commercial and manufacturing population had quite outgrown them, and the work required to be recommenced, and taken in hand in good earnest. He was, therefore, a strenuous and constant advocate for carrying out the system of TERRITORIAL SUBDIVISION. There was a vitally important principle in the accomplishing this great end, and one which Dr CHALMERS established with great ability: it was the principle of providing for the work being *effectually* done, in the particular portions or districts chosen—not only the taking in hand the worst localities, but in every one of these laying a sufficient foundation or substratum of good, so far as you go. I think this principle was first taken up by Dr CHALMERS. It is of immense importance, and I know was adopted from Dr CHALMERS by the BISHOP of LONDON, in consequence of consultation with him regarding the plans for providing churches, schools, and parsonages, for the recently-formed masses of the destitute population of the great metropolis. The experiment was tried in Bethnal Green, where ten new parishes were formed, dividing the population into sections manageable by a pastor, and curate, and school. For want of attending to this principle, a grant of a million of money for church-building in England had been rendered comparatively ineffective. Churches and schools were set down here and there; lost in the mass of surrounding poverty and destitution, their influence was little felt,—in some cases almost unnoticed.*

I have now to notice, in connection with the political economy of Dr CHALMERS, an important incident of his life. And I must allude to an achievement which exercised the greatest influence upon his own views of the parochial system and management of the poor, and which excited astonishment, admiration, and scepticism amongst his contemporaries. I refer to the remarkable effects produced by management of the poor in St John's Parish, Glasgow, under his direction and superintendence. I will endeavour to make a plain and distinct statement of the FACTS, as established by the evidence of the parties concerned in the operation.

It is well known how exceedingly Dr CHALMERS was opposed to the support of the poor by a *compulsory assessment*; that is to say, the *ordinary* wants and the *ordinary* support of the poor. He approved of assessments for disease and casualties, for supporting infirmaries, dispensaries, and lunatic asylums, also for extraordinary emergencies of famine, pestilence, or catastrophe; but general poor-

* This principle of territorial subdivision, for which Dr CHALMERS, as a Christian philanthropist, so long contended, is at last acknowledged as the essential preparation for bringing spiritual instruction to bear upon the worst portions of our crowded and demoralised population. Lord ASHLEY, the enlightened friend of the poor, has, with the full approbation of the Premier, moved for a commission to inquire into the best method of dividing all parishes in England which contain a population of 10,000 or upwards.

laws he utterly condemned. He had termed the system a "legalised enormity." He had ascribed to the action of those laws in England all the evils under which the country suffered from pauperism. He considered them to be the bane of Christian charity, and the curse of all connected with them. It remained, then, to test by experience, when he had a proper field, an *opposite* system; and this he was determined to do in Glasgow. When, in the year 1815, he took charge of the Tron Church Parish in Glasgow, the system of management for the poor throughout the city was somewhat peculiar. The whole funds raised for the poor, whether in the shape of assessments or collections at the church-doors, were under the administration of two bodies, one called the General Session, consisting of the elders and clergy of all the parishes, and the other called the Town-Hospital, which had pensioners within its walls, and *out*-pensioners residing in the city. The whole expense of poor support had been on the increase. In 1803 it amounted to about £4000; in 1818, to about £11,000; in 1820, to £13,000. His determination was, from the first, to manage his district *without* assessment. In this wild and extravagant scheme, as it was considered, he was opposed by the General Session, by the Magistrates, by the Town-Hospital, and by the Presbytery. Indeed, the Presbytery had carried up a case against him to the General Assembly; accordingly, he was glad to be transferred to St John's Parish, which took place in 1819, and where the same obstacles and impediments to his experiment did not exist. The population was 10,000; the people, with very few exceptions, of the poorest class of manufacturers. According to the due proportion of population and pauperism, the expenditure for St John's had been about one-tenth of the expenditure for all Glasgow, or upwards of £1400 annually. His first step was to release the General Session and the Town-Hospital from all obligation to support the St John's poor, and he undertook, with his own church-door collection, to meet their wants. This collection averaged £400 a-year. With £400 a-year, therefore, he began the work. Now, of this sum, £225 were already pledged for regular cases permanently settled upon parochial relief, so that, from this collection fund of £400, £175 only remained as a *surplus* to meet and to provide for *new* cases of pauperism. But, besides the £400—the result of day collections at the church-door—there was another and an evening collection made by a very poor congregation, chiefly in halfpence, which amounted to about £80 a-year. Out of *this* £80 he resolved to provide for new cases of paupers coming upon the parish, and to leave the £400 collection to take care of the old paupers. He had previously made a minute district subdivision of the parish, and secured the assistance of zealous and intelligent deacons as visitors, one for each district. What, then, was the *result* of the system, and the degree of success with which it was accompanied? The £80 covered the whole expense of the *new* pauperism, which did not require more than £66, 6s. The £400 were, in the mean time, increasing in the hands of the kirk-session by old paupers dropping off, and by the surplus

of £175 not being required. This command of money in the hands of the kirk-session Dr CHALMERS considered to be a snare and a danger; accordingly, as he expressed it with considerable *naïveté*, he sought to provide “a safe and salutary absorbent” to take off this plethora of pecuniary oppression, and this he did by expending it all in the permanent endowment of a school. Thus the system worked, and the only disturbing force seems to have been the occasional indiscreet and injudicious introduction of charitable contributions *from without*: And certainly here is a marvellous result,—the poor of a parish absolutely managed with a success varying *inversely* as the pecuniary resources at the command of the managers. But neither the principal mover of this scheme, nor his colleagues in the work, seemed to consider it a mystery or a miracle; their solution of the problem was; 1st, that former applicants who were conscious that they did not require or deserve support withdrew, and the idea of legal right ceasing, no cases but those of absolute necessity were left; but, 2d, and chiefly, that the sympathies of the poor themselves were thus called forth, and no one allowed his neighbour to starve so long as he could spare a morsel, and when he knew that neighbour was deprived of other resources on which he could depend. The poor, in short, helped each other through their difficulties when no one else would. The artificial channels of charity being closed, a more copious and more permanent supply flowed through the natural channels of relationship and vicinage. Such was the theory; the results were indisputable. The world was still sceptical, and two solutions were offered to account for the success of a scheme which would support poor people without poor-laws. It was said, in the first place, that the system was so hard upon the people that the poor were driven out of St John’s parish, and took refuge in other parishes, where more money was expended. It was said, in the second place, that the success was the consequence of Dr CHALMERS’ personal influence and powers. That what he accomplished in St John’s, another man *could not* accomplish in St Luke’s; and that, with the man, the scheme would die out. To both of these objections an answer was ready. To the first objection it was declared, that the balance of migratory pauper population was fully in favour of St John’s; and, to come to greater exactness, it was stated that a correct account was kept of poor *leaving* St John’s, and poor *coming in* to St John’s: the result was the imports *exceeded* the exports by fourteen souls. The exchange, in fact, was *against* them, and this they considered a conclusive answer to the charge of harsh treatment of paupers. To the second objection it was replied, that the system worked for many years *after* Dr CHALMERS’ departure from Glasgow, and succeeded also in other manufacturing parishes of Scotland where it was tried—the Gorbals of Glasgow and Langholm being cited as favourable examples. How it was that, in the face of an experiment apparently so successful, detailed by himself in evidence before a parliamentary Committee, a more stringent enactment of poor-laws for Scotland should have been made, and the system be adopted for

Ireland ; or how, in the public mind, it did not produce a stronger feeling *against* compulsory charity in general, I am not competent to decide. The facts are indisputable, and were, during the whole of Dr CHALMERS' lifetime, after he left Glasgow, referred to in corroboration of the correctness of his theory, and as a standing proof that charity, if left to itself, *would* supply means for the maintenance of the poor, and a maintenance of a more suitable and effective nature than could be done by a compulsory assessment. In all his treatises on Management of the Poor, he alludes with unshaken confidence to the great Glasgow experiment.

The complete and detailed account of the experiment will be found in four articles, forming the general Appendix on Pauperism, in the sixteenth volume of his collected Works, including his own evidence before the Committee of the House of Commons on the subject of a poor-law for Ireland. Great prejudice existed (in England especially) against the whole system, as harsh, and severe, and cruel, and numerous objections were urged against the possibility of success. One objection brought by the writers of articles on Poor-laws in the Quarterly Review, against the plan of withholding an assessment for supporting the poor, and throwing them on the natural or voluntary principle of charity, was an unjust one, and indicated a misapprehension of the whole system upon which that method was grounded. It was said that the principles advocated by Dr CHALMERS were an encouragement to vagrancy and mendicity. Therefore, according to this view, it was merely a question whether we were to have parish *paupers* or highway and street *beggars*. But the writers of those articles did not consider that on no point was Dr CHALMERS' views of pauperism more decided than on the *discouragement* of relief to common vagrants and beggars. The principles on which the Glasgow experiment was accomplished, when carried through, would have entirely put down common beggars ; and Dr CHALMERS drew an ingenious and novel argument *against* promiscuous charity from the example of our Lord, as recorded in the four Gospels. He healed all diseases and sickness in those who came to him ; but only on two occasions did he supply by miracle the multitudes with *food*. These were occasions of urgency ; and when he found that they came to him idly and *on account* of food, he firmly withheld it.

But, Sir, I would now turn to another subject connected with the great question of a nation's civic economy—and that is the Endowment of its Church and Universities. On these points Dr CHALMERS has written with remarkable force and much enthusiasm. And he has propounded the compulsory endowment theory for ecclesiastical and educational objects as vigorously as he has disclaimed it for sustaining the poor. His essay "On Ecclesiastical and Academical Endowments" has been described in the Quarterly Review (vol. xlv., p. 527) "as one of the most vigorous and eloquent defences of such endowments that ever proceeded from the press—a treatise which would alone have been sufficient to immortalize its author." This is high praise from such a quarter : But I think it is

deserved, and fully deserved. There is great power of argument, felicitous illustration, and a glowing enthusiasm of admiration, for the theological literature, and the erudition, and the learning, and the eminent men produced by the ecclesiastical and academical endowments of England. In reference to the Church of England he writes:—"There are many who look with an evil eye to the endowments of the English Church, and to the indolence of her dignitaries. But to that Church the theological literature of our nation stands indebted for her best acquisitions; and we hold it a refreshing spectacle, at any time that meagre Socinianism pours forth a new supply of flippancies and errors, when we behold, as we have often done, an armed champion come forth in full equipment, from some high and lettered retreat of that noble hierarchy; nor can we grudge her the wealth of her endowments, when we think how well, under her venerable auspices, the battles of orthodoxy have been fought,—that in this holy warfare they are *her* sons and *her* scholars who are ever foremost in the field—ready at all times to face the threatening mischief, and by the weight of their erudition to overturn it."

In the same work, "On the Use and Abuse of Literary and Ecclesiastical Endowments," he thus writes of Oxford and Cambridge:

"We cannot conclude this passing notice of the Universities of England, without the mention of how much they are ennobled by those great master-spirits, those men of might and of high achievement,—the Newtons, and the Miltons, and the Drydens, and the Barrows, and the Addisons, and the Butlers, and the Clarkes, and the Stillingfleets, and the Ushers, and the Foxes, and the Pitts, and Johnsons, who, within their attic retreats, received that first awakening, which afterwards expanded into the aspirations and the triumphs of loftiest genius. This is the true heraldry of colleges. Their family honour is built on the prowess of sons, not on the greatness of ancestors; and we will venture to say, that there are no seminaries in Europe on which there sits a greater weight of accumulated glory, than that which has been reflected, both on Oxford and Cambridge, by that long and bright train of descendants who have sprung from them. It is impossible to make even the bare perusal of their names without the feeling, that there has been summoned before the eye of the mind the panorama of all that has upheld the lustre, whether of England's philosophy, or of England's patriotism, for centuries together. We have often thought what a meagre and stinted literature we should have had without them; and what, but for the two Universities, would have been the present state of science or theology in England! These rich seminaries have been the direct and the powerful organs for the elaboration of both; and both would rapidly decline, as if languishing under the want of their needful aliment, were the endowments of colleges swept away. It were a truly Gothic spoliation; and the rule of that political economy which could seize upon their revenues, would be, in effect, as hostile to the cause of sound and elevated learn-

ing in Britain, as would be the rule of that popular violence which could make havoc of their architecture, and savagely exult over the ruin of their libraries and halls."

Now, throughout the whole of this Essay on Endowments, and in the lectures which he delivered with so much success in London before Princes of the Blood Royal, Peers, Bishops, Ministers of State—the highest and the most intelligent of the land—it will be observed that he constantly advocated compulsory enactment or permanent endowment for support of the objects on which he lectured. He maintains this opinion chiefly on the ground, that individuals are not in all cases the best judges of their own interests, and will not always voluntarily employ their means in that way which is most conducive to their own benefit and that of society. In religion the supply must not be delayed till the demand come forth to claim it. The demand is, in fact, to be created, for there is no natural appetency for religious instruction; and so, as he himself declares, "the great argument for *literary* endowments is founded on the want or weakness of the natural appetency for *literature*." Now the difficulty which most people have in following Dr CHALMERS' views on pauperism, arises out of this very argument of his own in defence of academical and ecclesiastical endowments. For may it not be urged, if the principle of provision by compulsory payment be so clear and applicable to the case of sustaining ecclesiastical and academical institutions, why is it not equally applicable to provision for maintaining the poor? The natural appetency for *charity* is frequently quite as dull and torpid as natural appetency for religious or literary instruction. As a high and moral obligation, should it not therefore also be compulsory equally with the others? But the poor do assist each other in their poverty. But then, again, it may be asked, why should the support of the poor be *confined* to the poor? They see their brethren suffer, and charity is forced upon them. The more wealthy neighbours live at a distance. If human distress were forced upon *their* notice, *they* too would help. But they do not witness suffering at their doors, and so they forget it. But ought they to be allowed to forget it? Whatever force there may be in these or similar arguments, one thing is clear, the Glasgow experiment did not practically convince the Legislature that they might now abandon all compulsory assessment for the poor, and throw themselves upon the natural charity of mankind for better attaining, *without* compulsion, the same object. This, however, be it remembered, is no real argument either against the truth of the statement or the soundness of the theory. The highest exercise of Christian charity is undoubtedly the voluntary; indeed, giving to the poor except voluntarily, is not charity at all. The principle may be pure and right, but human nature is not perhaps yet fitted to receive it, or capable of acting upon it. A time may come when the world will discern and receive it, when the outpourings of Christian love to the brethren will so promptly and so amply supply all the wants of the poor, that assessments will

be unheard of. Men will do that on principle which now they must do by legal enactment. Such a state of things would follow the universal prevalence of Christian charity in men's hearts, and is not therefore to be considered a mere chimera. Should this triumph of principle and of love ever be achieved amongst mankind, what will be said and thought in *those* days of the mind that, amidst scepticism and ridicule, had resolutely maintained the principle, nay, which had in its own sphere of action practically worked out its successful application?

Thirdly, And now, Sir, we have to consider Dr CHALMERS as an orator. He was distinguished as a preacher, as a speaker at public meetings, and as a member of ecclesiastical courts. We attribute to him in all these positions, especially in the pulpit, the quality of a high and a peculiar *eloquence*, and we have the utmost confidence in the correctness of this estimate; for if CHALMERS were not eloquent, where, we may ask, is eloquence to be found? Judge by the effects upon men's minds, and say, is not that eloquence which captivates and enchains the hearers? Is not that eloquence which delights all classes of mankind, all ages, all situations of life? Is not that eloquence which ensures an interest and admiration unbroken, and which to the last attend every appearance of the speaker in public? Nor was this attraction the result of art, or the merely artificial embellishments of oratory. It was not in graceful and studied action. It was not in musical and practised intonation. It was not in the purity and beauty of the accent. All these were plain, homely, to some hearers quite unusual; and yet how extraordinary were the effects of his eloquence! Such effects, then, being the result, not of artificial embellishments or natural grace of manner, tones of voice or skilful action, are attributable to the power and energy of the preacher's own spirit, to the vivid pictures which he brought before his hearers, the fervid oratory with which he took captive the heart and understanding. One important element of his success as a preacher, I think, was the impression of earnest truth and sincere conviction existing in his own mind. As to the mode of arguing and the style of composition, the remarks already made upon Dr CHALMERS as an author, apply to him as a preacher. Indeed, all his writings seem as if composed for *spoken* communication, and the method is favourable to producing one vivid and powerful effect upon the mind. No one indeed, who has not *heard* Dr CHALMERS in his day of vigour, can form a correct idea of his power as a pulpit orator. It is now thirty years since his Astronomical Sermons were delivered, and though I suppose no discourses ever produced a greater effect, the nature of that effect must be little known to the younger members of the present generation. The fame of a preacher mainly depends (like the fame of an actor or singer) upon traditionary description. In many cases, the perusal of written discourses gives little notion of the effect in delivery; in some cases, as of WHITFIELD, Dean KIRWAN, and other eminent preachers, who, in their day, produced marvellous sensations, they give *no*

notion at all ; the effect must have arisen entirely from the *manner*. And when we consider how much pleasure the printed Sermons of Dr CHALMERS now afford to the intelligent reader, we may easily imagine the delight with which they must have been heard, coming with all their novelty and fervour, fresh from the preacher's lips. To enter into any description or analysis of compositions so well known as these published Sermons, would be here quite out of place. I may perhaps refer to one or two passages as specimens, and favourable illustrations of his own peculiar manner. In his sermon "On Cruelty to Animals" (preached in consequence of an endowment), he has occasion to shew that suffering is often inflicted on the inferior creatures by man, not for the purpose of torment, but that it follows whilst he is occupied with other considerations and excitements ; and as an example, to illustrate the absence of any cruel *purpose* for the mere infliction of pain, he described in glowing colours the excitement and the interest of an English hunting-field, and he terms it "this favourite pastime of joyous old England, on which there sits a somewhat ancestral dignity and glory." And he described the "assembled jockeyship of half a province," the assemblage "of gallant knight-hood and hearty yeomen," and he spake of "the autumnal clearness of the sky," and "the high-breathed coursers," and "the echoing horn"—"the glee and fervency of the chace,"—"the deafening clamour of the hounds," and "the dying agonies of the fox," in such a strain of animation, that Lord ELCHO's huntsman, who was present, declared that he had difficulty in restraining himself from getting up and giving a *vue-holla*.

Of a far different character was the scene he drew in the conclusion of a sermon preached for the benefit of a Society in aid of Orphan Children of Clergymen. He described the sons and daughters of a Scottish pastor obliged, at their father's death, to leave the peacefulness of their father's dwelling, and appealed to his hearers for their assistance in behalf of those who were so friendless and so dependent. "With quietness on all the hills, and with every field glowing in the pride and luxury of vegetation, when summer was throwing its rich garment over this goodly scene of magnificence and glory, they think, in the bitterness of their souls, that this is the last summer which they shall ever witness smiling on that scene which all the ties of habit and affection have endeared to them ; and when this thought, melancholy as it is, is lost and overborne in the far darker melancholy of a father torn from their embrace, and a helpless family left to find their way unprotected and alone, through the lowering futurity of this earthly pilgrimage." I heard that sermon, and the tears of the *father* and the preacher, fell like rain-drops on the manuscript.

In his Sermon on the Death of Dr THOMSON, describing in a *picturesque* point of view, the proximity of tenderness and power, of gentleness and strength, in the same human character, he added this happy illustration : "This is often exemplified in those alpine wilds, where beauty may at times be seen embosomed in

the lap of grandeur, as when, at the base of a lofty precipice, some spot of verdure or peaceful cottage home seems to smile in more intense loveliness, because of the towering strength and magnificence which are behind it."

In a very striking Sermon on the "Paternal Character of God," when drawing "the picture of moral and pleasing qualities of mind and affections, *apart* from the love of God, or from the influence of divine grace upon the soul," he adds this beautiful illustration: "There is beauty in the blush of a rose, and there is beauty of a higher character in the blush that mantles the cheek of modesty, and yet there may be just as little of loyalty to God in the living as in the inanimate object."

Of his speaking at public meetings, I had fewer opportunities of judging than I have had of his pulpit discourses. On some of those occasions, he produced great impression. But, perhaps, the most distinguished of such appearances was on occasion of a public meeting held in Edinburgh, in the year 1829, on the subject of a bill then pending in Parliament, commonly called the Catholic Emancipation Bill. Dr CHALMERS, in opposition to the views of the generality of those with whom he usually acted in public affairs, civil and ecclesiastical, was in favour of that emancipation, and of the admission of Roman Catholics, Peers and Commoners, into the two Houses of Parliament. The effects of that speech have been described as something very remarkable. An excitement and enthusiasm pervaded the large and closely-crowded assemblage, seldom witnessed in modern times. I heard our most distinguished Scottish critic, who was present on the occasion, give it as his deliberate opinion, that never had eloquence produced a greater effect upon a popular assembly, and that he could not believe more had ever been done by the oratory of DEMOSTHENES, CICERO, BURKE, or SHERIDAN. And this was a case simply of eloquence. For the speech delivered was not remarkable either as to argument or literary composition. It was reported in the newspapers at the time, but has not been deemed worthy of being included in his collected Works. I shall refer to one incident only connected with his speaking in the General Assembly,—and the result was the more remarkable as the reply must have been unpremeditated. He had spoken very strongly against the principle of a clergyman holding the two offices of Professor and Pastor. It was alleged against him that such opinions were, at any rate, inconsistent in him, inasmuch as he had himself been an aspirant for the Chair of Mathematics, and justified the union of professional and pastoral duty. His answer to the charge was striking,—“I feel obliged,” he said, “I feel obliged to the Reverend Gentleman for reviving my pamphlet, and for bringing me forward to make my public renunciation of what is there written. I now confess myself to have indeed been guilty of a heinous crime, and I now stand a repentant culprit before the bar of this Venerable Assembly.” After stating that he had then certainly maintained that a devoted and exclusive attention

to the study of mathematics was not dissonant to the proper habit of a clergyman, he thus concluded:—

“Alas! Sir, so I thought in my ignorance and pride. I have now no reserve in saying that the sentiment was wrong, and that, in the utterance of it, I penned what was most outrageously wrong. Strangely blinded that I was! What, Sir, is the object of mathematical science? Magnitude, and the proportions of magnitude. But then, Sir, I had forgotten *two magnitudes*, I thought not of the littleness of Time,—I recklessly thought not of the greatness of Eternity.”

An important class of productions and of labours come under this head, and occupy a place somewhat intermediate between the pulpit and the public meeting. I refer to his *Lectures* on Moral Philosophy,—on Evidences,—and on Theology. These lectures were all composed and written with great care; but he introduced, parenthetically, further explanations and illustrations extempore. The remarks made, on his manner of discussion in the pulpit, apply also to his manner of discussion in the Chair. The same fulness of illustration, the same energetic and irresistible enforcement of some leading and fundamental truth,—the same fervour, and the same sincerity. These did not fail to secure the attention, and to engage the affections, of his class. Many persons, not intended for the ministry, attended these lectures; and we have reason to believe that his discussions on Evidences, on BUTLER’S Analogy, and on Natural Theology, have, in this generation, exercised considerable influence upon the supposed sceptical tendencies of the northern mind. I will only adduce one passage in illustration of his lecture style. In his Lectures on Natural Theology, he draws an argument in favour of an unquestionable act of God in creation, from the geological appearances of the world. The commencement of the present economy, after the destruction of the previous economy, is a convincing argument against the *eternity* of creation. The whole reasoning is ably and ingeniously conducted, and, at the same time, clothed in language of a high and imaginative eloquence. He thus asks, How could the present world, after former destruction, be produced otherwise than by a new and palpable act of creation? “Is there ought in the rude and boisterous play of a great physical catastrophe that can germinate those exquisite structures, which, in our yet undisturbed economy, have been transmitted in pacific succession to the present day? What is there in the rush and turbulence, and mighty clamour of such great elements of ocean, heaved from its old resting-place, and lifting its billows above the Alps and the Andes of a former continent? What is there in this to charm into being the embryos of an infant family, wherewith to stock and to people a now desolate world? We see, in the sweeping energy and uproar of this elemental war, enough to account for the disappearance of all the *old* generations, but nothing that might cradle any *new* generations into existence, so as to have effloresced on ocean’s deserted bed, the life and the loveliness which are now before our eyes. At no juncture, we apprehend, in the history of the world, is the interposition of Deity more

manifest than at this; nor can we better account for so goodly a creation, emerging again into new forms of animation and beauty from the wreck of the old one, than that the SPIRIT of GOD moved on the face of the chaos; and that nature, turned by the last catastrophe into a wilderness, was again repopled at the utterance of His word."

We naturally feel an interest about the appearance and address, the personal habits and peculiarities, of those who have been distinguished in their day and generation. Such peculiarities, in the subject of this biographical notice, must have been familiar to many now present. For upwards of twenty years I enjoyed the privilege of friendly intercourse; and it is a pleasing, though melancholy office of memory to recall those traits which rendered his society so interesting, and so delightful. I think I can safely say I never left his company without having some sentiments or expressions in my mind which I felt were worthy to be remembered. There was a mixture of guileless simplicity and acuteness, of playful humour and vigorous conversation, of urbanity and earnestness, which cannot be forgotten. His face was at times radiant with benevolence and kindly feeling. Like many powerful and striking countenances, the expression was chiefly in the mouth. The eye was dull, and often inanimate,—this, in combination with the massive brow, rendered the play of the lower part of the face the more striking;—on those occasions especially, when, after being silent and apparently abstracted, he would burst forth into some strain of admiration, or some strong expression of his opinion regarding the topic of conversation, or not unfrequently some humorous or ludicrous combination of thought. His habits were social—he was hospitable, and enjoyed the hospitality of his friends. Though, in his whole demeanour, utterly inartificial, he was eminently courteous and pleasing in his address. Though as plain and unpretending in his manners as possible, no man had a more acute perception of refinement of manners in others. I recollect his *enthusiastic admiration* of the polished and refined manners of an English dignitary of high birth and station, in whose company we had been.

In his ordinary conversation, there was constantly occurring some appropriate and striking expression. In fact he never expressed himself exactly like other people, and yet without any straining or affectation of effect. No man could have been more conscientiously and sincerely attached to his own Church, both from argument and from those numerous national associations and social feelings which are sometimes more binding even than convictions of reason. He was yet quite free from intolerance and bigotry, and illiberal prejudice. He admired and loved what was great and amiable in those from whom he differed, and differed in many important principles. Thus, as appears from passages I have quoted, he spoke with enthusiasm of the learning and the position of the Church of England. He gloried in the grandeur of her Gothic architecture, as much as any of her own

children could do. On one occasion I recollect his describing, with much interest, a Sunday he passed at Winchester, when a guest of the Bishop, and dilating on the services and "staff of the Cathedral," as he called them; the question was put, evidently expecting an unfavourable reply, "But, Doctor, what did you think of the *chanting*?" His immediate answer was, "Very grand, Sir!" He could discern what was good, and exercise kindness and forbearance towards those from whom he differed far more widely than he did from the Church of England. Thus, when told of a purpose on the part of Roman Catholics to establish in the old town a system of visiting the poor by Sisters of Charity, similar to the visiting in Paris and other continental cities, he exclaimed he was glad to hear it, as it might induce a similar plan of visits from Protestant Sisters of Charity. In his examination before the Committee of the House of Commons respecting his management of St John's, Glasgow, the question was put, "Did you meet with any contradiction on the part of the Roman Catholic clergy of Glasgow?" He replied, "Not in the least; for the clergyman was a party in the negotiation. He attended our meetings, and there was mutual understanding between the clergyman and the members of the committee." (This mutual understanding was, that there should be no attempts on either side at proselytizing, but simply to give education with reading of Scripture. There was this compromise made regarding schools with Roman Catholic children: The Roman Catholic clergyman consented to the use of the Bible as a school-book, according to the authorised version; the Protestants consenting to have Roman Catholic teachers). He had before said to the Committee that he attended at a Roman Catholic school from the delight he had in witnessing the display of native talent among the young Irish, and that he was received with welcome and respect by the Roman Catholic master, who asked him to address the children. Having done so freely, and according to his views, the master thanked him most cordially—and then he added, "This convinced me that a vast deal might be done by kindness, and by discreet and friendly personal intercourse with the Roman Catholics. I may also observe that, whereas it has been alleged that, under the superintendence of a Roman Catholic teacher, there might be a danger of only certain passages of Scripture being read to the exclusion of others, so far as my observation extended, he read quite indiscriminately and impartially over Scripture."

Dr CHALMERS going to the Roman Catholic schools to witness "display of native talent amongst the young Irish," reminds me of a trait in his character not generally perhaps understood, but which was on occasions very marked; I mean his turn for humour and keen sense of the ridiculous. At times he could not control his merriment at ludicrous and grotesque combinations; and I can easily imagine his exquisite enjoyment of answers from the half-naked little Irish urchins. Their odd mixture of acuteness and self-possession, with random confusion of ideas, would be to him irresistibly comic. He had an instinctive sense of the

ludicrous combination of circumstances, and narrated them with great effect. One of the most amusing scenes I remember, was his own description of what happened at Manchester when he had consented to preach a sermon for some public object at a large chapel in that town. He had not been thinking about the matter after he had given his consent to preach ; but his eye was attracted by seeing his own name in a printed paper, like an immense play-bill, posted on the walls all about the town. This was a *programme* of the ceremonial for the day. There were to be prayers, anthems, choruses from Handel's Oratorios, and a sermon by the celebrated Dr CHALMERS of Edinburgh ! Excessively annoyed at all this display he refused to take any part, or to preach on the occasion. The directors expostulated, and represented what would be the effects of his withdrawal, and the disappointment of the public. The matter was compromised, and Dr CHALMERS was to sit in the vestry till the proper time for him to come out and preach his sermon. But his troubles then only began, for, unfortunately, an anthem, with full instrumental accompaniments, was appointed to follow the sermon. The orchestra being placed immediately behind the pulpit, and more occupied with anticipations of their own performance than with anything else, the musicians annoyed and disturbed the preacher through the whole sermon by their preparations and preliminaries for the grand chorus, "actually," as the Doctor exclaimed, "tuning their very trombones close at my ear before I had finished."

One other feature of mental constitution, and one only I will refer to ; and it is an important one, as having its influence not only upon the imagery and ornament of his literary compositions, but, in some instances, upon the general current of his opinion and speculations, and that is his deep admiration of the beautiful in the *material* universe. This admiration was intense, it amounted to a passion, and he evidently had exquisite enjoyment in the contemplation of Nature's works, or rather, I should say, of the goodness and wisdom of the Creator, whether displayed in the wildness or loveliness of natural scenery, the delicate tints and texture of a flower, or the magnificence of the starry heavens. Hence, although no artist himself, he had the greatest interest and enjoyment in the society and conversation of artists. He delighted to hear their remarks on subjects of taste in connection with scenery ; on the tints of the landscape, the sky, the ocean, the forms and varieties of clouds, the appearances most suitable for picturesque representation, and the practical rules observed in transferring to the canvas imitations of what is in nature. Hence in his moral reasoning we find all his references, in the way of analogy or illustration, to the beauties and appearances of the natural world, expressed with so much freshness and feeling of reality. He always seems to be impressed with the conviction that, though a fallen world, the fall has chiefly affected the moral and spiritual nature of man himself ; that, though the ground was cursed for man's transgression, and so lost the power of supporting the species without toil and labour ; yet that, in the *material* world around us,

there remains an impress of primeval beauty,—that there are forms unscathed by the penalties of the primeval curse, and flowers as delicate and fair as those that bloomed in paradise. These sentiments of intense admiration for an external and material world, exercised, I think, considerable influence in modelling his views, and shaping his arguments for *Natural Theology*. He ever delighted in tracing the lineaments of God's moral character in the mirror of the material world, as reflecting his attributes, and as displaying the nature of his handiwork. He deprecated the notion of any essential connection between materialism and sin; and as the abode of man in innocence was a *terrestrial* one, so he believed that in glory there would be provided a new heaven and a new earth, with visible magnificence and material splendour, to be a fitting habitation, and to furnish fitting occupations and enjoyments, for the new and glorified bodies of the redeemed.

I have now, I think, touched upon all those points of character, and all those public acts and deeds, of which I have been capable of forming a judgment, and which have occurred to me as strictly coming within the province of such a paper as the present. In these remarks I have endeavoured to look upon Dr CHALMERS, not as a private friend, but as a public character. I have sought to give a fair transcript of the man as he appeared before us, with no undue partiality arising from those personal feelings of regard and admiration which I am proud to acknowledge. I am certain that those who knew him best esteemed him most. His character bore investigation; and, I think, whatever opinion, in a literary or critical point of view, the world may form of the posthumous volumes, on Scripture Reading, which have been laid before them, it must be allowed that they furnish unequivocal indications of a mind constantly and habitually occupied with sacred things,—of private thoughts and of retired meditations, ever conversant with God and with His holy word.

And now, Sir, to conclude. It will hardly be supposed that I should expect unanimity of opinion in all those questions by which the name of our late distinguished Vice-President has been brought before the notice of his contemporaries. On every subject, indeed, where there are not positive moral precepts or mathematical demonstration, the different tastes and habits of mankind will lead to a difference in their judgments. Different styles of writing, for instance, are congenial with different mental constitutions. The eloquence which affects and even overpowers one man, has little charm or influence over the mind and feelings of another. The early associations of individuals,—the various points of view from which they contemplate the actions of public men, almost inevitably lead to differences in their decisions. In great questions of national or ecclesiastical policy, the conduct utterly condemned by one party, will often be extravagantly lauded by another. It was impossible for any one to take so prominent a position in that

recent movement of our country,—the Disruption of a National Church, with all its accompanying excitements,—its breaking up of old associations,—its contending opinions and hasty sayings,—without running counter to the opinions of many early admirers, without partially, at least, alienating himself from former friends, and separating himself from former coadjutors. On such points it were vain to expect a concurrent judgment on all he has done and said. But of this I feel assured, that none who have had favourable opportunities of personal acquaintance with his character and disposition,—that none who have deeply entered upon a study of his writings, so as fully to appreciate the lofty and benevolent spirit of their sentiments and tendencies, will hesitate to admit that he was both a good and a great man,—that he was imbued with the spirit of Christian philanthropy,—that he had a fervent mind, keen sensibility, and indomitable energy. His highest praise, but, at the same time, his *just* eulogium is, that his fervency of spirit, his sensibility, and his energy, were all exercised and called forth in the one great and magnificent cause,—promoting the glory of God and the welfare of Mankind. In all his meditations, and in all his labours, he had ever distinctly before his eyes the advancement of his fellow-creatures, in their best and truest relations to this world and the world to come.

His greatest delight was to contrive plans and schemes for raising degraded human nature in the scale of moral being,—the favourite object of his contemplation was human nature attaining the highest perfection of which it is capable: and, as that perfection was manifested in saintly individuals, in characters of great acquirement adorned with the graces of Christian piety. His greatest sorrow was to contemplate masses of mankind hopelessly bound to vice and misery by chains of passion, ignorance, and prejudice. As no one more firmly believed in the power of Christianity to regenerate a fallen race,—as faith and experience both conspired to assure him that the only effectual deliverance for the sinful and the degraded was to be wrought by Christian education, and by the active agency of Christian instruction penetrating into the haunts of vice and the abodes of misery;—these acquisitions he strove to gain for all his beloved countrymen; for these he laboured, and for these he was willing to spend and be spent. From the fields of earthly toil and trial he has been removed, and he has entered into his rest. The great business of Christian benevolence, and the contest with ignorance and crime, are left in other hands. But *his* memory will not die, nor his good example in these things be forgotten. His countrymen will do his memory justice. Of the thousands who were assembled to witness the funeral procession which conveyed his earthly remains to the tomb, all felt conviction on that day that a Great Man had fallen in Israel,—that a Scotchman had gone to the grave, of whom Scotland might be proud,—a Scotchman who had earned a name in his country's annals, and a place in his country's literature, which will not pass away.

XXXV.—*On the Theory of Rolling Curves.* By Mr JAMES CLERK MAXWELL.
Communicated by the Rev. Professor KELLAND.

(Read, 19th February 1849.)

There is an important geometrical problem which proposes to find a curve having a given relation to a series of curves described according to a given law. This is the problem of Trajectories in its general form.

The series of curves is obtained from the general equation to a curve by the variation of its parameters. In the general case, this variation may change the form of the curve, but, in the case which we are about to consider, the curve is changed only in position.

This change of position takes place partly by rotation, and partly by transference through space. The rolling of one curve on another is an example of this compound motion.

As examples of the way in which the new curve may be related to the series of curves, we may take the following:—

1. The new curve may cut the series of curves at a given angle. When this angle becomes zero, the curve is the envelope of the series of curves.

2. It may pass through corresponding points in the series of curves. There are many other relations which may be imagined, but we shall confine our attention to this, partly because it affords the means of tracing various curves, and partly on account of the connection which it has with many geometrical problems.

Therefore the subject of this paper will be the consideration of the relations of three curves, one of which is fixed, while the second rolls upon it and traces the third. The subject of rolling curves is by no means a new one. The first idea of the cycloid is attributed to ARISTOTLE, and involutes and evolutes have been long known.

In the “History of the Royal Academy of Sciences” for 1704, page 97, there is a memoir entitled “Nouvelle formation des Spirales,” by M. VARIGNON, in which he shews how to construct a polar curve from a curve referred to rectangular co-ordinates by substituting the radius vector for the abscissa, and a circular arc for the ordinate. After each curve, he gives the curve into which it is “unrolled,” by which he means the curve which the spiral must be rolled upon in order that its pole may trace a straight line; but as this is not the principal subject of his paper, he does not discuss it very fully.

There is also a memoir by M. DE LA HIRE, in the volume for 1706, Part II., page 489, entitled,—“Methode generale pour réduire toutes les Lignes courbes à des Roulettes, leur generatrice ou leur base étant donnée telle qu’on voudra.”

M. DE LA HIRE treats curves as if they were polygons, and gives geometrical constructions for finding the fixed curve or the rolling curve, the other two being given; but he does not work any examples.

In the volume for 1707, page 79, there is a paper entitled,—“Methode generale pour déterminer la nature des Courbes formées par le roulement de toutes sortes de Courbes sur une autre Courbe quelconque.” Par M. NICOLE.

M. NICOLE takes the equations of the three curves referred to rectangular co-ordinates, and finds three general equations to connect them. He takes the tracing-point either at the origin of the co-ordinates of the rolled curve or not. He then shews how these equations may be simplified in several particular cases. These cases are,—

1st, When the tracing-point is the origin of the rolled curve.

2d, When the fixed curve is the same as the rolling curve.

3d, When both of these conditions are satisfied.

4th, When the fixed line is straight.

He then says, that if we roll a geometric curve on itself, we obtain a new geometric curve, and that we may thus obtain an infinite number of geometric curves.

The examples which he gives of the application of his method are all taken from the cycloid and epicycloid, except one which relates to a parabola, rolling on itself, and tracing a cissoid with its vertex. The reason of so small a number of examples being worked may be, that it is not easy to eliminate the co-ordinates of the fixed and rolling curves from his equations.

The case in which one curve rolling on another produces a circle is treated of in WILLIS'S *Principles of Mechanism*. Class C. *Rolling Contact*.

He employs the same method of finding the one curve from the other which is used here, and he attributes it to EULER (see the *Acta Petropolitana*, vol. v. i.).

Thus, nearly all the simple cases have been treated of by different authors; but the subject is still far from being exhausted, for the equations have been applied to very few curves, and we may easily obtain new and elegant properties from any curve we please.

Almost all the more notable curves may be thus linked together in a great variety of ways, so that there are scarcely two curves, however dissimilar, between which we cannot form a chain of connected curves.

This will appear in the list of examples given at the end of this paper.

Let there be a curve KAS, whose pole is at C.

Let the angle $DCA = \theta_1$ and $CA = r_1$ and let

$$\theta_1 = \varphi_1(r_1).$$

Let this curve remain fixed to the paper.

Let there be another curve BAT, whose pole is B.

Let the angle $MBA = \theta_2$, and $BA = r_2$, and let

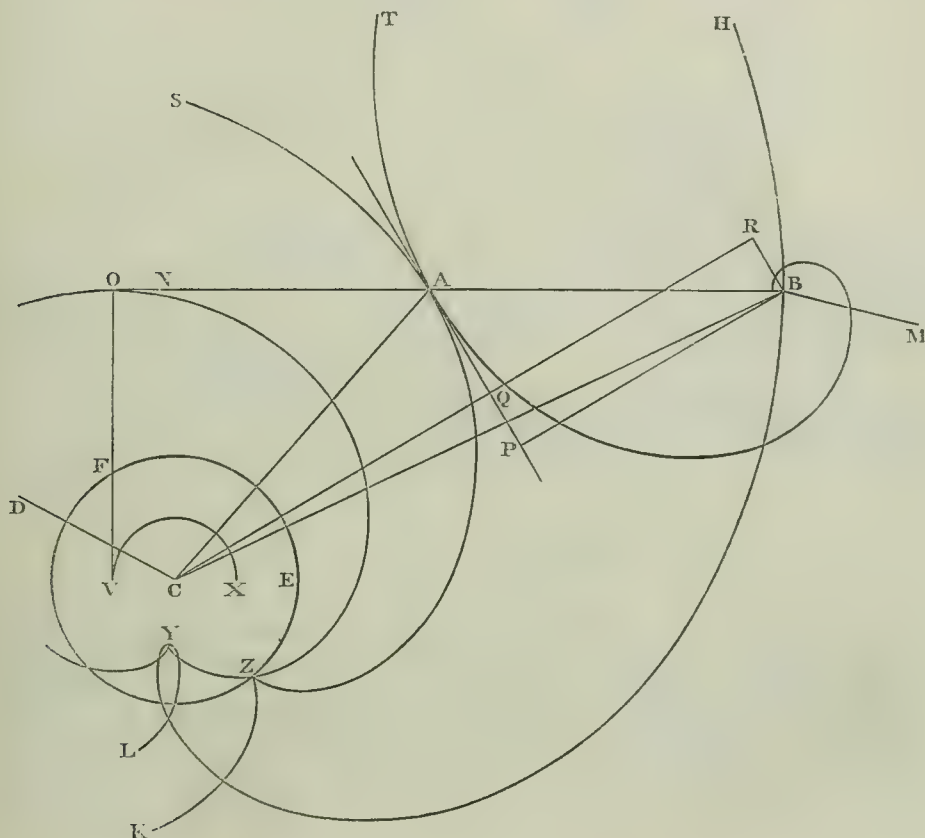
$$\theta_2 = \varphi_2(r_2).$$

Let this curve roll along the curve KAS without slipping.

Then the pole B will describe a third curve, whose pole is C.

Let the angle $DCB = \theta_3$, and $CB = r_3$, and let

$$\theta_3 = \varphi_3(r_3).$$



We have here six unknown quantities, $\theta_1 \theta_2 \theta_3 r_1 r_2 r_3$; but we have only three equations given to connect them, therefore the other three must be sought for in the enunciation.

But before proceeding to the investigation of these three equations, we must premise that the three curves will be denominated as follows:—

The Fixed Curve, Equation, $\theta_1 = \varphi_1(r_1)$

The Rolled Curve, Equation, $\theta_2 = \varphi_2(r_2)$

The Traced Curve, Equation, $\theta_3 = \varphi_3(r_3)$

When it is more convenient to make use of equations between rectangular co-ordinates, we shall use the letters $x_1 y_1, x_2 y_2, x_3 y_3$. We shall always employ

the letters s_1 s_2 s_3 to denote the length of the curve from the pole, p_1 p_2 p_3 for the perpendiculars from the pole on the tangent, and q_1 q_2 q_3 for the intercepted part of the tangent.

Between these quantities, we have the following equations:—

$$\begin{aligned} r &= \sqrt{x^2 + y^2} & \theta &= \tan^{-1} \frac{y}{x} \\ x &= r \cos \theta & y &= r \sin \theta \\ s &= \int \sqrt{r^2 + \left(\frac{dr}{d\theta}\right)^2} d\theta & s &= \int \sqrt{1 + \left(\frac{dy}{dx}\right)^2} dx \\ p &= \frac{r^2}{\sqrt{r^2 + \left(\frac{dr}{d\theta}\right)^2}} & p &= \frac{y dx - x dy}{\sqrt{(dx)^2 + (dy)^2}} \\ q &= \frac{r \frac{dr}{d\theta}}{\sqrt{r^2 + \left(\frac{dr}{d\theta}\right)^2}} & q &= \frac{x dx + y dy}{\sqrt{(dx)^2 + (dy)^2}} \\ R &= \frac{\left(r^2 + \left(\frac{dr}{d\theta}\right)^2\right)^{\frac{3}{2}}}{r^2 + 2\left(\frac{dr}{d\theta}\right)^2 - r \frac{d^2r}{d\theta^2}} & R &= \frac{\left(1 + \left(\frac{dy}{dx}\right)^2\right)^{\frac{3}{2}}}{\frac{d^2y}{dx^2}} \end{aligned}$$

We come now to consider the three equations of rolling which are involved in the enunciation. Since the second curve rolls upon the first *without slipping*, the length of the fixed curve at the point of contact is the measure of the length of the rolled curve, therefore we have the following equation to connect the fixed curve and the rolled curve,—

$$s_1 = s_2$$

Now, by combining this equation with the two equations

$$\left\{ \begin{array}{l} \theta_1 = \varphi_1(r_1) \\ \theta_2 = \varphi_2(r_2) \end{array} \right\} \text{ or } \left\{ \begin{array}{l} x_1 = \psi_1(y_1) \\ x_2 = \psi_2(y_2) \end{array} \right\},$$

it is evident that from any of the four quantities θ_1 r_1 θ_2 r_2 or x_1 y_1 x_2 y_2 , we can obtain the other three, therefore we may consider these quantities as known functions of each other.

Since the curve *rolls* on the fixed curve, they must have a common tangent.

Let PA be this tangent, draw BP, CQ perpendicular to PA, produce CQ, and draw BR perpendicular to it, then we have CA= r_1 , BA= r_2 , and CB= r_3 ; CQ= p_1 , PB= p_2 , and BN= p_3 ; AQ= q_1 , AP= q_2 and CN= q_3 .

Also,

$$\begin{aligned}
 r_3^2 &= CB^2 = CR^2 + RB^2 = (CQ + PB)^2 + (AP - AQ)^2 \\
 &= (p_1 + p_2)^2 + (q_2 - q_1)^2 \\
 &= p_1^2 + 2p_1p_2 + p_2^2 + r_2^2 - p_2^2 - 2q_1q_2 + r_1^2 - p_1^2 \\
 r_3^2 &= r_1^2 + r_2^2 + 2p_1p_2 - 2q_1q_2
 \end{aligned}$$

Since the first curve is fixed to the paper, we may find the angle θ_3

Thus $\theta_3 = DCB = DCA + ACQ + RCB$

$$= \theta_1 + \tan^{-1} \frac{q_1}{p_1} + \tan^{-1} \frac{RB}{RC}$$

$$\theta_3 = \theta_1 + \tan^{-1} \frac{dr_1}{r_1 d\theta_1} + \tan^{-1} \frac{q_2 - q_1}{p_2 + p_1}$$

Thus we have found three independent equations, which, together with the equations of the curves, make up six equations, of which each may be deduced from the others. There is an equation connecting the radii of curvature of the three curves which is sometimes of use.

The angle through which the rolled curve revolves during the description of the element ds_3 , is equal to the angle of contact of the fixed curve and the rolling curve, or to the sum of their curvatures,

$$\therefore \frac{ds_3}{r_2} = \frac{ds_1}{R_1} + \frac{ds_2}{R_2}$$

But the radius of the rolled curve has revolved in the opposite direction through an angle equal to $d\theta_2$, therefore the angle between two successive positions of r_2 is equal to $\frac{ds_3}{r_2} - d\theta_2$. Now this angle is the angle between two successive positions of the normal to the traced curve, therefore, if O be the centre of curvature of the traced curve, it is the angle which ds_3 or ds_1 subtends at O. Let OA=T, then

$$\begin{aligned}
 \frac{ds_3}{R_3} &= \frac{r_2 d\theta_2}{T} = \frac{ds_3}{r_2} - d\theta_2 = \frac{ds_2}{R_1} + \frac{ds_2}{R_2} - d\theta_2 \\
 \therefore r_2 \frac{d\theta_2}{ds_2} \frac{1}{T} &= \frac{1}{R_1} + \frac{1}{R_2} - r_2 \left(\frac{d\theta_2}{ds_2} \right)^2 \\
 \therefore \frac{p_2}{r_2} \left(\frac{1}{T} + \frac{1}{r_2} \right) &= \frac{1}{R_1} + \frac{1}{R_2}
 \end{aligned}$$

As an example of the use of this equation, we may examine a property of the logarithmic spiral.

In this curve, $p = m r$, and $R = \frac{r}{m}$, therefore if the rolled curve be the logarithmic spiral

$$m \left(\frac{1}{T} + \frac{1}{r_2} \right) = \frac{1}{R_1} + \frac{m}{r_2}$$

$$\frac{m}{T} = \frac{1}{R_1}$$

therefore AO in the figure = $m R_1$, and $\frac{AO}{R_1} = m$.

Let the locus of O, or the evolute of the traced curve LYBH, be the curve OZY, and let the evolute of the fixed curve KZAS be FEZ, and let us consider FEZ as the fixed curve, and OZY as the traced curve.

Then in the triangles BPA, AOF, we have OAF = PBA, and $\frac{OA}{AF} = m = \frac{BP}{AB}$,

therefore the triangles are similar, and FOA = APB = $\frac{\pi}{2}$, therefore OF is perpendicular to OA, the tangent to the curve OZY, therefore OF is the radius of the curve which when rolled on FEZ traces OZY, and the angle which the curve makes with this radius is OFA = PAB = $\sin^{-1} m$, which is constant, therefore the curve, which, when rolled on FEZ, traces OZY, is the logarithmic spiral. Thus we have proved the following proposition: "The involute of the curve traced by the pole of a logarithmic spiral which rolls upon any curve, is the curve traced by the pole of the same logarithmic spiral when rolled on the involute of the primary curve."

It follows from this, that if we roll on any curve a curve having the property $p_1 = m_1 r_1$, and roll another curve having $p_2 = m_2 r_2$ on the curve traced, and so on, it is immaterial in what order we roll these curves. Thus, if we roll a logarithmic spiral, in which $p = m r$, on the n th involute of a circle whose radius is a , the curve traced is the $n + 1$ th involute of a circle whose radius is $\sqrt{1 - m^2}$.

Or, if we roll successively m logarithmic spirals, the resulting curve is the $n + m$ th involute of a circle, whose radius is

$$a \sqrt{1 - m_1^2} \sqrt{1 - m_2^2} \sqrt{\text{etc.}}$$

We now proceed to the cases in which the solution of the problem may be simplified. This simplification is generally effected by the consideration that the radius vector of the rolled curve is the normal drawn from the traced curve to the fixed curve.

In the case in which the curve is rolled on a straight line, the perpendicular on the tangent of the rolled curve is the distance of the tracing point from the straight line; therefore, if the traced curve be defined by an equation in x_3 and y_3 ,

$$x_3 = p_2 = \frac{r_2^2}{\sqrt{r^2 + \left(\frac{dr}{d\theta}\right)^2}} \quad \dots \quad (1.)$$

and

$$r_2 = x \sqrt{\left(\frac{dx}{dy}\right)^2 + 1} \quad . \quad . \quad . \quad (2.)$$

By substituting for r_2 in the first equation, its value, as derived from the second, we obtain

$$x_3^2 \left(\frac{dx_3}{dy_3}\right)^2 \left[\left(\frac{dx_3}{dy_3}\right)^2 + 1 \right] = \left(\frac{dr_2}{d\theta_2}\right)^2$$

If we know the equation to the rolled curve, we may find $\left(\frac{dr_2}{d\theta_2}\right)^2$ in terms of r_2 , then by substituting for r_2 its value in the second equation, we have an equation containing x_3 and $\frac{dx_3}{dy_3}$, from which we find the value of $\frac{dx_3}{dy_3}$ in terms of x_3 , the integration of this gives the equation of the traced curve.

As an example, we may find the curve traced by the pole of a hyperbolic spiral which rolls on a straight line.

The equation of the rolled curve is $\theta_2 = \frac{a}{r_2}$

$$\therefore \left(\frac{dr_2}{d\theta_2}\right)^2 = \frac{r_2^4}{a^2}$$

$$= x_3^2 \left(\frac{dx_3}{dy_3}\right)^2 \left[\left(\frac{dx_3}{dy_3}\right)^2 + 1 \right] = \frac{x_3^4}{a^2} \left[\left(\frac{dx_3}{dy_3}\right)^2 + 1 \right]^2$$

$$\therefore a^2 \left(\frac{dx_3}{dy_3}\right)^2 = x_3^2 \left[\left(\frac{dx_3}{dy_3}\right)^2 + 1 \right]$$

$$\therefore \frac{dx_3}{dy_3} = \frac{x_3}{\sqrt{a^2 - x_3^2}}$$

This is the differential equation of the tractory of the straight line, which is the curve traced by the pole of the hyperbolic spiral.

By eliminating x_3 in the two equations, we obtain

$$\frac{dr_2}{d\theta_2} = r_2 \left(\frac{dx_3}{dy_3}\right)$$

This equation serves to determine the rolled curve when the traced curve is given.

As an example we shall find the curve, which being rolled on a straight line, traces a common catenary.

Let the equation to the catenary be

$$x = \frac{a}{2} \left(e^{\frac{y}{a}} + e^{-\frac{y}{a}} \right)$$

Then

$$\frac{dx_3}{dy_3} = \sqrt{\frac{x_3^2}{a^2} - 1}$$

$$\begin{aligned}
\therefore \left(\frac{d r_2}{d \theta_2} \right)^2 &= \frac{r_2^2}{a^2} \frac{r^4}{\left(\frac{d r_2}{d \theta_2} \right)^2} - r_2^2 \\
\therefore \left[\left(\frac{d r_2}{d \theta_2} \right)^2 + r_2^2 \right] &= \left(\frac{r^2}{a} \right)^2 \\
\therefore \left(\frac{d r_2}{d \theta_2} \right)^2 &= \frac{r^2}{a} (r - a) \\
\therefore \frac{d \theta}{d r} &= \frac{1}{r \sqrt{\frac{r}{a} - 1}} \quad \text{then by integration} \\
\theta &= \cos^{-1} \left(\frac{2a}{r} - 1 \right) \\
r &= \frac{2a}{1 + \cos \theta}
\end{aligned}$$

This is the polar equation of the parabola, the focus being the pole, therefore, if we roll a parabola on a straight line, its focus will trace a catenary.

The rectangular equation of this parabola is $x^2 = 4ay$, and we shall now consider what curve must be rolled along the axis of y to trace the parabola.

By the second equation (2.),

$$\begin{aligned}
r_2 &= x_3 \sqrt{\frac{4a^2}{x_3^2} + 1} \quad \text{but } x_3 = p_2 \\
\therefore r_2 &= \sqrt{4a^2 + p_2^2} \\
\therefore r_2^2 &= p_2^2 + 4a^2 \\
\therefore 2a &= \sqrt{r_2^2 - p_2^2} = q_2
\end{aligned}$$

but q_2 is the perpendicular on the normal, therefore the normal to the curve always touches a circle whose radius is Qa , therefore the curve is the involute of this circle.

Therefore we have the following method of describing a catenary by continued motion.

Describe a circle whose radius is twice the parameter of the catenary; roll a straight line on this circle, then any point in the line will describe an involute of the circle; roll this curve on a straight line, and the centre of the circle will describe a parabola; roll this parabola on a straight line, and its focus will trace the catenary required.

We come now to the case in which a straight line rolls on a curve.

When the tracing-point is in the straight line, the problem becomes that of involutes and evolutes, which we need not enter upon, and when the tracing-point is

not in the straight line, the calculation is somewhat complex, we shall therefore consider only the relations between the curves described in the first and second cases.

Definition.—The curve which cuts at a given angle all the circles of a given radius whose centres are in a given curve, is called a tractory of the given curve.

Let a straight line roll on a curve A, and let a point in the straight line describe a curve B, and let another point, whose distance from the first point is b , and from the straight line a , describe a curve C, then it is evident that the curve B cuts the circle whose centre is in C, and whose radius is b , at an angle whose sine is equal to $\frac{a}{b}$, therefore the curve B is a tractory of the curve C.

When $a = b$, the curve B is the orthogonal tractory of the curve C. If tangents equal to a be drawn to the curve B, they will be terminated in the curve C; and if one end of a thread be carried along the curve C, the other end will trace the curve B.

When $a = 0$, the curves B and C are both involutes of the curve A, they are always equidistant from each other, and if a circle, whose radius is b , be rolled on the one, its centre will trace the other.

If the curve A is such that, if the distance between two points measured along the curve is equal to b , the two points are similarly situate, then the curve B is the same with the curve C. Thus, the curve A may be a re-entrant curve, the circumference of which is equal to b .

When the curve A is a circle, the curves B and C are always the same.

The equations between the radii of curvature become

$$\frac{1}{T} + \frac{1}{r_2} = \frac{r}{a R_1}$$

When $a = 0$, $T = 0$, or the centre of curvature of the curve B is at the point of contact. Now, the normal to the curve C passes through this point, therefore—

“The normal to any curve passes through the centre of curvature of its tractory.”

In the next case, one curve, by rolling on another, produces a straight line. Let this straight line be the axis of y , then, since the radius of the rolled curve is perpendicular to it, and terminates in the fixed curve, and since these curves have a common tangent, we have these equations,

$$r_2 = x_1 \quad \frac{d y_1}{d x_1} = r_2 \frac{d \theta_2}{d r_2}$$

If the equation of the rolled curve be given, find $\frac{d \theta_2}{d r_2}$ in terms of r_2 , substitute

x_1 for r_2 , and multiply by x_1 , equate the result to $\frac{dy}{dx}$, and integrate.

Thus, if the equation of the rolled curve be

$$\theta = A r^{-n} + \text{etc.} + K r^{-2} + L r^{-1} + M \log r + N r + \text{etc.} + Z r^n$$

$$\frac{d\theta}{dr} = -n A r^{-(n+1)} - \text{etc.} - 2K r^{-3} - L r^{-2} + M r^{-1} + N + \text{etc.} + n Z r^{n-1}$$

$$\frac{dy}{dx} = -n A x^{-n} - \text{etc.} - 2K x^{-2} - L x^{-1} + M + N x + \text{etc.} + n Z x^n$$

$$y = \frac{n}{n-1} A x^{1-n} + \text{etc.} + 2K x^{-1} - L \log x + M x + \frac{1}{2} N x^2 + \text{etc.} + \frac{n}{n+1} Z x^{n+1}$$

which is the equation of the fixed curve.

If the equation of the fixed curve be given, find $\frac{dy}{dx}$ in terms of x , substitute r for x , and divide by r , equate the result to $\frac{d\theta}{dr}$, and integrate.

Thus, if the fixed curve be the orthogonal tractory of the straight line, whose equation is

$$y = a \log \frac{x}{a + \sqrt{a^2 - x^2}} + \sqrt{a^2 - x^2}$$

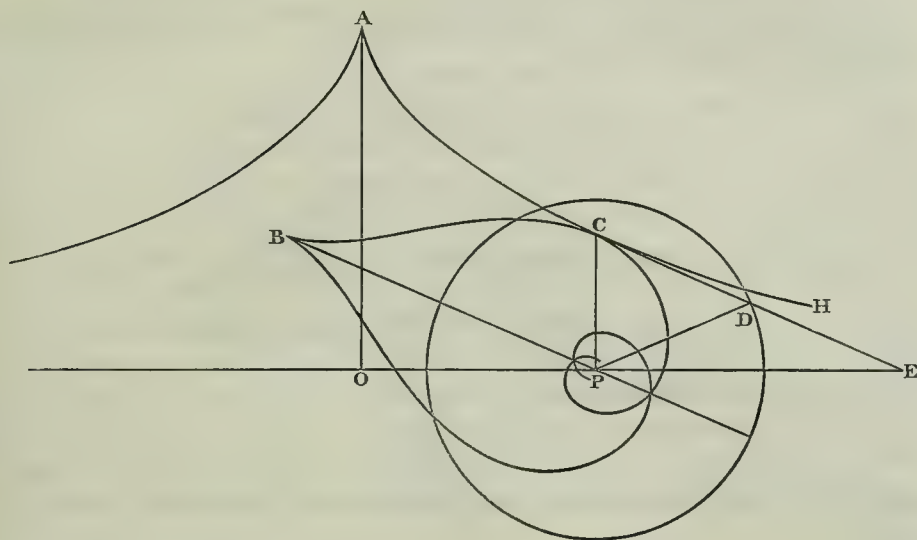
$$\frac{dy}{dx} = \frac{\sqrt{a^2 - x^2}}{x}$$

$$\frac{d\theta}{dr} = \frac{\sqrt{a^2 - r^2}}{r^2}$$

$$\theta = \cos^{-1} \frac{r}{a} - \sqrt{\frac{a^2}{r^2} - 1}$$

this is the equation to the orthogonal tractory of a circle whose diameter is equal to the constant tangent of the fixed curve, and its constant tangent equal to half that of the fixed curve.

This property of the tractory of the circle may be proved geometrically, thus—Let P be the centre of a circle whose radius is PD, and let CD be a line constantly equal to the radius. Let BCP be the curve described by the point C when the point D is moved along the circumference of the circle, then if tangents equal to CD be drawn to the curve, their extremities will be in the circle. Let ACH be the curve on which BCP rolls, and let OPE be the straight line traced by the pole, let CDE be the common tangent, let it cut the circle in D, and the straight line in E.



Then $CD = PD \therefore \angle DCP = \angle DPC$, and CP is perpendicular to OE ,
 $\therefore \angle CPE = \angle DCP + \angle DEP$. Take away $\angle DCP = \angle DPC$, and there remains
 $\angle DPE = \angle DEP \therefore PD = DE \therefore CE = 2 PD$.

Therefore the curve ACH has a constant tangent equal to the diameter of the circle, therefore ACH is the orthogonal tractory of the straight line, which is the tractrix or equitangential curve.

The operation of finding the fixed curve from the rolled curve is what Sir JOHN LESLIE calls “divesting a curve of its radiated structure.”

The method of finding the curve which must be rolled on a circle to trace a given curve is mentioned here because it generally leads to a double result, for the normal to the traced curve cuts the circle in two points, either of which may be a point in the rolled curve.

Thus, if the traced curve be the involute of a circle concentric with the given circle, the rolled curve is one of two similar logarithmic spirals.

If the line traced be a tangent to the circle, the rolled curve is either of the parts of the polar catenary.

If the curve traced be the spiral of ARCHIMEDES, the rolled curve may be either the hyperbolic spiral or the straight line.

In the next case, one curve rolls on another and traces a circle.

Since the curve traced is a circle, the distance between the poles of the fixed curve and the rolled curve is always the same ; therefore, if we fix the rolled curve and roll the fixed curve, the curve traced will still be a circle, and, if we fix the poles of both the curves, we may roll them on each other without friction.

Let a be the radius of the traced circle, then the sum or difference of the radii

of the other curves is equal to a , and the angles which they make with the radius at the point of contact are equal,

$$\therefore r_1 = \pm(a \pm r_2) \text{ and } r_1 \frac{d\theta_1}{dr_1} = r_2 \frac{d\theta_2}{dr_2}$$

$$\therefore \frac{d\theta_2}{dr_2} = \frac{\pm(a \pm r_2)}{r_2} \frac{d\theta_1}{dr_1}$$

If we know the equation between θ_1 and r_1 , we may find $\frac{d\theta_1}{dr_1}$ in terms of r_1 , substitute $\pm(a \pm r_2)$ for r_1 , multiply by $\frac{\pm(a \pm r_2)}{r_2}$, and integrate.

Thus, if the equation between θ_1 and r_1 be

$$r_1 = a \sec \theta_1$$

which is the polar equation of a straight line touching the traced circle whose equation is $r = a$,

then

$$\begin{aligned} \frac{d\theta}{dr_1} &= \frac{a}{r_1 \sqrt{r_1^2 - a^2}} \\ &= \frac{a}{(r_2 \pm a) \sqrt{r_2^2 \pm 2r_2 a}} \\ \frac{d\theta_2}{dr_2} &= \frac{r_2 \pm a}{r_2} \frac{a}{(r_2 \pm a) \sqrt{r_2^2 \pm 2r_2 a}} \\ &= \frac{a}{r_2 \sqrt{r_2^2 \pm 2a r_2}} \\ \theta_2 &= \pm \sqrt{1 \pm 2 \frac{a}{r}} \\ r_2 &= \frac{2a}{\theta_2^2 - 1} = \frac{2a}{\theta^2 - 1} \end{aligned}$$

Now, since the rolling curve is a straight line, and the tracing point is not in its direction, we may apply to this example the observations which have been made upon tractories.

Let, therefore, the curve $r = \frac{2a}{\theta^2 - 1}$ be denoted by A, its involute by B, and the circle traced by C, then B is the tractory of C; therefore the involute of the curve $r = \frac{2a}{\theta^2 - 1}$ is the tractory of the circle, the equation of which is $\theta = \cos^{-1} \frac{r}{a} - \sqrt{\frac{a^2}{r^2} - 1}$. The curve whose equation is $r = \frac{2a}{\theta^2 - 1}$ seems to be among spirals what the catenary is among curves whose equations are between rectangular co-ordinates; for, if we represent the vertical direction by the radius

vector, the tangent of the angle which the curve makes with this line is proportional to the length of the curve reckoned from the origin; the point at the distance a from a straight line rolled on this curve generates a circle, and when rolled on the catenary produces a straight line; the involute of this curve is the tractory of the circle, and that of the catenary is the tractory of the straight line, and the tractory of the circle rolled on that of the straight line traces the straight line; if this curve is rolled on the catenary, it produces the straight line touching the catenary at its vertex; the method of drawing tangents is the same as in the catenary, namely, by describing a circle whose radius is a on the production of the radius vector, and drawing a tangent to the circle from the given point.

In the next case, the rolled curve is the same as the fixed curve. It is evident that the traced curve will be similar to the locus of the intersection of the tangent with the perpendicular from the pole; the magnitude, however, of the traced curve will be double that of the other curve; therefore, if we call $r_0 = \phi_0 \theta_0$ the equation to the fixed curve, $r_1 = \phi_1 \theta_1$ that of the traced curve, we have,

$$r_1 = 2p_0 \quad \theta_1 = \theta_0 - \cos^{-1} \frac{p_0}{r_0} = \theta_0 - \frac{\pi}{2} + \sin^{-1} \frac{p_0}{r_0}$$

$$\text{also, } \frac{p_1}{r_1} = \frac{p_0}{r_0}$$

$$\text{Similarly, } r_2 = 2p_1 = 2r_1 \frac{p_0}{r_0} = 4 \frac{p_0^2}{r_0} \frac{1}{r_0} \left(\frac{p_0}{r_0} \right)^2, \quad \theta^2 = \theta_0 - 2 \cos^{-1} \frac{p_0}{r_0}$$

$$\text{Similarly, } r_n = 2p_{n-1} = 2r_{n-1} \frac{p_0}{r_0} \text{ etc.} = 2^n r_0 \left(\frac{p_0}{r_0} \right)^n$$

$$\text{and } \frac{p_n}{r_n} = \frac{p_0}{r_0}$$

$$\theta_n = \theta_0 - n \cos^{-1} \frac{p_0}{r_0}$$

$$\theta_n = \theta_0 - n \cos^{-1} \frac{p_n}{r_n}$$

$$\text{Let } \theta_n \text{ become } \theta_n^1; \theta_0, \theta_0^1 \text{ and } \frac{p_0}{r_0}, \frac{p_0^1}{r_0^1}. \quad \text{Let } \theta_n^1 - \theta_n = \alpha$$

$$\left| \quad \theta_n^1 = \theta_0^1 - n \cos^{-1} \frac{p^1}{r_n^1} \right.$$

$$\alpha = \theta_n^1 - \theta_n = \theta_0^1 - \theta_0 - n \cos^{-1} \frac{p_n^1}{r_n^1} + n \cos^{-1} \frac{p_n}{r_n}$$

$$\therefore \cos^{-1} \frac{p_n}{r_n} - \cos^{-1} \frac{p_n^1}{r_n^1} = \frac{\alpha}{n} + \frac{\theta_0 - \theta_0^1}{n}$$

Now, $\cos^{-1} \frac{p_n}{r_n}$ is the complement of the angle at which the curve cuts the radius

vector, and $\cos^{-1} \frac{\rho_n}{r_n} - \cos^{-1} \frac{\rho_1}{r_1}$ is the variation of this angle when θ_n varies by an angle equal to α . Let this variation $= \phi$; then if $\theta_0 - \theta_1 = \beta$

$$\phi = \frac{\alpha}{n} + \frac{\beta}{n}$$

Now, if n increases, ϕ will diminish; and if n become infinite,

$$\phi = \frac{\alpha}{\infty} + \frac{\beta}{\infty} = 0 \text{ when } \alpha \text{ and } \beta \text{ are finite.}$$

Therefore, when n is infinite, ϕ vanishes; therefore, the curve cuts the radius vector at a constant angle; therefore the curve is the logarithmic spiral.

Therefore, if any curve be rolled on itself, and the operation repeated an infinite number of times, the resulting curve is the logarithmic spiral.

Hence we may find, analytically, the curve which, being rolled on itself, traces itself.

For the curve which has this property, if rolled on itself, and the operation repeated an infinite number of times, will still trace itself.

But, by this proposition, the resulting curve is the logarithmic spiral; therefore the curve required is the logarithmic spiral. As an example of a curve rolling on itself, we will take the curve whose equation is

$$r_0 = 2^n a \left(\cos \frac{\theta_0}{n} \right)^n$$

$$\text{Here } -\frac{dr_0}{d\theta_0} = 2^n a \left(\sin \frac{\theta_0}{n} \right) \left(\cos \frac{\theta_0}{n} \right)^{n-1}$$

$$\therefore r_1 = 2 \rho_0 = 2 \frac{2^{2n} a^2 \left(\cos \frac{\theta_0}{n} \right)^{2n}}{\sqrt{2^{2n} a^2 \left(\cos \frac{\theta_0}{n} \right)^{2n} + 2^{2n} a^2 \left(\sin \frac{\theta_0}{n} \right)^2 \left(\cos \frac{\theta_0}{n} \right)^{2n-2}}}$$

$$r_1 = 2 \frac{2^n a \left(\cos \frac{\theta_0}{n} \right)^{n+1}}{\sqrt{\left(\cos \frac{\theta_0}{n} \right)^2 + \left(\sin \frac{\theta_0}{n} \right)^2}} = 2^{n+1} a \left(\cos \frac{\theta_0}{n} \right)^{n+1}$$

$$\text{Now } \theta_1 - \theta_0 = -\cos^{-1} \frac{\rho_0}{r_0} = -\cos^{-1} \cos \frac{\theta_0}{n} = \frac{\theta_0}{n}$$

$$\therefore \theta_0 = \theta_1 \frac{n}{n+1}$$

substituting this value of θ_0 in the expression for r_1

$$r_1 = 2^{n+1} a \left(\cos \frac{\theta_1}{n+1} \right)^{n+1}$$

similarly if the operation be repeated m times, the resulting curve is

$$r_m = 2^{n+m} a \left(\cos \frac{\theta_m}{n+m} \right)^{n+m}$$

When $n = 1$, the curve is

$$r = 2 a \cos \theta$$

the equation to a circle, the pole being in the circumference.

When $n = 2$, it is the equation to the cardioid.

$$r = 4 a \left(\cos \frac{\theta}{2} \right)^2$$

In order to obtain the cardioid from the circle, we roll the circle upon itself, and thus obtain it by one operation; but there is an operation which, being performed on a circle, and again on the resulting curve, will produce a cardioid, and the intermediate curve between the circle and cardioid is

$$r = 2^{\frac{3}{2}} a \left(\cos \frac{2\theta}{3} \right)^{\frac{3}{2}}$$

As the operation of rolling a curve on itself is represented by changing n into $n + 1$ in the equation, so this operation may be represented by changing n into $n + \frac{1}{2}$.

Similarly there may be many other fractional operations performed upon the curves comprehended under the equation

$$r = 2^n a \left(\cos \frac{\theta}{n} \right)^n$$

We may also find the curve, which, being rolled on itself, will produce a given curve, by making $n = -1$.

We may likewise prove by the same method as before, that the result of performing this inverse operation an infinite number of times is the logarithmic spiral.

As an example of the inverse method, let the traced line be straight, let its equation be

$$r_0 = 2 a \sec \theta_0$$

$$\text{then } \frac{p_{-1}}{r_{-1}} = \frac{p_0}{r_0} = \frac{2a}{r_0} = \frac{2a}{2p_{-1}}$$

$$\therefore p_{-1}^2 = a r_{-1}$$

therefore suppressing the suffix,

$$r^4 + \frac{d}{d\theta} r^2 = a r$$

$$\therefore \left(\frac{dr}{d\theta} \right)^2 = \frac{r^3}{a} - r^2$$

$$\therefore \frac{d\theta}{dn} = \frac{1}{r \sqrt{\frac{r}{a} - 1}}$$

$$\therefore \theta = \cos^{-1} \left(\frac{2}{r} a - 1 \right)$$

$$r = \frac{2a}{1 - \cos \theta}$$

the polar equation of the parabola whose parameter is $4a$.

The last case which we shall here consider, affords the means of constructing two wheels whose centres are fixed, and which shall roll on each other, so that the angle described by the first shall be a given function of the angle described by the second.

$$\text{Let } \theta_2 = \varphi \theta_1 \text{ then } r_1 + r_2 = a, \text{ and } \frac{d\theta_2}{d\theta_1} = \frac{r}{r_2}$$

$$\therefore \frac{d(\varphi \theta_1)}{d\theta_1} = \frac{r_1}{a - r_1}$$

Let us take as an example, the pair of wheels which will represent the angular motion of a comet in a parabola.

$$\text{Here } \theta_2 = \tan \frac{\theta_1}{2} \therefore \frac{d\theta_2}{d\theta_1} = \frac{1}{\cos^2 \frac{\theta_1}{2}} = \frac{r}{a - r_1}$$

$$\therefore \frac{r_1}{a} = \frac{1}{2 + \cos \theta_1}$$

therefore the first wheel is an ellipse, whose major axis is equal to $\frac{4}{3}$ of the distance between the centres of the wheels, and in which the distance between the foci is half the major axis.

Now, since

$$\theta_1 = 2 \tan^{-1} \theta_2 \text{ and } r_1 = a - r_2$$

$$\frac{r}{a} = 1 + \frac{1}{2(2 - \theta^4)}$$

$$\theta^4 = 2 - \frac{1}{\frac{2r}{a} - 2}$$

which is the equation to the wheel which revolves with constant angular velocity.

Before proceeding to give a list of examples of rolling curves, we shall state a theorem which is almost self-evident after what has been shewn previously.

Let there be three curves, A, B, and C. Let the curve A, when rolled on itself, produce the curve B, and when rolled on a straight line let it produce the curve C; then, if the dimensions of C be doubled, and B be rolled on it, it will trace a straight line.

A Collection of Examples of Rolling Curves.

1st. Examples of a curve rolling on a straight line.

Ex. 1. When the rolling curve is a circle whose tracing-point is in the circumference, the curve traced is a cycloid, and when the point is not in the circumference, the cycloid becomes a trochoid.

Ex. 2. When the rolling curve is the involute of the circle whose radius is $2a$, the traced curve is a parabola whose parameter is $4a$.

Ex. 3. When the rolled curve is the parabola whose parameter is $4a$, the traced curve is a catenary whose parameter is a , and whose vertex is distant a from the straight line.

Ex. 4. When the rolled curve is a logarithmic spiral, the pole traces a straight line which cuts the fixed line at the same angle as the spiral cuts the radius vector.

Ex. 5. When the rolled curve is the hyperbolic spiral, the traced curve is the tractory of the straight line.

Ex. 6. When the rolled curve is the polar catenary

$$\theta = \pm \sqrt{1 \pm \frac{2a}{r}}$$

the traced curve is a circle whose radius is a , and which touches the straight line.

Ex. 7. When the equation of the rolled curve is

$$\theta = \log \left(\sqrt{\frac{r^4}{a^4} - 1} + \frac{r^2}{a^2} \right) - \log \left(\sqrt{\frac{a^4}{r^4} + 1} - \frac{a^2}{r^2} \right)$$

the traced curve is the hyperbola whose equation is

$$y^2 = a^2 + x^2$$

2d. In the examples of a straight line rolling on a curve, we shall use the letters A, B, and C to denote the three curves treated of in page 555.

Ex. 1. When the curve A is a circle whose radius is a , then the curve B is the involute of that circle, and the curve C is the spiral of Archimedes, $r = a\theta$.

Ex. 2. When the curve A is a catenary whose equation is

$$x = \frac{a}{2} \left(e^{\frac{y}{a}} + e^{-\frac{y}{a}} \right)$$

the curve B is the tractory of the straight line, whose equation is

$$y = a \log \frac{x}{a + \sqrt{a^2 - x^2}} + \sqrt{a^2 - x^2}$$

and C is a straight line at a distance a from the vertex of the catenary.

Ex. 3. When the curve A is the polar catenary

$$\vartheta = \pm \sqrt{1 \pm \frac{2a}{r}}$$

the curve B is the tractory of the circle

$$\vartheta = \cos^{-1} \frac{r}{a} - \sqrt{\frac{a^2}{r^2} - 1}$$

and the curve C is a circle of which the radius is $\frac{a}{2}$.

3d. Examples of one curve rolling on another, and tracing a straight line.

Ex. 1. The curve whose equation is

$$\vartheta = A r^{-n} + \text{etc.} + K r^{-2} + L r^{-1} + M \log r + N r + \text{etc.} + Z r^r$$

when rolled on the curve whose equation is

$$y = \frac{n}{n-1} A x^{1-n} + \text{etc.} + 2 K x^{-1} - L \log x + M x + \frac{1}{2} N x^2 + \text{etc.} + \frac{n}{n+1} Z x^{n+1}$$

traces the axis of y .

Ex. 2. The circle whose equation is $r = a \cos \theta$ rolled on the circle whose radius is a traces a diameter of the circle.

Ex. 3. The curve whose equation is

$$\vartheta = \sqrt{\frac{2a}{r} - 1} - \text{versin}^{-1} \frac{r}{a}$$

rolled on the circle whose radius is a traces the tangent to the circle.

Ex. 4. If the fixed curve be a parabola whose parameter is $4a$, and if we roll on it the spiral of Archimedes $r = a\theta$, the pole will trace the axis of the parabola.

Ex. 5. If we roll an equal parabola on it, the focus will trace the directrix of the first parabola.

Ex. 6. If we roll on it the curve $r = \frac{a}{4\vartheta^2}$ the pole will trace the tangent at the vertex of the parabola.

Ex. 7. If we roll the curve whose equation is

$$r = a \cos \left(\frac{a}{b} \vartheta \right)$$

on the ellipse whose equation is

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$$

the pole will trace the axis b .

Ex. 8. If we roll the curve whose equation is

$$r = \frac{a}{2} \left(e^{\frac{a\theta}{b}} - e^{-\frac{a\theta}{b}} \right)$$

on the hyperbola whose equation is

$$\frac{y^2}{b^2} - \frac{x^2}{a^2} = 1$$

the pole will trace the axis b .

Ex. 9. If we roll the lituus, whose equation is

$$r^2 = \frac{a^2}{3\theta}$$

on the hyperbola whose equation is

$$x y = a^2$$

the pole will trace the asymptote.

Ex. 10. The cardioid whose equation is

$$r = a (1 + \cos \theta)$$

rolled on the cycloid whose equation is

$$y = a \operatorname{versin}^{-1} \frac{x}{a} + \sqrt{2ax - x^2}$$

traces the base of the cycloid.

Ex. 11. The curve whose equation is

$$\theta = \operatorname{versin}^{-1} \frac{r}{a} + 2 \sqrt{\frac{2a}{r} - 1}$$

rolled on the cycloid traces the tangent at the vertex.

Ex. 12. The straight line whose equation is

$$r = a \sec \theta$$

rolled on a catenary whose parameter is a , traces a line whose distance from the vertex is a .

Ex. 13. The part of the polar catenary whose equation is

$$\theta = \pm \sqrt{1 + \frac{2a}{r}}$$

rolled on the catenary traces the tangent at the vertex.

Ex. 14. The other part of the polar catenary whose equation is

$$\theta = \pm \sqrt{1 - \frac{2a}{r}}$$

rolled on the catenary traces a line whose distance from the vertex is equal to $2a$.

Ex. 15. The tractory of the circle whose diameter is a , rolled on the tractory of the straight line whose constant tangent is a , produces the straight line.

Ex. 16. The hyperbolic spiral whose equation is

$$r = \frac{a}{\theta}$$

rolled on the logarithmic curve whose equation is

$$y = a \log \frac{x}{a}$$

traces the axis of y or the asymptote.

Ex. 17. The involute of the circle whose radius is a , rolled on an orthogonal trajectory of the catenary whose equation is

$$y = \frac{x}{2a} \sqrt{x^2 - a^2} + \frac{a}{2} \log \left(\sqrt{\frac{x^2}{a^2} - 1} + \frac{x}{a} \right)$$

traces the axis of y .

Ex. 18. The curve whose equation is

$$\theta = \left(\frac{a}{x} + 1 \right) \sqrt{2 \frac{a}{x} + 1}$$

rolled on the witch, whose equation is

$$y = 2a \sqrt{\frac{2a}{x} - 1}$$

traces the asymptote.

Ex. 19. The curve whose equation is

$$r = a \tan \theta$$

rolled on the curve whose equation is

$$y = \frac{a}{2} \log \left(\frac{x^2}{a^2} - 1 \right)$$

traces the axis of y .

Ex. 20. The curve whose equation is

$$\theta = \frac{2r}{\sqrt{a^2 - r^2}}$$

rolled on the curve whose equation is

$$y = \frac{x^2}{\sqrt{a^2 - x^2}} \quad \text{or } r = a \tan \theta$$

traces the axis of y .

Ex. 21. The curve whose equation is

$$r = a (\sec \theta - \tan \theta)$$

rolled on the curve whose equation is

$$y = a \log \left(\frac{x^2}{a^2} + 1 \right)$$

traces the axis of y .

4th. Examples of pairs of rolling curves which have their poles at a fixed distance = a

- Ex. 1. $\left\{ \begin{array}{l} \text{The straight line whose equation is} \\ \text{The polar catenary whose equation is} \end{array} \right. \quad \begin{array}{l} \theta = \sec^{-1} \frac{r}{a} \\ \theta = \pm \sqrt{1 \pm \frac{2a}{r}} \end{array}$
- Ex. 2. Two equal ellipses or hyperbolas centred at the foci.
- Ex. 3. Two equal logarithmic spirals.
- Ex. 4. $\left\{ \begin{array}{l} \text{Circle whose equation is} \\ \text{Curve whose equation is} \end{array} \right. \quad \begin{array}{l} r = 2a \cos \theta. \\ \theta = \sqrt{2 \frac{a}{r} - 1} + \text{versin}^{-1} \frac{r}{a} \end{array}$
- Ex. 5. $\left\{ \begin{array}{l} \text{Cardioid whose equation is} \\ \text{Curve whose equation is} \end{array} \right. \quad \begin{array}{l} r = 2a(1 + \cos \theta.) \\ \theta = \sin^{-1} \frac{r}{a} + \log \frac{r}{\sqrt{a^2 - r^2 + a}} \end{array}$
- Ex. 6. $\left\{ \begin{array}{l} \text{Conchoid,} \\ \text{Curve,} \end{array} \right. \quad \begin{array}{l} r = a(\sec \theta - 1.) \\ \theta = \sqrt{1 - \frac{a^2}{r^2}} + \sec^{-1} \frac{r}{a} \end{array}$
- Ex. 7. $\left\{ \begin{array}{l} \text{Spiral of Archimedes,} \\ \text{Curve,} \end{array} \right. \quad \begin{array}{l} r = a \theta \\ \theta = \frac{r}{a} + \log \frac{r}{a} \end{array}$
- Ex. 8. $\left\{ \begin{array}{l} \text{Hyperbolic spiral,} \\ \text{Curve,} \end{array} \right. \quad \begin{array}{l} r = \frac{a}{\theta} \\ r = \frac{a}{e^\theta + 1} \end{array}$
- Ex. 9. $\left\{ \begin{array}{l} \text{Ellipse whose equation is,} \\ \text{Curve,} \end{array} \right. \quad \begin{array}{l} r = a \frac{1}{2 + \cos \theta} \\ r = a \left(1 + \frac{1}{2(2 - \theta^2)} \right) \end{array}$
- Ex. 10. $\left\{ \begin{array}{l} \text{Involute of circle,} \\ \text{Curve,} \end{array} \right. \quad \begin{array}{l} \theta = \sqrt{\frac{r^2}{a^2} - 1} \sec^{-1} \frac{r}{a} \\ \theta = \sqrt{\frac{r^2}{a^2} \pm 2 \frac{r}{a}} \pm \log \left(\frac{r}{a} \pm 1 + \sqrt{\frac{r^2}{a^2} \pm 2 \frac{r}{a}} \right) \end{array}$

5th. Examples of curves rolling on themselves.

Ex. 1. When the curve which rolls on itself is a circle, equation

$$r = a \cos \theta$$

the traced curve is a cardioid, equation $r = a(1 + \cos \theta)$.

Ex. 2. When it is the curve whose equation is

$$r = 2^n a \left(\cos \frac{\theta}{r} \right)^n$$

the equation of the traced curve is

$$r = 2^{n+1} a \left(\cos \frac{\theta}{n+1} \right)^{n+1}$$

Ex. 3. When it is the involute of the circle, the traced curve is the spiral of Archimedes.

Ex. 4. When it is a parabola, the focus traces the directrix, and the vertex traces the cissoid.

Ex. 5. When it is the hyperbolic spiral, the traced curve is the tractory of the circle.

Ex. 6. When it is the polar catenary, the equation of the traced curve is

$$\theta = \sqrt{\frac{2a}{r} - 1} - \text{versin}^{-1} \frac{r}{a}$$

Ex. 7. When it is the curve whose equation is

$$\theta = \log \left(\sqrt{\frac{r^4}{a^4} - 1} + \frac{r^2}{a^2} \right) - \log \left(\sqrt{1 + \frac{a^4}{r^4} - \frac{a^2}{r^2}} \right)$$

the equation of the traced curve is, $r = a (e^{\theta} - e^{-\theta})$.

XXXVI.—*An Account of CARNOT'S Theory of the Motive Power of Heat*;* with *Numerical Results deduced from REGNAULT'S Experiments on Steam*.† By WILLIAM THOMSON, Professor of Natural Philosophy in the University of Glasgow.

(Read January 2, 1849.)

1. The presence of heat may be recognised in every natural object; and there is scarcely an operation in nature which is not more or less affected by its all-pervading influence. An evolution and subsequent absorption of heat generally give rise to a variety of effects; among which may be enumerated, chemical combinations or decompositions; the fusion of solid substances; the vaporisation of solids or liquids; alterations in the dimensions of bodies, or in the statical pressure by which their dimensions may be modified; mechanical resistance overcome; electrical currents generated. In many of the actual phenomena of nature, several or all of these effects are produced together; and their complication will, if we attempt to trace the agency of heat in producing any individual effect, give rise to much perplexity. It will, therefore, be desirable, in laying the foundation of a physical theory of any of the effects of heat, to discover or to imagine phenomena free from all such complication, and depending on a definite thermal agency; in which the relation between the cause and effect, traced through the medium of certain simple operations, may be clearly appreciated. Thus it is that CARNOT, in accordance with the strictest principles of philosophy, enters upon the investigation of the theory of the motive power of heat.

2. The sole effect to be contemplated in investigating the motive power of heat is *resistance overcome*, or, as it is frequently called, "*work performed*," or "*mechanical effect*." The questions to be resolved by a complete theory of the subject are the following:

(1.) What is the precise nature of the thermal agency by means of which *mechanical effect* is to be produced, without effects of any other kind?

* Published in 1824, in a work entitled, "Réflexions sur la Puissance Motrice du Feu, et sur les Machines Propres à Développer cette Puissance. Par S. CARNOT." An account of CARNOT'S Theory is also published in the *Journal d'Ecole Polytechnique*, vol. xiv., 1834, in a paper by Mons. CLAPEYRON.

† An account of the first part of a series of researches undertaken by Mons. REGNAULT, by order of the late French Government, for ascertaining the various physical data of importance in the theory of the steam-engine, has been recently published (under the title, "Relation des Expériences," &c.) in the *Mémoires de l'Institut*, of which it constitutes the twenty-first volume (1847). The second part of these researches has not yet been published.

(2.) How may the amount of this thermal agency necessary for performing a given quantity of work be estimated?

3. In the following paper I shall commence by giving a short abstract of the reasoning by which CARNOT is led to an answer to the first of these questions: I shall then explain the investigation by which, in accordance with his theory, the experimental elements necessary for answering the second question are indicated; and, in conclusion, I shall state the *data* supplied by REGNAULT'S recent observations on steam, and apply them to obtain, as approximately as the present state of experimental science enables us to do, a complete solution of the question.

I. On the nature of Thermal agency, considered as a motive power.

4. There are [at present known] two, and only two, distinct ways in which mechanical effect can be obtained from heat. One of these is by means of the alterations of volume which bodies may experience through the action of heat: the other is through the medium of electric agency. SEEBECK'S discovery of thermo-electric currents enables us at present to conceive of an electro-magnetic engine supplied from a thermal origin, being used as a motive power: but this discovery was not made until 1821, and the subject of thermo-electricity can only have been generally known in a few isolated facts, with reference to the electrical effects of heat upon certain crystals, at the time when CARNOT wrote. He makes no allusion to it, but confines himself to the method for rendering thermal agency available as a source of mechanical effect, by means of the expansions and contractions of bodies.

5. A body expanding or contracting under the action of force, may, in general, either produce mechanical effect by overcoming resistance, or receive mechanical effect by yielding to the action of force. The amount of mechanical effect thus developed will depend not only on the calorific agency concerned, but also on the alteration in the physical condition of the body. Hence, after allowing the volume and temperature of the body to change, we must restore it to its original temperature and volume; and then we may estimate the aggregate amount of mechanical effect developed as due solely to the thermal origin.

6. Now the ordinarily-received, and almost universally-acknowledged, principles with reference to "quantities of caloric" and "latent heat," lead us to conceive that, at the end of a cycle of operations, when a body is left in precisely its primitive physical condition, if it has absorbed any heat during one part of the operations, it must have given out again exactly the same amount during the remainder of the cycle. The truth of this principle is considered as axiomatic by CARNOT, who admits it as the foundation of his theory; and expresses himself in the following terms regarding it, in a note on one of the passages of his treatise.*

* CARNOT, p. 37.

“ In our demonstrations we tacitly assume that after a body has experienced a certain number of transformations, if it be brought identically to its primitive physical state as to density, temperature, and molecular constitution, it must contain the same quantity of heat as that which it initially possessed; or, in other words, we suppose that the quantities of heat lost by the body under one set of operations are precisely compensated by those which are absorbed in the others. This fact has never been doubted; it has at first been admitted without reflection, and afterwards verified, in many cases, by calorimetrical experiments. To deny it would be to overturn the whole theory of heat, in which it is the fundamental principle. It must be admitted, however, that the chief foundations on which the theory of heat rests, would require a most attentive examination. Several experimental facts appear nearly inexplicable in the actual state of this theory.”

7. Since the time when CARNOT thus expressed himself, the necessity of a most careful examination of the entire experimental basis of the theory of heat has become more and more urgent. Especially all those assumptions depending on the idea that heat is a *substance*, invariable in quantity; not convertible into any other element, and incapable of being *generated* by any physical agency; in fact the acknowledged principles of latent heat; would require to be tested by a most searching investigation before they ought to be admitted, as they usually have been, by almost every one who has been engaged on the subject, whether in combining the results of experimental research, or in general theoretical investigations.

8. The extremely important discoveries recently made by Mr JOULE of Manchester, that heat is evolved in every part of a closed electric conductor, moving in the neighbourhood of a magnet,* and that heat is *generated* by the friction of fluids in motion, seem to overturn the opinion commonly held that heat cannot be *generated*, but only produced from a source, where it has previously existed either in a sensible or in a latent condition.

* The *evolution* of heat in a fixed conductor, through which a galvanic current is sent from any source whatever, has long been known to the scientific world; but it was pointed out by Mr JOULE that we cannot infer from any previously-published experimental researches, the actual *generation* of heat when the current originates in electro-magnetic induction; since the question occurs, *is the heat which is evolved in one part of the closed conductor merely transferred from those parts which are subject to the inducing influence?* Mr JOULE, after a most careful experimental investigation with reference to this question, finds that it must be answered in the negative.—(See a paper “*On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat*”; by J. P. JOULE, Esq.” Read before the British Association at Cork in 1843, and subsequently communicated by the Author to the *Philosophical Magazine*, vol. xxiii., pp. 263, 347, 435.)

Before we can finally conclude that heat is absolutely generated in such operations, it would be necessary to prove that the inducing magnet does not become lower in temperature, and thus compensate for the heat evolved in the conductor. I am not aware that any examination with reference to the truth of this conjecture has been instituted; but, in the case where the inducing body is a pure electro-magnet (without any iron), the experiments actually performed by Mr JOULE render the conclusion probable that the heat evolved in the wire of the electro-magnet is not affected by the inductive action, otherwise than through the reflected influence which increases the strength of its own current.

In the present state of science, however, no operation is known by which heat can be absorbed into a body without either elevating its temperature, or becoming latent, and producing some alteration in its physical condition; and the fundamental axiom adopted by CARNOT may be considered as still the most probable basis for an investigation of the motive power of heat; although this, and with it every other branch of the theory of heat may ultimately require to be reconstructed upon another foundation, when our experimental data are more complete. On this understanding, and to avoid a repetition of doubts, I shall refer to CARNOT'S fundamental principle, in all that follows, as if its truth were thoroughly established.

9. We are now led to the conclusion that the origin of motive power, developed by the alternate expansions and contractions of a body, must be found in the agency of heat entering the body and leaving it; since there cannot, at the end of a complete cycle, when the body is restored to its primitive physical condition, have been any absolute absorption of heat, and consequently no conversion of heat, or caloric, into mechanical effect; and it remains for us to trace the precise nature of the circumstances under which heat must enter the body, and afterwards leave it, so that mechanical effect may be produced. As an example, we may consider that machine for obtaining motive power from heat with which we are most familiar—the steam-engine.

10. Here, we observe, that heat enters the machine from the furnace, through the sides of the boiler, and that heat is continually abstracted by the water employed for keeping the condenser cool. According to CARNOT'S fundamental principle, the quantity of heat thus discharged, during a complete revolution (or double stroke) of the engine must be precisely equal to that which enters the water of the boiler;* provided the total mass of water and steam be invariable, and be restored to its primitive physical condition (which will be the case rigorously, if the condenser be kept cool by the external application of cold water, instead of by injection, as is more usual in practice), and if the condensed water be restored to the boiler at the end of each complete revolution. Thus, we perceive, that a certain quantity of heat is *let down* from a hot body, the metal of the boiler, to another body at a lower temperature, the metal of the condenser; and that there results from this transference of heat, a certain development of mechanical effect.

11. If we examine any other case in which mechanical effect is obtained from a thermal origin, by means of the alternate expansions and contractions of any substance whatever, instead of the water of a steam-engine, we find that a similar transference of heat is effected, and we may therefore answer the first question proposed, in the following manner:—

The thermal agency by which mechanical effect may be obtained, is the transference of heat from one body to another at a lower temperature.

* So generally is CARNOT'S principle tacitly admitted as an axiom, that its application in this case has never, so far as I am aware, been questioned by practical engineers.

II. On the measurement of Thermal Agency, considered with reference to its equivalent of mechanical effect.

12. A *perfect* thermo-dynamic engine of any kind, is a machine by means of which the greatest possible amount of mechanical effect can be obtained from a given thermal agency; and, therefore, if in any manner we can construct or imagine a perfect engine which may be applied for the transference of a given quantity of heat from a body at any given temperature, to another body, at a lower given temperature, and if we can evaluate the mechanical effect thus obtained, we shall be able to answer the question at present under consideration, and so to complete the theory of the motive power of heat. But whatever kind of engine we may consider with this view, it will be necessary for us to prove that it is a perfect engine; since the transference of the heat from one body to the other may be wholly, or partially, effected by conduction through a solid,* without the development of mechanical effect; and, consequently, engines may be constructed in which the whole, or any portion of the thermal agency is wasted. Hence it is of primary importance to discover the criterion of a perfect engine. This has been done by CARNOT, who proves the following proposition:—

13. *A perfect thermo-dynamic engine is such that, whatever amount of mechanical effect it can derive from a certain thermal agency; if an equal amount be spent in working it backwards, an equal reverse thermal effect will be produced.*†

14. This proposition will be made clearer by the applications of it which are given below (§ 29), in the cases of the air-engine and the steam-engine, than it could be by any general explanation; and it will also appear, from the nature of the operations described in those cases, and the principles of CARNOT's reasoning, that a perfect engine may be constructed with any substance of an indestructible texture as the alternately expanding and contracting medium. Thus we might conceive thermo-dynamic engines founded upon the expansions

* When "thermal agency" is thus spent in conducting heat through a solid, what becomes of the mechanical effect which it might produce? Nothing can be lost in the operations of nature—no energy can be destroyed. What effect then is produced in place of the mechanical effect which is lost? A perfect theory of heat imperatively demands an answer to this question; yet no answer can be given in the present state of science. A few years ago, a similar confession must have been made with reference to the mechanical effect lost in a fluid set in motion in the interior of a rigid closed vessel, and allowed to come to rest by its own internal friction; but in this case, the foundation of a solution of the difficulty has been actually found, in Mr JOULE's discovery of the generation of heat, by the internal friction of a fluid in motion. Encouraged by this example, we may hope that the very perplexing question in the theory of heat, by which we are at present arrested, will, before long, be cleared up.

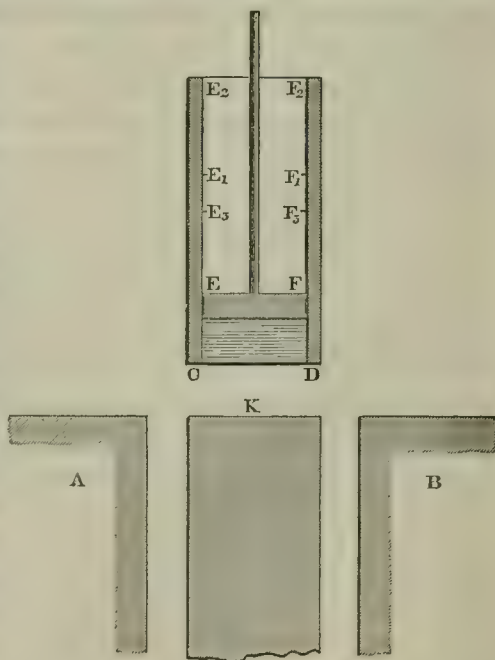
It might appear, that the difficulty would be entirely avoided, by abandoning CARNOT's fundamental axiom; a view which is strongly urged by Mr JOULE (at the conclusion of his paper "On the Changes of Temperature produced by the Rarefaction and Condensation of Air." *Phil. Mag.*, May 1845, vol. xxvi.) If we do so, however, we meet with innumerable other difficulties—insuperable without farther experimental investigation, and an entire reconstruction of the theory of heat, from its foundation. It is in reality to experiment that we must look—either for a verification of CARNOT's axiom, and an explanation of the difficulty we have been considering; or for an entirely new basis of the Theory of Heat.

† For a demonstration, see § 29, below.

and contractions of a perfectly elastic solid, or of a liquid; or upon the alterations of volume experienced by substances, in passing from the liquid to the solid state,* each of which being perfect, would produce the same amount of mechanical effect from a given thermal agency; but there are two cases which CARNOT has selected as most worthy of minute attention, because of their peculiar appropriateness for illustrating the general principles of his theory, no less than on account of their very great practical importance; the steam-engine, in which the substance employed as the transferring medium is water, alternately in the liquid state, and in the state of vapour; and the air-engine, in which the transference is effected by means of the alternate expansions and contractions of a medium, always in the gaseous state. The details of an actually practicable engine of either kind are not contemplated by CARNOT, in his general theoretical reasonings, but he confines himself to the ideal construction, in the simplest possible way in each case, of an engine in which the economy is perfect. He thus determines the degree of perfectibility which cannot be surpassed; and, by describing a conceivable method of attaining to this perfection by an air-engine or a steam-engine, he points out the proper objects to be kept in view in the practical construction and working of such machines. I now proceed to give an outline of these investigations.

CARNOT'S Theory of the Steam-Engine.

15. Let CDF_2E_2 be a cylinder, of which the curved surface is perfectly impermeable to heat, with a piston also impermeable to heat, fitted in it; while the fixed bottom CD , itself with no capacity for heat, is possessed of perfect conducting power. Let K be an impermeable stand, such that when the cylinder is placed upon it, the contents below the piston can neither gain nor lose heat. Let A and B be two bodies permanently retained at constant temperatures, S^0 and T^0 , respectively, of which the former is higher than the latter. Let the cylinder, placed on the impermeable stand, K , be partially filled with water, at the temperature S , of the body A , and (there being no air below it) let the piston be placed in a position $E F$, near the surface of the water. The



* A case minutely examined in another paper, to be laid before the Society at the present meeting.

pressure of the vapour above the water will tend to push up the piston, and must be resisted by a force applied to the piston,* till the commencement of the operations, which are conducted in the following manner.

(1.) The cylinder being placed on the body A, so that the water and vapour may be retained at the temperature S, *let the piston rise any convenient height $E E_1$, to a position $E_1 F_1$, performing work by the pressure of the vapour below it during its ascent.*

[During this operation a certain quantity, H, of heat, the amount of latent heat in the fresh vapour which is formed, is abstracted from the body A.]

(2.) The cylinder being removed, and placed on the impermeable stand K, *let the piston rise gradually, till, when it reaches a position $E_2 F_2$, the temperature of the water and vapour is T, the same as that of the body B.*

[During this operation the fresh vapour continually formed requires heat to become latent; and, therefore, as the contents of the cylinder are protected from any accession of heat, their temperature sinks.]

(3.) The cylinder being removed from K, and placed on B, *let the piston be pushed down, till, when it reaches the position $E_3 F_3$, the quantity of heat evolved and abstracted by B amounts to that which, during the first operation, was taken from A.*

[During this operation the temperature of the contents of the cylinder is retained constantly at T^0 , and all the latent heat of the vapour which is condensed into water at the same temperature, is given out to B.]

(4.) The cylinder being removed from B, and placed on the impermeable stand, *let the piston be pushed down from $E_3 F_3$ to its original position EF.*

[During this operation, the impermeable stand preventing any loss of heat, the temperature of the water and air must rise continually, till (since the quantity of heat evolved during the third operation was precisely equal to that which was previously absorbed), at the conclusion it reaches its primitive value, S, in virtue of Carnot's fundamental axiom.]

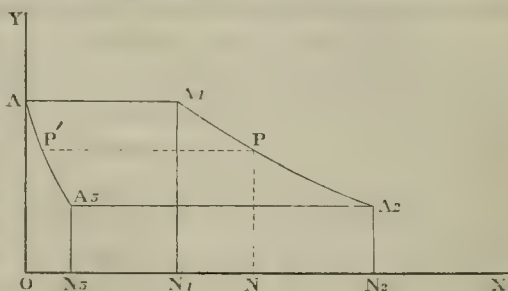
16. At the conclusion of this cycle of operations† the total thermal agency has been the *letting down* of H units of heat from the body A, at the temperature S, to B, at the lower temperature T; and the aggregate of the mechanical effect has been a certain amount of *work produced*, since during the ascent of the piston in the first and second operations, the temperature of the water and vapour, and therefore the pressure of the vapour on the piston, was on the whole higher than during the descent, in the third and fourth operations. It remains for us actually to evaluate this aggregate amount of work performed; and for this purpose the

* In all that follows, the pressure of the atmosphere on the upper side of the piston will be included in the applied forces, which, in the successive operations described, are sometimes overcome by the upward motion, and sometimes yielded to in the motion downwards. It will be unnecessary, in reckoning at the end of a cycle of operations, to take into account the work thus spent upon the atmosphere, and the restitution which has been made, since these precisely compensate for one another.

† In CARNOT'S work some perplexity is introduced with reference to the temperature of the water, which, in the operations he describes, is not brought back exactly to what it was at the commencement; but the difficulty which arises is explained by the author. No such difficulty occurs with reference to the cycle of operations described in the text, for which I am indebted to Mons. CLAPEYRON.

following graphical method of representing the mechanical effect developed in the several operations, taken from Mons. CLAPEYRON'S paper, is extremely convenient.

17. Let $O X$ and $O Y$ be two lines at right angles to one another. Along $O X$ measure off distances $O N_1, N_1 N_2, N_2 N_3, N_3 O$, respectively proportional to the spaces described by the piston during the four successive operations described above; and, with reference to these four operations respectively, let the following constructions be made:—



(1.) Along $O Y$ measure a length $O A$, to represent the pressure of the saturated vapour at the temperature S ; and draw $A A_1$ parallel to $O X$, and let it meet an ordinate through N_1 in A_1 .

(2.) Draw a curve $A_1 P A$ such that, if $O N$ represent, at any instant during the second operation, the distance of the piston from its primitive position, $N P$ shall represent the pressure of the vapour at the same instant.

(3.) Through A_2 draw $A_2 A_3$ parallel to $O X$, and let it meet an ordinate through N_3 in A_3 .

(4.) Draw the curve $A_3 A$ such that the abscissa and ordinate of any point in it may represent respectively the distances of the piston from its primitive position, and the pressure of the vapour, at some instant during the fourth operation. The last point of this curve must, according to Carnot's fundamental principle, coincide with A , since the piston is, at the end of the cycle of operations, again in its primitive position, and the pressure of the vapour is the same as it was at the beginning.

18. Let us now suppose that the lengths, $O N_1, N_1 N_2, N_2 N_3$, and $N_3 O$, represent *numerically* the volumes of the spaces moved through by the piston during the successive operations. It follows that the mechanical effect obtained during the first operation will be *numerically represented* by the area $A A_1 N_1 O$; that is, the number of superficial units in this area will be equal to the number of "foot-pounds" of work performed by the ascending piston during the first operation. The work performed by the piston during the second operation will be similarly represented by the area $A_1 A_2 N_2 N_1$. Again, during the third operation a certain amount of work is spent on the piston, which will be represented by the area $A_2 A_3 N_3 N_2$; and lastly, during the fourth operation, work is spent in pushing the piston to an amount represented by the area $A_3 A O N_3$.

19. Hence the mechanical effect (represented by the area $O A A_1 A_2 N_2$) which was obtained during the first and second operations, exceeds the work (represented by $N_2 A_2 A_3 A O$) spent during the third and fourth, by an amount represented by the area of the quadrilateral figure $A A_1 A_2 A_3$; and, consequently, it

only remains for us to evaluate this area, that may determine the total mechanical effect gained in a complete cycle of operations. Now, from experimental data, at present nearly complete, as will be explained below, we may determine the length of the line $A A_1$ for the given temperature S , and a given absorption H , of heat, during the first operation; and the length of $A_2 A_3$ for the given lower temperature T , and the evolution of the same quantity of heat during the fourth operation: and the curves $A_1 P A_2$, $A_3 P' A$ may be drawn as graphical representations of actual observations.* The figure being thus constructed, its area may be measured, and we are, therefore, in possession of a graphical method of determining the amount of mechanical effect to be obtained from any given thermal agency. As, however, it is merely the area of the figure which it is required to determine, it will not be necessary to be able to describe each of the curves $A_1 P A_2$, $A_3 P' A$, but it will be sufficient to know the difference of the abscissas corresponding to any equal ordinates in the two; and the following analytical method of completing the problem is the most convenient for leading to the actual numerical results.

20. Draw any line $P P'$ parallel to $O X$, meeting the curvilinear sides of the quadrilateral in P and P' . Let ξ denote the length of this line, and p its distance from $O X$. The area of the figure, according to the integral calculus, will be denoted by the expression

$$\int_{p_3}^{p_1} \xi dp,$$

where p_1 , and p_3 (the limits of integration indicated according to FOURIER'S notation) denote the lines $O A$, and $N_3 A_3$, which represent respectively the pressures during the first and third operations. Now, by referring to the construction described above, we see that ξ is the difference of the volumes below the piston at corresponding instants of the second and fourth operations, or instants at which the saturated steam and the water in the cylinder have the same pressure p , and, consequently, the same temperature which we may denote by t . Again, throughout the second operation the entire contents of the cylinder possess a greater amount of heat by H units than during the fourth; and, therefore, at any instant of the second operation there is as much more steam as contains H units of latent heat, than at the corresponding instant of the fourth operation. Hence, if k denote the latent heat in a unit of saturated steam at the temperature t , the volume of the steam at the two corresponding instants must differ by $\frac{H}{k}$. Now, if σ denote the ratio of the density of the steam to that of the water, the volume $\frac{H}{k}$ of steam will be formed from the volume $\sigma \frac{H}{k}$ of water; and, consequently, we have

* See Note at the end of this Paper.

for the difference of volumes of the entire contents at the corresponding instants,

$$\xi = (1 - \sigma) \frac{H}{k}.$$

Hence the expression for the area of the quadrilateral figure becomes

$$\int_{p_3}^{p_1} (1 - \sigma) \frac{H}{k} dp.$$

Now, σ , k , and p , being quantities which depend upon the temperature, may be considered as functions of t ; and it will be convenient to modify the integral so as to make t the independent variable. The limits will be from $t=T$ to $t=S$, and, if we denote by M the value of the integral, we have the expression

$$M = H \int_T^S (1 - \sigma) \frac{dp}{k} dt \quad . \quad . \quad . \quad (1)$$

for the total amount of mechanical effect gained by the operations described above.

21. If the interval of temperatures be extremely small; so small that $(1 - \sigma) \frac{dp}{k}$ will not sensibly vary for values of t between T and S , the preceding expression becomes simply

$$M = (1 - \sigma) \frac{dp}{k} \cdot H (S - T) \quad . \quad . \quad . \quad (2).$$

This might, of course, have been obtained at once, by supposing the breadth of the quadrilateral figure AA_1A_2A to be extremely small compared with its length, and then taking for its area, as an approximate value, the product of the breadth into the line AA_1 , or the line A_2A , or any line of intermediate magnitude.

The expression (2) is rigorously correct for any interval $S-T$, if the mean value of $(1 - \sigma) \frac{dp}{k}$ for that interval be employed as the coefficient of $H(S-T)$.

CARNOT'S *Theory of the Air-Engine.*

22. In the ideal air-engine imagined by CARNOT four operations performed upon a mass of air or gas enclosed in a closed vessel of variable volume, constitute a complete cycle, at the end of which the medium is left in its primitive physical condition; the construction being the same as that which was described above for the steam-engine, a body A , permanently retained at the temperature S , and B at the temperature T ; an impermeable stand K ; and a cylinder and piston, which, in this case, contains a mass of air at the temperature S , instead of

water in the liquid state, at the beginning and end of a cycle of operations. The four successive operations are conducted in the following manner :—

(1.) The cylinder is laid on the body A, so that the air in it is kept at the temperature S; and the piston is allowed to rise, performing work.

(2.) The cylinder is placed on the impermeable stand K, so that its contents can neither gain nor lose heat, and the piston is allowed to rise farther, still performing work, till the temperature of the air sinks to T.

(3.) The cylinder is placed on B, so that the air is retained at the temperature T, and the piston is pushed down till the air gives out to the body B as much heat as it had taken in from A, during the first operation.

(4.) The cylinder is placed on K, so that no more heat can be taken in or given out, and the piston is pushed down to its primitive position.

23. *At the end of the fourth operation the temperature must have reached its primitive value S, in virtue of CARNOT'S axiom.*

24. Here, again, as in the former case, we observe that work is performed by the piston during the first two operations; and, during the third and fourth, work is spent upon it, but to a less amount, since the pressure is on the whole less during the third and fourth operations than during the first and second, on account of the temperature being lower. Thus, at the end of a complete cycle of operations, mechanical effect has been obtained; and the thermal agency from which it is drawn is the taking of a certain quantity of heat from A, and *letting it down*, through the medium of the engine, to the body B at a lower temperature.

25. To estimate the actual amount of effect thus obtained, it will be convenient to consider the alterations of volume of the mass of air in the several operations as extremely small. We may afterwards pass by the integral calculus, or, practically, by summation, to determine the mechanical effect whatever be the amplitudes of the different motions of the piston.

26. Let dq be the quantity of heat absorbed during the first operation, which is evolved again during the third; and let dv be the corresponding augmentation of volume which takes place while the temperature remains constant, as it does during the first operation.* The diminution of volume in the third operation must be also equal to dv , or only differ from it by an infinitely small

* Thus, $\frac{dq}{dv}$ will be the partial differential coefficient, with respect to v of that function of v and t , which expresses the quantity of heat that must be added to a mass of air when in a "standard" state (such as at the temperature zero, and under the atmospheric pressure), to bring it to the temperature t , and the volume v . That there is such a function, of two independent variables v and t , is merely an analytical expression of CARNOT'S fundamental axiom, as applied to a mass of air. The general principle may be analytically stated in the following terms :—If $M dv$ denote the accession of heat received by a mass of any kind, not possessing a destructible texture, when the volume is increased by dv , the temperature being kept constant, and if $N dt$ denote the amount of heat which must be supplied to raise the temperature by dt , without any alteration of volume; then $M dv + N dt$ must be the differential of a function of v and t .

quantity of the second order. During the second operation we may suppose the volume to be increased by an infinitely small quantity ϕ ; which will occasion a diminution of pressure, and a diminution of temperature, denoted respectively by ω and τ . During the fourth operation there will be a diminution of volume, and an increase of pressure and temperature, which can only differ, by infinitely small quantities of the second order, from the changes in the other direction, which took place in the second operation, and they also may, therefore, be denoted by ϕ , ω , and τ , respectively. The alteration of pressure, during the first and third operations, may at once be determined by means of MARIOTTE'S law, since, in them, the temperature remains constant. Thus, if, at the commencement of the cycle, the volume and pressure be v and p , they will have become $v + dv$ and $p \frac{v}{v + dv}$ at the end of the first operation. Hence the diminution of pressure, during the first operation, is $p - p \frac{v}{v + dv}$ or $p \frac{dv}{v + dv}$; and, therefore, if we neglect infinitely small quantities of the second order, we have $p \frac{dv}{v}$ for the diminution of pressure during the first operation; which, to the same degree of approximation, will be equal to the increase of pressure during the third. If $t + \tau$ and t be taken to denote the superior and inferior limits of temperature, we shall thus have for the volume, the temperature, and the pressure at the commencements of the four successive operations, and at the end of the cycle, the following values respectively:—

(1.)	$v,$	$t + \tau,$	$p;$
(2.)	$v + dv,$	$t + \tau,$	$p \left(1 - \frac{dv}{v}\right);$
(3.)	$v + dv + \phi,$	$t,$	$p \left(1 - \frac{dv}{v}\right) - \omega;$
(4.)	$v + \phi,$	$t,$	$p - \omega;$
(5.)	$v,$	$t + \tau,$	$p.$

Taking the mean of the pressures at the beginning and end of each operation, we find

$$\begin{aligned}
 (1.) \quad & p \left(1 - \frac{1}{2} \frac{dv}{v}\right) \\
 (2.) \quad & p \left(1 - \frac{dv}{v}\right) - \frac{1}{2} \omega \\
 (3.) \quad & p \left(1 - \frac{1}{2} \frac{dv}{v}\right) - \omega \\
 (4.) \quad & p - \frac{1}{2} \omega,
 \end{aligned}$$

which, as we are neglecting infinitely small quantities of the second order, will be

the expressions for the mean pressures during the four successive operations. Now, the mechanical effect gained or spent, during any of the operations, will be found by multiplying the mean pressure by the increase or diminution of volume which takes place; and we thus find

$$(1.) \quad p \left(1 - \frac{1}{2} \frac{dv}{v} \right) dv$$

$$(2.) \quad \left\{ p \left(1 - \frac{dv}{v} \right) - \frac{1}{2} \omega \right\} \varphi$$

$$(3.) \quad \left\{ p \left(1 - \frac{1}{2} \frac{dv}{v} \right) - \omega \right\} dv$$

$$(4.) \quad (p - \frac{1}{2} \omega) \varphi$$

for the amounts gained during the first and second, and spent during the third and fourth operations; and hence, by addition and subtraction, we find

$$\omega dv - p \varphi \frac{dv}{v}, \text{ or } (v \omega - p \varphi) \frac{dv}{v},$$

for the aggregate amount of mechanical effect gained during the cycle of operations. It only remains for us to express this result in terms of dq and τ , on which the given thermal agency depends. For this purpose, we remark that φ and ω are alterations of volume and pressure which take place along with a change of temperature τ , and hence, by the laws of compressibility and expansion, we may establish a relation* between them in the following manner.

Let p_0 be the pressure of the mass of air when reduced to the temperature zero, and confined in a volume v_0 ; then, whatever be v , the product $p_0 v_0$ will, by the law of compressibility, remain constant; and, if the temperature be elevated from 0 to $t + \tau$, and the gas be allowed to expand freely without any change of pressure, its volume will be increased in the ratio of 1 to $1 + E(t + \tau)$, where E is very nearly equal to .00366 (the centigrade scale of the air-thermometer being referred to), whatever be the gas employed, according to the researches of REGNAULT and of MAGNUS on the expansion of gases by heat. If, now, the volume be altered arbitrarily with the temperature continually at $t + \tau$, the product of the pressure and volume will remain constant; and, therefore, we have

$$p v = p_0 v_0 \{1 + E(t + \tau)\}.$$

Similarly

$$(p - \omega)(v + \varphi) = p_0 v_0 \{1 + E t\}.$$

Hence, by subtraction, we have

$$v \omega - p \varphi + \omega \varphi = p_0 v_0 E \tau,$$

or, neglecting the product $\omega \varphi$,

$$v \omega - p \varphi = p_0 v_0 E \tau.$$

* We might also investigate another relation, to express the fact that there is no accession or removal of heat during either the second or the fourth operation; but it will be seen that this will not affect the result in the text; although it would enable us to determine both φ and ω in terms of τ .

Hence, the preceding expression for mechanical effect, gained in the cycle of operations, becomes

$$p_0 v_0 \cdot E \tau \cdot \frac{dv}{v},$$

Or, as we may otherwise express it,

$$\frac{E p_0 v_0}{v \frac{dq}{dv}} \cdot dq \cdot \tau.$$

Hence, if we denote by M the mechanical effect due to H units of heat descending through the same interval τ , which might be obtained by repeating the cycle of operations described above, $\frac{H}{\frac{dq}{dv}}$ times, we have

$$M = \frac{E p_0 v_0}{v \frac{dq}{dv}} \cdot H \tau \quad \dots \quad (3)$$

27. If the *amplitudes* of the operations had been finite, so as to give rise to an absorption of H units of heat during the first operation, and a lowering of temperature from S to T during the second, the amount of work obtained would have been found to be expressed by means of a double definite integral, thus:—

$$\left. \begin{aligned} M &= \int_0^H dq \int_T^S dt \cdot \frac{E p_0 v_0}{v \frac{dq}{dv}} \\ \text{or} \quad M &= E p_0 v_0 \int_0^H \int_T^S \frac{1}{v} \frac{dv}{dq} \cdot dt dq; \end{aligned} \right\} \dots \dots \dots (4),$$

this second form being sometimes more convenient.

28. The preceding investigations, being founded on the approximate laws of compressibility and expansion (known as the law of MARIOTTE and BOYLE, and the law of DALTON and GAY-LUSSAC), would require some slight modifications, to adapt them to cases in which the gaseous medium employed is such as to present sensible deviations from those laws. REGNAULT'S very accurate experiments shew that the deviations are insensible, or very nearly so, for the ordinary gases at ordinary pressures; although they may be considerable for a medium, such as

* This result might have been obtained by applying the usual notation of the integral calculus to express the area of the curvilinear quadrilateral, which, according to CLAPEYRON'S graphical construction, would be found to represent the entire mechanical effect gained in the cycle of operations of the air-engine. It is not necessary, however, to enter into the details of this investigation, as the formula (3), and the consequences derived from it, include the whole theory of the air-engine, in the best practical form; and the investigation of it which I have given in the text, will probably give as clear a view of the reasoning on which it is founded, as could be obtained by the graphical method, which, in this case, is not so valuable as it is from its simplicity in the case of the steam-engine.

sulphurous acid, or carbonic acid under high pressure, which approaches the physical condition of a vapour at saturation; and therefore, in general, and especially in practical applications to real air-engines, it will be unnecessary to make any modification in the expressions. In cases where it may be necessary, there is no difficulty in making the modifications, when the requisite data are supplied by experiment.

29.* Either the steam-engine or the air-engine, according to the arrangements described above, gives all the mechanical effect that can possibly be obtained from the thermal agency employed. For it is clear, that, in either case, the operations may be performed in the reverse order, with every thermal and mechanical effect reversed. Thus, in the steam-engine, we may commence by placing the cylinder on the impermeable stand, allow the piston to rise, performing work, to the position $E_3 F_3$; we may then place it on the body B, and allow it to rise, performing work, till it reaches $E_2 F_2$; after that the cylinder may be placed again on the impermeable stand, and the piston may be pushed down to $E_1 F_1$; and, lastly, the cylinder being removed to the body A, the piston may be pushed down to its primitive position. In this inverse cycle of operations, a certain amount of work has been spent, precisely equal, as we readily see, to the amount of mechanical effect gained in the direct cycle described above; and heat has been abstracted from B, and deposited in the body A, at a higher temperature, to an amount precisely equal to that which, in the direct cycle, was *let down* from A to B. Hence it is impossible to have an engine which will derive more mechanical effect from the same thermal agency, than is obtained by the arrangement described above; since, if there could be such an engine, it might be employed to perform, as a part of its whole work, the inverse cycle of operations, upon an engine of the kind we have considered, and thus to continually restore the heat from B to A, which has descended from A to B for working itself; so that we should have a complex engine, giving a residual amount of mechanical effect without any thermal agency, or alteration of materials, which is an impossibility in nature. The same reasoning is applicable to the air-engine; and we conclude, generally, that any two engines, constructed on the principles laid down above, whether steam-engines with different liquids, an air-engine and a steam-engine, or two air-engines with different gases, must derive the same amount of mechanical effect from the same thermal agency.

30. Hence, by comparing the amounts of mechanical effect obtained by the steam-engine and the air-engine from the letting down of the H units of heat from A at the temperature $(t + \tau)$ to B at t , according to the expressions (2) and (3), we have

* This paragraph is the demonstration referred to above, of the proposition stated in § 13; as it is readily seen that it is applicable to any conceivable kind of thermo-dynamic engine.

$$M = (1 - \sigma) \frac{\frac{dp}{dt}}{k} \cdot H \tau = \frac{E p_0 v_0}{v \frac{dq}{dv}} \cdot H \tau \quad . \quad . \quad . \quad (5).$$

If we denote the coefficient of $H \tau$ in these equal expressions by μ , which may be called "CARNOT'S coefficient," we have

$$\mu = (1 - \sigma) \frac{\frac{dp}{dt}}{k} = \frac{E p_0 v_0}{v \frac{dq}{dv}} \quad . \quad . \quad . \quad (6),$$

and we deduce the following very remarkable conclusions:—

(1.) For the saturated vapours of all different liquids, at the same temperature, the value of

$$(1 - \sigma) \frac{\frac{dp}{dt}}{k}$$

must be the same.

(2.) For any different gaseous masses, at the same temperature, the value of

$$\frac{E p_0 v_0}{v \frac{dq}{dv}}$$

must be the same.

(3.) The values of these expressions for saturated vapours and for gases, at the same temperature, must be the same.

31. No conclusion can be drawn *a priori* regarding the values of this coefficient μ for different temperatures, which can only be determined, or compared, by experiment. The results of a great variety of experiments, in different branches of physical science (Pneumatics and Acoustics), cited by CARNOT and by CLAPEYRON, indicate that the values of μ for low temperatures exceed the values for higher temperatures; a result amply verified by the continuous series of experiments performed by REGNAULT on the saturated vapour of water for all temperatures from 0° to 230° , which, as we shall see below, give values for μ gradually diminishing from the inferior limit to the superior limit of temperature. When, by observation, μ has been determined as a function of the temperature, the amount of mechanical effect, M , deducible from H units of heat descending from a body at the temperature S to a body at the temperature T , may be calculated from the expression,

$$M = H \int_T^S \mu dt \quad . \quad . \quad . \quad (7)$$

which is, in fact, what either of the equations (1) for the steam-engine, or (4) for the air-engine, becomes, when the notation μ , for CARNOT'S multiplier, is introduced.

The values of this integral may be practically obtained, in the most convenient manner, by first determining, from observation, the mean values of μ for the successive degrees of the thermometric scale, and then adding the values for all the degrees within the limits of the extreme temperatures S and T.*

32. The complete theoretical investigation of the motive power of heat is thus reduced to the experimental determination of the coefficient μ ; and may be considered as perfect, when, by any series of experimental researches whatever, we can find a value of μ for every temperature within practical limits. The special character of the experimental researches, whether with reference to gases, or with reference to vapours, necessary and sufficient for this object, is defined and restricted in the most precise manner, by the expressions (6) for μ , given above.

33. The object of REGNAULT'S great work, referred to in the title of this paper, is the experimental determination of the various physical elements of the steam-engine; and when it is complete, it will furnish all the *data* necessary for the calculation of μ . The valuable researches already published in a first part of that work, make known the latent heat of a given weight, and the pressure, of saturated steam for all temperatures between 0° and 230° cent. of the air-thermometer. Besides these data, however, the density of saturated vapour must be known, in order that k , the latent heat of a unit of volume, may be calculated from REGNAULT'S determination of the latent heat of a given weight.† Between the limits of 0° and 100°, it is probable, from various experiments which have been made, that the density of vapour follows very closely the simple laws which are so accurately verified by the ordinary gases;‡ and thus it may be calculated from REGNAULT'S table giving the pressure at any temperature within those limits. Nothing as yet is known with accuracy as to the density of saturated steam between 100° and 230°, and we must be contented at present to estimate it by calculation from REGNAULT'S table of pressures; although, when accurate experimental researches on the subject shall have been made, considerable deviations from the laws of BOYLE and DALTON, on which this calculation is founded, may be discovered.

* The results of these investigations are exhibited in Tables I. and II. below.

† It is, comparatively speaking, of little consequence to know accurately the value of σ , for the factor $(1-\sigma)$ of the expression for μ , since it is so small (being less than $\frac{1}{17700}$ for all temperatures between 0° and 100°) that, unless all the data are known with more accuracy than we can count

upon at present, we might neglect it altogether, and take $\frac{dp}{dt}$ simply, as the expression for μ , without committing any error of important magnitude.

‡ This is well established, within the ordinary atmospheric limits, in REGNAULT'S *Études Météorologiques*, in the *Annales de Chimie*, vol. xv., 1846.

34. Such are the experimental data on which the mean values of μ for the successive degrees of the air-thermometer, from 0° to 230° , at present laid before the Royal Society, is founded. The unit of length adopted is the English foot; the unit of weight, the pound; the unit of work, a "foot-pound;" and the unit of heat that quantity which, when added to a pound of water at 0° , will produce an elevation of 1° in temperature. The mean value of μ for any degree is found to a sufficient degree of approximation, by taking, in place of σ , $\frac{dp}{dt}$, and k ; in the expression

$$(1 - \sigma) \frac{\frac{dp}{dt}}{k};$$

the mean values of those elements; or, what is equivalent to the corresponding accuracy of approximation, by taking, in place of σ and k respectively, the mean of the values of those elements for the limits of temperature, and in place of $\frac{dp}{dt}$, the difference of the values of p , at the same limits.

35. In REGNAULT'S work (at the end of the eighth *Mémoire*), a table of the pressures of saturated steam for the successive temperatures 0° , 1° , 2° , . . . 230° , expressed in millimetres of mercury, is given. On account of the units adopted in this paper, these pressures must be estimated in pounds on the square foot, which we may do by multiplying each number of millimetres by 2.7896, the weight in pounds of a sheet of mercury, one millimetre thick, and a square foot in area.

36. The value of k , the latent heat of a cubic foot, for any temperature t , is found from λ , the latent heat of a pound of saturated steam, by the equation

$$k = \frac{p}{760} \cdot \frac{1 + .00366 \times 100}{1 + .00366 \times t} \cdot \times .036869 \cdot \lambda,$$

where p denotes the pressure in millimetres, and λ the latent heat of a pound of saturated steam; the values of λ being calculated by the empirical formula*

$$\lambda = (606.5 + 0.305 t) - (t + .00002 t^2 + 0.000000 t^3),$$

given by REGNAULT as representing, between the extreme limits of his observations, the latent heat of a unit weight of saturated steam.

* The part of this expression in the first vinculum (see REGNAULT, end of ninth *Mémoire*) is what is known as "the total heat" of a pound of steam, or the amount of heat necessary to convert a pound of water at 0° into a pound of saturated steam at t° ; which, according to "WATT'S law," thus approximately verified, would be constant. The second part, which would consist of the single term t , if the specific heat of water were constant for all temperatures, is the number of thermic units necessary to raise the temperature of a pound of water from 0° to t° , and expresses empirically the results of REGNAULT'S experiments on the specific heat of water (see end of the tenth *Mémoire*), described in the work already referred to.

Explanation of Table I.

37. The mean values of μ for the first, for the eleventh, for the twenty-first, and so on, up to the 231st* degree of the air-thermometer, have been calculated in the manner explained in the preceding paragraphs. These, and interpolated results, which must agree with what would have been obtained, by direct calculation from REGNAULT'S data, to three significant places of figures (and even for the temperatures between 0° and 100° , the experimental data do not justify us in relying on any of the results to a greater degree of accuracy), are exhibited in Table I.

To find the amount of mechanical effect due to a unit of heat, descending from a body at a temperature S to a body at T, if these numbers be integers, we have merely to add the values of μ in Table I. corresponding to the successive numbers.

$$T+1, T+2, \dots S-2, S-1,$$

Explanation of Table II.

38. The calculation of the mechanical effect, in any case, which might always be effected in the manner described in § 37 (with the proper modification for fractions of degrees, when necessary), is much simplified by the use of Table II., where the first number of Table I., the sum of the first and second, the sum of the first three, the sum of the first four, and so on, are successively exhibited. The sums thus tabulated are the values of the integrals

$$\int_0^1 \mu dt, \int_0^2 \mu dt, \int_0^3 \mu dt, \dots \int_0^{231} \mu dt;$$

and, if we denote $\int_0^t \mu dt$ by the letter M, Table II. may be regarded as a table of the values of M.

To find the amount of mechanical effect due to a unit of heat descending from a body at a temperature S to a body at T, if these numbers be integers, we have merely to subtract the value of M, for the number T + 1, from the value for the number S, given in Table II.

* In strictness, the 230th is the last degree for which the experimental data are complete; but the data for the 231st may readily be assumed in a sufficiently satisfactory manner.

TABLE I.* *Mean Values of μ for the successive Degrees of the Air-Thermometer from 0° to 230°.*

	μ		μ		μ		μ		μ
1°	4.960	48°	4.366	94°	3.889	140°	3.549	186°	3.309
2	4.946	49	4.355	95	3.880	141	3.543	187	3.304
3	4.932	50	4.343	96	3.871	142	3.537	188	3.300
4	4.918	51	4.331	97	3.863	143	3.531	189	3.295
5	4.905	52	4.319	98	3.854	144	3.525	190	3.291
6	4.892	53	4.308	99	3.845	145	3.519	191	3.287
7	4.878	54	4.296	100	3.837	146	3.513	192	3.282
8	4.865	55	4.285	101	3.829	147	3.507	193	3.278
9	4.852	56	4.273	102	3.820	148	3.501	194	3.274
10	4.839	57	4.262	103	3.812	149	3.495	195	3.269
11	4.826	58	4.250	104	3.804	150	3.490	196	3.265
12	4.812	59	4.239	105	3.796	151	3.484	197	3.261
13	4.799	60	4.227	106	3.788	152	3.479	198	3.257
14	4.786	61	4.216	107	3.780	153	3.473	199	3.253
15	4.773	62	4.205	108	3.772	154	3.468	200	3.249
16	4.760	63	4.194	109	3.764	155	3.462	201	3.245
17	4.747	64	4.183	110	3.757	156	3.457	202	3.241
18	4.735	65	4.172	111	3.749	157	3.451	203	3.237
19	4.722	66	4.161	112	3.741	158	3.446	204	3.233
20	4.709	67	4.150	113	3.734	159	3.440	205	3.229
21	4.697	68	4.140	114	3.726	160	3.435	206	3.225
22	4.684	69	4.129	115	3.719	161	3.430	207	3.221
23	4.672	70	4.119	116	3.712	162	3.424	208	3.217
24	4.659	71	4.109	117	3.704	163	3.419	209	3.213
25	4.646	72	4.098	118	3.697	164	3.414	210	3.210
26	4.634	73	4.088	119	3.689	165	3.409	211	3.206
27	4.621	74	4.078	120	3.682	166	3.404	212	3.202
28	4.609	75	4.067	121	3.675	167	3.399	213	3.198
29	4.596	76	4.057	122	3.668	168	3.394	214	3.195
30	4.584	77	4.047	123	3.661	169	3.389	215	3.191
31	4.572	78	4.037	124	3.654	170	3.384	216	3.188
32	4.559	79	4.028	125	3.647	171	3.380	217	3.184
33	4.547	80	4.018	126	3.640	172	3.375	218	3.180
34	4.535	81	4.009	127	3.633	173	3.370	219	3.177
35	4.522	82	3.999	128	3.627	174	3.365	220	3.173
36	4.510	83	3.990	129	3.620	175	3.361	221	3.169
37	4.498	84	3.980	130	3.614	176	3.356	222	3.165
38	4.486	85	3.971	131	3.607	177	3.351	223	3.162
39	4.474	86	3.961	132	3.601	178	3.346	224	3.158
40	4.462	87	3.952	133	3.594	179	3.342	225	3.155
41	4.450	88	3.943	134	3.586	180	3.337	226	3.151
42	4.438	89	3.934	135	3.579	181	3.332	227	3.148
43	4.426	90	3.925	136	3.573	182	3.328	228	3.144
44	4.414	91	3.916	137	3.567	183	3.323	229	3.141
45	4.402	92	3.907	138	3.561	184	3.318	230	3.137
46	4.390	93	3.898	139	3.555	185	3.314	231	3.134
47	4.378								

* The numbers here tabulated may also be regarded as, the actual values of μ for $t = \frac{1}{2}$, $t = 1\frac{1}{2}$, $t = 2\frac{1}{2}$, $t = 3\frac{1}{2}$, &c.

TABLE II. *Mechanical Effect in Foot-Pounds due to a Thermic Unit Centigrade, passing from a body, at any Temperature less than 230° to a body at 0.*

Superior Limit of Tempe- rature.	Mechanical Effect.	Superior Limit of Tempe- rature.	Mechanical Effect.	Superior Limit of Tempe- rature.	Mechanical Effect.	Superior Limit of Tempe- rature.	Mechanical Effect.	Superior Limit of Tempe- rature.	Mechanical Effect.
	Foot-pounds.		Foot-pounds.		Foot-pounds.		Foot-pounds.		Foot-pounds.
1°	4·960	48°	223·487	94°	412·545	140°	582·981	186°	740·310
2	9·906	49	227·842	95	416·425	141	586·524	187	743·614
3	14·838	50	232·185	96	420·296	142	590·061	188	746·914
4	19·756	51	236·516	97	424·159	143	593·592	189	750·209
5	24·661	52	240·835	98	428·013	144	597·117	190	753·500
6	29·553	53	245·143	99	431·858	145	600·636	191	756·787
7	34·431	54	249·439	100	435·695	146	604·099	192	760·069
8	39·296	55	253·724	101	439·524	147	607·656	193	763·347
9	44·148	56	257·997	102	443·344	148	611·157	194	766·621
10	48·987	57	262·259	103	447·156	149	614·652	195	769·890
11	53·813	58	266·509	104	450·960	150	618·142	196	773·155
12	58·625	59	270·743	105	454·756	151	621·626	197	776·416
13	63·424	60	274·975	106	458·544	152	625·105	198	779·673
14	68·210	61	279·191	107	462·324	153	628·578	199	782·926
15	72·983	62	283·396	108	466·096	154	632·046	200	786·175
16	77·743	63	287·590	109	469·860	155	635·508	201	789·420
17	82·490	64	291·773	110	473·617	156	638·965	202	792·661
18	87·225	65	295·945	111	477·366	157	642·416	203	795·898
19	91·947	66	300·106	112	481·107	158	645·862	204	799·131
20	96·656	67	304·256	113	484·841	159	649·302	205	802·360
21	101·353	68	308·396	114	488·567	160	652·737	206	805·585
22	106·037	69	312·525	115	492·286	161	656·167	207	808·806
23	110·709	70	316·644	116	495·998	162	659·591	208	812·023
24	115·368	71	320·752	117	499·702	163	663·010	209	815·236
25	120·014	72	324·851	118	503·399	164	666·424	210	818·446
26	124·648	73	328·939	119	507·088	165	669·833	211	821·652
27	129·269	74	333·017	120	510·770	166	673·237	212	824·854
28	133·878	75	337·084	121	514·445	167	676·636	213	828·052
29	138·474	76	341·141	122	518·113	168	680·030	214	831·247
30	143·058	77	345·188	123	521·174	169	683·419	215	834·438
31	147·630	78	349·225	124	525·428	170	686·803	216	837·626
32	152·189	79	353·253	125	529·075	171	690·183	217	840·810
33	156·736	80	357·271	126	532·715	172	693·558	218	843·990
34	161·271	81	361·280	127	536·348	173	696·928	219	847·167
35	165·793	82	365·279	128	539·975	174	700·293	220	850·340
36	170·303	83	369·269	129	543·595	175	703·654	221	853·509
37	174·801	84	373·249	130	547·209	176	707·010	222	856·674
38	179·287	85	377·220	131	550·816	177	710·361	223	859·836
39	183·761	86	381·181	132	554·417	178	713·707	224	862·994
40	188·223	87	385·133	133	558·051	179	717·049	225	866·149
41	192·673	88	389·076	134	561·597	180	720·386	226	869·300
42	197·111	89	393·010	135	565·176	181	723·718	227	872·448
43	201·537	90	396·935	136	568·749	182	727·046	228	875·592
44	205·951	91	400·851	137	572·316	183	730·369	229	878·733
45	210·353	92	404·758	138	575·877	184	733·687	230	881·870
46	214·743	93	408·656	139	579·432	185	737·001	231	885·004
47	219·121								

Note.—On the curves described in CLAPEYRON'S graphical method of exhibiting CARNOT'S Theory of the Steam-Engine.

39. At any instant when the temperature of the water and vapour is t , during the fourth operation (see above, § 16), the latent heat of the vapour must be precisely equal to the amount of heat that would be necessary to raise the temperature of the whole mass, if in the liquid state, from t to S .* Hence, if v' denote the volume of the vapour, c the mean capacity for heat of a pound of water between the temperatures S and t , and W the weight of the entire mass, in pounds, we have

$$k v' = c (S - t) W.$$

Again, the circumstances during the second operation are such that the mass of liquid and vapour possesses H units of heat more than during the fourth; and consequently, at the instant of the second operation, when the temperature is t , the volume v of the vapour will exceed v' by an amount of which the latent heat is H , so that we have

$$v = v' + \frac{H}{k}$$

40. Now, at any instant, the volume between the piston and its primitive position is less than the actual volume of vapour by the volume of the water evaporated. Hence, if x and x' denote the abscissæ of the curve at the instants of the second and fourth operations respectively, when the temperature is t , we have

$$x = v - \sigma v, \quad x' = v' - \sigma v',$$

and, therefore, by the preceding equations,

$$x = \frac{1 - \sigma}{k} \{H + c (S - t) W\} \quad . \quad . \quad . \quad (a)$$

$$x' = \frac{1 - \sigma}{k} c (S - t) W \quad . \quad . \quad . \quad (b)$$

These equations, along with

$$y = y' = p \quad . \quad . \quad . \quad . \quad . \quad (c)$$

enable us to calculate, from the data supplied by REGNAULT, the abscissa and ordinate for each of the curves described above (§ 17), corresponding to any as-

* For, at the end of the fourth operation, the whole mass is liquid, and at the temperature t . Now, this state might be arrived at by first compressing the vapour into water at the temperature t , and then raising the temperature of the liquid to S ; and however this state may be arrived at, there cannot, on the whole, be any heat added to or subtracted from the contents of the cylinder, since, during the fourth operation, there is neither gain nor loss of heat. This reasoning is, of course, founded on CARNOT'S fundamental principle, which is tacitly assumed in the commonly-received ideas connected with "WATT'S law," the "latent heat of steam," and "the total heat of steam."

sumed temperature t . After the explanations of §§ 33, 34, 35, 36, it is only necessary to add that c is a quantity of which the value is very nearly unity, and would be exactly so were the capacity of water for heat the same at every temperature as it is between 0° and 1° ; and that the value of $c(S-t)$, for any assigned values of S and t , is found, by subtracting the number corresponding to t from the number corresponding to s , in the column headed "*Nombre des unités de chaleur abandonnées par un kilogramme d'eau en descendant de T° à 0°* ", of the last table (at the end of the Tenth Mémoire) of REGNAULT'S work. By giving S the value 230° , and by substituting successively 220, 210, 200, &c., for t , values for x , y , x' , y' , have been found, which are exhibited in the following Table:—

Temperatures. t	Volumes to be described by the piston, to complete the fourth operation. x'	Volumes from the primitive position of the piston to those occupied at instants of the second operation. x	Pressures of saturated steam, in pounds on the square foot. $y = y' = p$
0°	1269.W	$x' + 5.409.H$	12.832
10	639.6.W	$x' + 2.847.H$	25.567
20	337.3.W	$x' + 1.571.H$	48.514
30	185.5.W	$x' + .9062.H$	88.007
40	105.9.W	$x' + .5442.H$	153.167
50	62.62.W	$x' + .3392.H$	256.595
60	38.19.W	$x' + .2188.H$	415.070
70	21.94.W	$x' + .1456.H$	650.240
80	15.38.W	$x' + .09962.H$	989.318
90	10.09.W	$x' + .06994.H$	1465.80
100	6.744.W	$x' + .05026.H$	2120.11
110	4.578.W	$x' + .03688.H$	2999.87
120	3.141.W	$x' + .02758.H$	4160.10
130	2.176.W	$x' + .02098.H$	5663.70
140	1.519.W	$x' + .01625.H$	7581.15
150	1.058.W	$x' + .01271.H$	9990.26
160	0.7369.W	$x' + .01010.H$	12976.2
170	0.5085.W	$x' + .008116.H$	16630.7
180	0.3454.W	$x' + .006592.H$	21051.5
190	0.2267.W	$x' + .005406.H$	26341.5
200	0.1409.W	$x' + .004472.H$	32607.7
210	0.0784.W	$x' + .003729.H$	39960.7
220	0.3310.W	$x' + .003130.H$	48512.4
230	0	$x' + .002643.H$	58376.6

Appendix.

(Read April 30, 1849.)

41. In p. 30, some conclusions drawn by CARNOT from his general reasoning were noticed; according to which it appears, that if the value of μ for any temperature is known, certain information may be derived with reference to the saturated vapour of any liquid whatever, and, with reference to any gaseous mass, without the necessity of experimenting upon the specific medium considered. Nothing in the whole range of Natural Philosophy is more remarkable than the establishment of general laws by such a process of reasoning. We have seen, however, that doubt may exist with reference to the truth of the axiom on which the entire theory is founded, and it therefore becomes more than a matter of mere curiosity to put the inferences deduced from it to the test of experience. The importance of doing so was clearly appreciated by CARNOT; and, with such data as he had from the researches of various experimenters, he tried his conclusions. Some very remarkable propositions which he derives from his Theory, coincide with DULONG and PETIT's subsequently-discovered experimental laws with reference to the heat developed by the compression of a gas; and the experimental verification is therefore in this case (so far as its accuracy could be depended upon) decisive. In other respects, the data from experiment were insufficient, although, so far as they were available as tests, they were confirmatory of the theory.

42. The recent researches of REGNAULT add immensely to the experimental data available for this object, by giving us the means of determining with considerable accuracy the values of μ within a very wide range of temperature, and so affording a trustworthy standard for the comparison of isolated results at different temperatures, derived from observations in various branches of physical science.

In the first section of this Appendix the Theory is tested, and shewn to be confirmed by the comparison of the values of μ found above, with those obtained by CARNOT and CLAPEYRON from the observations of various experimenters on air, and the vapours of different liquids. In the second and third sections some striking confirmations of the theory arising from observations by DULONG, on the specific heat of gases, and from Mr JOULE's experiments on the heat developed by the compression of air, are pointed out; and in con-

clusion, the actual methods of obtaining mechanical effect from heat are briefly examined with reference to their economy.

I. *On the values of μ derived by Carnot and Clapeyron from observations on Air, and on the Vapours of various liquids.*

43. In CARNOT'S work, p. 80-82, the mean value of μ between 0° and 1° is derived from the experiments of DELAROCHE and BERARD on the specific heat of gases, by a process approximately equivalent to the calculation of the value of $E \frac{p_0 v_0}{v \frac{d q}{d v}}$ for the temperature $\frac{1}{2}^\circ$. There are also, in the same work, determinations

of the values of μ from observations on the vapours of alcohol and water; but a table given in M. CLAPEYRON'S paper, of the values of μ derived from the data supplied by various experiments with reference to the vapours of ether, alcohol, water, and oil of turpentine, at the respective boiling-points of these liquids, afford us the means of comparison through a more extensive range of temperature. In the cases of alcohol and water, these results ought of course to agree with those of CARNOT. There are, however, slight discrepancies which must be owing to the uncertainty of the experimental data.* In the following table, CARNOT'S results with reference to air, and CLAPEYRON'S results with reference to the four different liquids, are exhibited, and compared with the values of μ which have been given above (Table I.) for the same temperatures, as derived from REGNAULT'S observations on the vapour of water.

Names of the Media.	Temperatures.	Values of μ	Values of μ deduced from Regnault's Observations.	Differences.
Air,	0.5°	(CARNOT) 4.377	4.960	.383
Sulphuric Ether,	(Boiling point) 35.5°	(CLAPEYRON) 4.478	4.510	.032
Alcohol,	78.8	3.963	4.030	.071
Water,	100	3.658	3.837	.179
Essence of Turpentine,	156.8	3.530	3.449	-.081

44. It may be observed that the discrepancies between the results founded on the experimental data supplied by the different observers with reference to water at the boiling-point, are greater than those which are presented between the results deduced from any of the other liquids, and water at the other temperatures; and we may therefore feel perfectly confident that the verification is com-

* Thus, from CARNOT'S calculations, we find, in the case of alcohol, 4.035; and in the case of water, 3.648, instead of 3.963, and 3.658, which are CLAPEYRON'S results in the same cases.

plete to the extent of accuracy of the observations.* The considerable discrepancy presented by CARNOT'S result, deduced from experiments on air, is not to be wondered at when we consider the very uncertain nature of his data.

45. The fact of the gradual decrease of μ , through a very extensive range of temperature, being indicated both by REGNAULT'S continuous series of experiments, and by the very varied experiments on different media, and in different branches of Physical Science, must be considered as a striking verification of the theory.

II. *On the Heat developed by the compression of Air.*

46. Let a mass of air, occupying initially a given volume V , under a pressure P , at a temperature t , be compressed to a less volume V' , and allowed to part with heat until it sinks to its primitive temperature t . The quantity of heat which is evolved may be determined, according to CARNOT'S theory, when the particular value of μ , corresponding to the temperature t , is known. For, by equation § 30, equation (6), we have

$$v \frac{dq}{dv} = \frac{E p_0 v_0}{\mu},$$

where dq is the quantity of heat absorbed, when the volume is allowed to increase from v to $v + dv$; or the quantity evolved by the reverse operation. Hence we deduce

$$dq = \frac{E p_0 v_0}{\mu} \frac{dv}{v} \dots \dots (8),$$

Now, $\frac{E p_0 v_0}{\mu}$ is constant, since the temperature remains unchanged; and therefore, we may at once integrate the second number. By taking it between the limits V' and V , we thus find

$$Q = \frac{E p_0 v_0}{\mu} \log \frac{V^\dagger}{V'} \dots \dots (9),$$

where Q denotes the required amount of heat, evolved by the compression from V to V' . This expression may be modified by employing the equations $PV = P' V' = p_0 v_0 (1 + E t)$; and we thus obtain

$$Q = \frac{EPV}{\mu(1 + Et)} \log \frac{V}{V'} = \frac{EP'V'}{\mu(1 + Et)} \log \frac{V}{V'} \dots \dots (10)$$

* A still closer agreement must be expected, when more accurate experimental data are afforded with reference to the other media. Mons. REGNAULT informs me that he is engaged in completing some researches, from which we may expect, possibly before the end of the present year, to be furnished with all the data for five or six different liquids which we possess at present for water. It is therefore to be hoped that, before long, a most important test of the validity of CARNOT'S theory will be afforded.

† The Napierian logarithm of $\frac{V}{V'}$ is here understood.

From this result we draw the following conclusion :—

47. *Equal volumes of all elastic fluids, when compressed to smaller equal volumes, disengage equal quantities of heat.*

This extremely remarkable theorem of CARNOT'S was independently laid down as a probable experimental law by DULONG, in his "*Recherches sur la Chaleur Spécifique des Fluides Élastiques*," and it therefore affords a most powerful confirmation of the theory.*

48. In some very remarkable researches made by Mr JOULE upon the heat developed by the compression of air, the quantity of heat produced in different experiments has been ascertained with reference to the amount of work spent in the operation. To compare the results which he has obtained with the indications of theory, let us determine the amount of work necessary actually to produce the compression considered above.

49. In the first place, to compress the gas from the volume $v + dv$ to v , the work required is $p dv$, or, since $p v = p_0 v_0 (1 + E t)$,

$$p_0 v_0 (1 + E t) \frac{dv}{v},$$

Hence, if we denote by W the total amount of work necessary to produce the compression from V to V' , we obtain, by integration,

$$W = p_0 v_0 (1 + E t) \log \frac{V}{V'},$$

Comparing this with the expression above, we find

$$\frac{W}{Q} = \frac{\mu (1 + E t)}{E} \quad . \quad . \quad . \quad (11)$$

50. Hence we infer that

(1.) The amount of work necessary to produce a unit of heat by the compression of a gas, is the same for all gases at the same temperature.

(2.) And that the quantity of heat evolved in all circumstances, when the temperature of the gas is given, is proportional to the amount of work spent in the compression.

* CARNOT varies the statement of his theorem, and illustrates it in a passage, pp. 52, 53, of which the following is a translation :—

"When a gas varies in volume without any change of temperature, the quantities of heat absorbed or evolved by this gas are in arithmetical progression, if the augmentation or diminutions of volume are in geometrical progression.

"When we compress a litre of air maintained at the temperature 10° , and reduce it to half a litre, it disengages a certain quantity of heat. If, again, the volume be reduced from half a litre to a quarter of a litre, from a quarter to an eighth, and so on, the quantities of heat successively evolved will be the same.

"If, in place of compressing the air, we allow it to expand to two litres, four litres, eight litres, &c., it will be necessary to supply equal quantities of heat to maintain the temperature always at the same degree."

51. The expression for the amount of work necessary to produce a unit of heat is

$$\frac{\mu(1 + E t)}{E},$$

and therefore REGNAULT'S experiments on steam are available to enable us to calculate its value for any temperature. By finding the values of μ at 0° , 10° , 20° , &c., from Table I., and by substituting successively the values 0, 10, 20, &c., for t , the following results have been obtained.

Table of the Values of $\frac{\mu(1 + E t)}{E}$,

Work requisite to produce a unit of Heat by the compression of a Gas.	Temperature of the Gas.	Work requisite to produce a unit of Heat by the compression of a Gas.	Temperature of the Gas.
Ft.-lbs.	$^\circ$	Ft.-lbs.	$^\circ$
1357.1	0	1446.4	120
1368.7	10	1455.8	130
1379.0	20	1465.3	140
1388.0	30	1475.8	150
1395.7	40	1489.2	160
1401.8	50	1499.0	170
1406.7	60	1511.3	180
1412.0	70	1523.5	190
1417.6	80	1536.5	200
1424.0	90	1550.2	210
1430.6	100	1564.0	220
1438.2	110	1577.8	230

Mr JOULE'S experiments were all conducted at temperatures from 50° to about 60° Fahr., or from 10° to 16° cent.; and, consequently, although some irregular differences in the results, attributable to errors of observation inseparable from experiments of such a very difficult nature are presented, no regular dependence on the temperature is observable. From three separate series of experiments, Mr JOULE deduces the following numbers for the work, in foot-pounds, necessary to produce a thermic unit Fahrenheit by the compression of a gas.

820, 814, 760.

Multiplying these by 1.8, to get the corresponding number for a thermic unit centigrade, we find

1476, 1465, and 1368.

The largest of these numbers is most nearly conformable with Mr JOULE'S views of the relation between such experimental "equivalents," and others which he obtained in his electro-magnetic researches; but the smallest agrees almost perfectly with the indications of CARNOT'S theory; from which, as exhibited in the preceding Table, we should expect, from the temperature in Mr JOULE'S experiments, to find a number between 1369 and 1379 as the result.

III. *On the Specific-Heats of Gases.*

52. The following proposition is proved by CARNOT as a deduction from his general theorem regarding the specific heats of gases.

The excess of the specific heat under a constant pressure above the specific heat at a constant volume, is the same for all gases at the same temperature and pressure.*

53. To prove this proposition, and to determine an expression for the "excess" mentioned in its enunciation, let us suppose a unit of volume of a gas to be elevated in temperature by a small amount, τ . The quantity of heat required to do this will be $A\tau$, if A denote the specific heat at a constant volume. Let us next allow the gas to expand without going down in temperature, until its pressure becomes reduced to its primitive value. The expansion which will take place will be $\frac{E\tau}{1+E\tau}$, if the temperature be denoted by t ; and hence, by (8), the quantity of heat that must be supplied, to prevent any lowering of temperature, will be

$$\frac{E p_0 v_0}{\mu} \cdot \frac{E\tau}{1+E\tau}, \quad \text{or} \quad \frac{E^2 p}{\mu (1+E\tau)^2} \tau.$$

Hence, the total quantity added is equal to

$$A\tau + \frac{E^2 p}{\mu (1+E\tau)^2} \tau$$

But, since B denotes the specific heat under constant pressure, the quantity of heat requisite to bring the gas into this state, from its primitive condition, is equal to $B\tau$; and hence we have

$$B = A + \frac{E^2 p}{\mu (1+E\tau)^2} \quad \dots \quad (12)$$

IV. *Comparison of the Relative advantages of the Air-Engine and Steam-Engine.*

54. In the use of water-wheels for motive power, the economy of the engine depends not only upon the excellence of its adaptation for actually transmitting any given quantity of water through it, and producing the equivalent of work, but upon turning to account the entire available fall; so, as we are taught by CARNOT, the object of a thermodynamic engine is to economize in the best possible way the transference of all the heat evolved, from bodies at the temperature of the source, to bodies at the lowest temperature at which the heat can be discharged. With reference then to any engine of the kind, there will be two points to be considered.

(1.) The extent of the *fall* utilised.

(2.) The economy of the engine, with the fall which it actually uses.

55. In the first respect, the air-engine, as CARNOT himself points out, has a

* Or the capacity of a unit of volume for heat.

vast advantage over the steam-engine; since the temperature of the hot part of the machine may be made very much higher in the air-engine than would be possible in the steam-engine, on account of the very high pressure produced in the boiler, by elevating the temperature of the water which it contains to any considerable extent above the atmospheric boiling point. On this account, a "perfect air-engine" would be a much more valuable instrument than a "perfect steam-engine."*

Neither steam-engines nor air-engines, however, are nearly perfect; and we do not know in which of the two kinds of machine the nearest approach to perfection may be actually attained. The beautiful engine invented by Mr STIRLING of Galston, may be considered as an excellent beginning for the air-engine;† and it is only necessary to compare this with NEWCOMEN'S steam-engine, and consider what WATT has effected, to give rise to the most sanguine anticipations of improvement.

V. *On the Economy of actual Steam-Engines.*

56. The steam-engine being universally employed at present as the means for deriving motive power from heat, it is extremely interesting to examine, according to CARNOT'S theory, the economy actually attained in its use. In the first place, we remark that, out of the entire "fall" from the temperature of the coals to that of the atmosphere, it is only part—that from the temperature of the boiler to the temperature of the condenser—that is made available; while the very great fall from the temperature of the burning coals to that of the boiler, and the comparatively small fall from the temperature of the condenser to that of the atmosphere, are entirely lost as far as regards the mechanical effect which it is desired to obtain. We infer from this, that the temperature of the boiler ought to be kept as high as, according to the strength, is consistent with safety, while that of the condenser ought to be kept as nearly down at the atmospheric temperature as possible. To take the entire benefit of the actual fall, CARNOT shewed that the "principle of expansion" must be pushed to the utmost.‡

* CARNOT suggests a combination of the two principles, with air as the medium for receiving the heat at a very high temperature from the furnace; and a second medium, alternately in the state of saturated vapour and liquid water, to receive the heat, discharged at an intermediate temperature from the air, and transmit it to the coldest part of the apparatus. It is possible that a complex arrangement of this kind might be invented, which would enable us to take the heat at a higher temperature, and discharge it at a lower temperature than would be practicable in any simple air-engine or simple steam-engine. If so, it would no doubt be equally possible, and perhaps more convenient, to employ steam alone, but to use it at a very high temperature not in contact with water in the hottest part of the apparatus, instead of, as in the steam-engine, always in a saturated state.

† It is probably this invention to which CARNOT alludes in the following passage (p. 112):—"Il a été fait, dit-on, tout récemment en Angleterre des essais heureux sur le développement de la puissance motrice par l'action de la chaleur sur l'air atmosphérique. Nous ignorons entièrement ne quoi ces essais ont consisté, si toutefois ils sont réels."

‡ From this point of view, we see very clearly how imperfect is the steam-engine, even after all WATT'S improvements. For to "push the principle of expansion to the utmost," we must allow the

57. To obtain some notion of the economy which has actually been obtained, we may take the alleged performances of the best Cornish engines, and some other interesting practica cases as examples.*

(1.) The engine of the *Fowey Consols mine* was reported, in 1845, to have given 125,089,000 foot-pounds of effect, for the consumption of one bushel or 94 lbs. of coals. Now, the average amount evaporated from Cornish boilers, by one pound of coal, is $8\frac{1}{2}$ lbs. of steam; and hence, for each pound of steam evaporated 156,556 foot-pounds of work are produced.

The pressure of the saturated steam in the boiler may be taken as $3\frac{1}{2}$ atmospheres;† and, consequently, the temperature of the water will be 140° . Now (REGNAULT, end of Memoire X.), the latent heat of a pound of saturated steam at 140° is 508, and since, to compensate for each pound of steam removed from the boiler in the working of the engine, a pound of water, at the temperature of the condenser, which may be estimated at 30° , is introduced from the hot well; it follows that 618 units of heat are introduced to the boiler for each pound of water evaporated. But the work produced, for each pound of water evaporated, was found above to be 156,556 foot-pounds. Hence, $\frac{156556}{618}$, or 253 foot-pounds is the amount of work produced for each unit of heat transmitted through the Fowey Consols engine. Now, in Table II., we find 583·0 as the theoretical effect due to a unit descending from 140° to 0° , and 143 as the effect due to a unit descending from 30° to 0° . The difference of these numbers, or 440,‡ is the number of foot-pounds of work that a *perfect* engine with its boiler at 140° , and its condenser at 30° would produce for each unit of heat transmitted. Hence, the Fowey Consols engine, during the experiments reported on, performed $\frac{253}{440}$ of its theoretical duty, or $57\frac{1}{2}$ per cent.

(2.) The best duty on record, as performed by an engine at work (not for merely experimental purposes), is that of TAYLOR'S engine, at the United mines, which, in 1840, worked regularly, for several months, at the rate of 98,000,000 foot-pounds for each bushel of coals burned. This is $\frac{98}{125}$, or ·784 of the experimental

steam, before leaving the cylinder, to expand until its pressure is the same as that of the vapour in the condenser. According to "WATT'S law," its temperature would then be the same as (actually a little above, as REGNAULT has shewn) that of the condenser, and hence the steam-engine worked in this most advantageous way, has in reality the very fault that WATT found in NEWCOMEN'S engine. This defect is partially remedied by HORNBLOWER'S system of using a separate expansion cylinder, an arrangement, the advantages of which did not escape CARNOT'S notice, although they have not been recognised extensively among practical engineers, until within the last few years.

* I am indebted to the kindness of Professor GORDON of Glasgow, for the information regarding the various cases given in the text.

† In different Cornish engines, the pressure in the boiler is from $2\frac{1}{2}$ to 5 atmospheres; and, therefore, as we find from REGNAULT'S table of the pressure of saturated steam, the temperature of the water in the boiler must, in all of them, lie between 128° and 152° . For the better class of engines, the average temperature of the water in the boiler may be estimated at 140° , the corresponding pressure of steam being $3\frac{1}{2}$ temperatures.

‡ This number agrees very closely with the number corresponding to the fall from 100° to 0° , given in Table II. Hence, the fall from 140° to 30° of the scale of the air-thermometer is equivalent, with reference to motive power, to the fall from 100° to 0° .

duty reported in the case of the Fowey Consols engine. Hence, the best useful work on record, is at the rate of 198·3 foot-pounds for each unit of heat transmitted, and is $\frac{198\cdot3}{440}$, or 45 per cent. of the theoretical duty, on the supposition that the boiler is at 140°, and the condenser at 30°.

(3.) French engineers contract (in Lille, in 1847, for example) to make engines for mill power which will produce 30,000 metre-lbs., or 98,427 foot-lbs. of work for each pound of steam used. If we divide this by 618, we find 159 foot-pounds for the work produced by each unit of heat. This is 36·1 per cent. of 440, the theoretical duty.*

(4.) English engineers have contracted to make engines and boilers which will require only 3½ lbs. of the best coal per horse-power per hour. Hence, in such engines, each pound of coal ought to produce 565,700 foot-pounds of work, and if 7 lbs. of water be evaporated by each pound of coal, there would result 80,814 foot-pounds of work for each pound of water evaporated. If the pressure in the boiler be 3½ atmospheres (temperature 140°) the amount of work for each unit of heat will be found, by dividing this by 618, to be 130·7 foot-pounds, which is $\frac{130\cdot7}{440}$ or 29·7 per cent. of the theoretical duty.†

(5.) The actual average of work performed by good Cornish engines and boilers is 55,000,000 foot-pounds for each bushel of coal, or less than half the experimental performance of the Fowey Consols engine, more than half the actual duty performed by the United Mines engine in 1840; in fact about 25 per cent. of the theoretical duty.

(6.) The average performances of a number of Lancashire engines and boilers have been recently found to be such as to require 12 lbs. of Lancashire coal per horse-power per hour (*i. e.*, for performing 60 × 33,000 foot-pounds) and of a number of Glasgow engines, such as to require 15 lbs. (of a somewhat inferior coal) for the same effect. There are, however, more than twenty large engines in Glasgow at present,‡ which work with a consumption of only 6½ lbs. of dross, equivalent to 5 lbs. of the best Scotch, or 4 lbs. of the best Welsh coal, per horse-power

* It being assumed that the temperatures of the boiler and condenser are the same as those of the Cornish engines. If, however, the pressure be lower, two atmospheres, for instance, the numbers would stand thus: The temperature in the boiler would be only 121. Consequently, for each pound of steam evaporated, only 614 units of heat would be required; and, therefore, the work performed for each unit of heat transmitted would be 160·3 foot-pounds, which is *more* than according to the estimate in the text. On the other hand, the range of temperatures, or the fall utilised, is only from 131 to 30, instead of from 140 to 30°, and, consequently (Table II.), the theoretical duty for each unit of heat is only 371 foot-pounds. Hence, if the engine, to work according to the specification, requires a pressure of only 15 lbs. on the square inch (*i. e.*, a total steam pressure of two atmospheres), its performance is $\frac{160\cdot3}{371}$, or 43·2 per cent. of its theoretical duty.

† If, in this case again, the pressure required in the boiler to make the engine work according to the contract were only 15 lbs. on the square inch, we should have a different estimate of the economy, for which, see Table B, at the end of this paper.

‡ These engines are provided with separate expansive cylinders, which have been recently added to them by Mr McNAUGHT of Glasgow.

per hour. The economy may be estimated from these data, as in the other cases, on the assumption which, with reference to these, is the most probable we can make, that the evaporation produced by a pound of best coal is 7 lbs. of steam.

58. The following Tables afford a synoptic view of the performances and theoretical duties in the various cases discussed above.

In Table A the numbers in the second column are found by dividing the numbers in the first by $8\frac{1}{2}$ in cases (1.), (2.), and (5.), and by 7 in cases (4.), (6.), and (7.), the estimated numbers of pounds of steam actually produced in the different boilers by the burning of 1 lb. of coal.

The numbers in the third column are found from those in the second, by dividing by 618, in Table A, and 614 in Table B, which are respectively the quantities of heat required to convert a pound of water taken from the hot well at 30° , into saturated steam, in the boiler, at 140° or at 121° .

With reference to the cases (3.), (4.), (6.), (7.), the hypothesis of Table B is probably in general nearer the truth than that of Table A. In (4.), (6.), and (7.), especially upon hypothesis B, there is much uncertainty as to the amount of evaporation that will be actually produced by 1 lb. of fuel. The assumption on which the numbers in the second column in Table B are calculated, is, that each pound of coal will send the same number of units of heat into the boiler whether hypothesis A or hypothesis B be followed. Hence, except in the case of the French contract, in which the *evaporation*, not the fuel, is specified, the numbers in the third column are the same as those in the third column of Table A.

TABLE A. *Various Engines in which the temperature of the Boiler is 140° , and that of the Condenser 30° .*

Theoretical Duty for each Unit of Heat transmitted, 440 foot-pounds.

CASES.	Work produced for each pound of coal con- sumed.	Work produced for each pound of water eva- porated.	Work produced for each unit of heat trans- mitted.	Per cent- age of theoreti- cal duty.
(1.) Fowey Consols Experiment, reported in 1845,	Foot-Pounds. 1,330,734	Foot-Pounds. 156,556	Foot-Pounds. 253	57.5
(2.) Taylor's Engine at the United Mines, work- ing in 1840, }	1,042,553	122,653	198.4	45.1
(3.) French Engines, according to contract, }	* * * *	98,427	159	36.1
(4.) English Engines, according to contract, }	565,700	80,814	130.8	29.7
(5.) Average actual performance of Cornish Engines,	585,106	68,836	111.3	25.3
(6.) Common Engines, consuming 12 lbs. of best coal per hour per horse-power, }	165,000	23,571	38.1	8.6
(7.) Improved Engines with Expansion Cylinders, consuming an equivalent to 4 lbs. of best coal per horse-power per hour, }	495,000	70,710	114.4	26

TABLE B. *Various Engines in which the Temperature of the Boilers is 121 °* and that of the Condenser 30°.**Theoretical Duty for each Unit of Heat transmitted, 371 foot-pounds.*

CASES.	Work produced for each pound of coal con- sumed.	Work produced for each pound of water eva- porated.	Work produced for each unit of heat trans- mitted.	Per cent- age of theoreti- cal duty.
	Foot-Pounds.	Foot-Pounds.	Foot-Pounds.	
(3.) French Engines, according to contract, .	* * *	98,427	160·3	43·2
(4.) English Engines, according to contract, .	565,700	$\frac{5}{8} \frac{1}{8} \times 80,814$	130·8	35
(6.) Common Engines, consuming 12 lbs. of coal per horse-power per hour, .	165,000	$\frac{5}{8} \frac{1}{8} \times 23,571$	38·1	10·3
(7.) Improved Engines with expansion cylinders, consuming an equivalent to 4 lbs. best coal per horse-power per hour, .	495,000	$\frac{5}{8} \frac{1}{8} \times 70,710$	114·4	30·7

* Pressure 15 lbs. on the square inch.

XXXVII.—*Theoretical Considerations on the Effect of Pressure in Lowering the Freezing Point of Water.* By JAMES THOMSON, Esq., of Glasgow. Communicated by Professor WILLIAM THOMSON.

(Read 2d January 1849.)

Some time ago my brother, Professor WILLIAM THOMSON, pointed out to me a curious conclusion to which he had been led, by reasoning on principles similar to those developed by CARNOT, with reference to the motive power of heat. It was, that *water at the freezing point may be converted into ice by a process solely mechanical, and yet without the final expenditure of any mechanical work.* This at first appeared to me to involve an impossibility, because water expands while freezing; and, therefore, it seemed to follow, that if a quantity of it were merely enclosed in a vessel with a moveable piston, and frozen, the motion of the piston, consequent on the expansion, being resisted by pressure, mechanical work would be given out without any corresponding expenditure; or, in other words, a perpetual source of mechanical work, commonly called a perpetual motion, would be possible. After farther consideration, however, the former conclusion appeared to be incontrovertible; but then, to avoid the absurdity of supposing that mechanical work could be got out of nothing, it occurred to me that it is necessary farther to conclude, that *the freezing point becomes lower as the pressure to which the water is subjected is increased.*

The following is the reasoning by which these conclusions are proved. Let there be supposed to be a cylinder, and a piston fitting water-tight to it, and capable of moving without friction. Let these be supposed to be formed of a substance which is a perfect non-conductor of heat; also, let the bottom of the cylinder be closed by a plate, supposed to be a perfect conductor, and to possess no capacity for heat. Now, to convert a given mass of ice into water without the expenditure of mechanical work, let this imaginary vessel be partly filled with air at 0° C., and let the end of it be placed in contact with an indefinite mass of water, a lake for instance, at the same temperature. Now, let the piston be pushed towards the bottom of the cylinder by pressure from some external reservoir of mechanical work, which, for the sake of fixing our ideas, may be supposed to be the hand of an operator. During this process the air in the cylinder would tend to become heated on account of the compression, but it is constrained to remain at 0° by being in communication with the lake at that temperature. The change, then, which takes place is, that a certain amount of work is given from the hand to the air, and a certain amount of heat is given from the air to the water of the lake. In the next place, let the bottom of the cylinder be placed in

contact with the mass of water at 0 , which is proposed to be converted into ice, and let the piston be allowed to move back to the position it had at the commencement of the first process. During this second process, the temperature of the air would tend to sink on account of the expansion, but it is constrained to remain constant at 0 by the air being in communication with the freezing water, which cannot change its temperature so long as any of it remains unfrozen. Hence, so far as the air and the hand are concerned, this process has been exactly the converse of the former one. Thus the air has expanded through the same distance through which it was formerly compressed; and, since it has been constantly at the same temperature during both processes, the law of the variation of its pressure with its volume must have been the same in both. From this it follows, that the hand has received back exactly the same amount of mechanical work in the second process as it gave out in the first. By an analogous reason it is easily shewn, that the air also has received again exactly the same amount of heat as it gave out during its compression; and, hence, it is now left in a condition the same as that in which it was at the commencement of the first process. *The only change which has been produced, then, is, that a certain quantity of heat has been abstracted from a small mass of water at 0 , and dispersed through an indefinite mass at the same temperature, the small mass having thus been converted into ice.* This conclusion, it may be remarked, might be deduced at once by the application, to the freezing of water, of the general principle developed by CARNOT, that no work is given out when heat passes from one body to another without a fall of temperature; or rather by the application of the converse of this, which of course equally holds good, namely, that no work requires to be expended to make heat pass from one body to another at the same temperature.

Next, to prove that the freezing point of water is lowered by an increase of the pressure to which the water is subjected:—Let a cylinder, of the same imaginary construction as that used in the foregoing demonstration, contain some air at 0° C. Let the bottom of the cylinder be placed in contact with the water of an indefinitely large lake, of which the temperature is above 0° by an infinitely small quantity; and let the air be subjected to compression by pressure applied by the hand to the piston. A certain amount of work is thus given from the hand to the air, and a certain amount of heat is given out from the air to the lake. Next, let the bottom of the cylinder be placed in communication with a small quantity of water at 0° , enclosed in a second imaginary cylinder similar in character to the first; and let this water be, at the commencement, subject merely to the atmospheric pressure. Let, however, resistance be offered by the hand to any motion of the piston of this second cylinder which may take place. Things being in this state, let the piston of the cylinder containing the air move back to its original position. During this process part of the heat of the air becomes latent on account of the increase of volume. Thus the temperature of the air, from being

above 0° , by an infinitely small quantity, instantly becomes absolutely 0° ; and afterwards, as the motion of the piston continues, the air absorbs heat from the mass of water in the second cylinder, part of the mass passing at the same time into the state of ice. Hence the whole mass expands; and therefore, on account of the resistance offered by the hand to the motion of the piston of the cylinder containing the mass, the internal pressure is increased, and a quantity of work, not infinitely small, is given out by the piston, and is received by the hand. Towards the end of this process, let the resistance offered by the hand gradually decrease till, just at the end (that is, when the piston of the air-cylinder has resumed its first position) it becomes nothing, and the pressure within the water-cylinder thus becomes again equal to that of the atmosphere. The temperature of the mass of partly frozen water must now be 0° , and the air in the other cylinder being in communication with this, must have the same temperature. The air is therefore, infinitely nearly at its original temperature, and it has its original volume. Hence it is now left in a state infinitely nearly the same as that in which it was at first. Farther, let the ice, which has been formed by the freezing of the water, be placed in contact with the lake till it melts, which it will really do since the lake is warmer than 0° , though only by an infinitely small quantity. Thus the mass of water is left in its original state, and it has been already shewn that the air is left infinitely nearly in its original state. Hence no work, except an infinitely small quantity, can have been absorbed or developed by any change on the air and water, which have been used. But a quantity of work not infinitely small has been given out by the piston of the water-cylinder to the hand: and therefore an equal quantity* of work must have been given from the hand to the air-piston, as there is no other way in which the work developed could have been introduced into the apparatus. Now, the only way in which this can have taken place is by the air having been colder, while it was expanding in the second process, than it was while it was undergoing compression during the first. Hence it was colder than 0° during the course of the second process; or, in other words, *while the water was freezing, under a pressure greater than that of the atmosphere, its temperature was lower than 0° .*

The fact of the lowering of the freezing point being thus demonstrated, it becomes desirable, in the next place, to find what is the freezing point of water for any given pressure. The most obvious way to determine this would be by direct experiment with freezing water. I have not, however, made any attempt to do so in this way. The variation to be appreciated is extremely small, so small, in fact, as to afford sufficient reason for its existence never having been observed by any experimenter. Even to detect its existence, much more to arrive at its exact amount by direct experiment, would require very delicate apparatus which would

* In saying "an equal quantity" I, of course, neglect infinitely small quantities in comparison to quantities not infinitely small.

not be easily planned out or procured. Another, and a better, mode of proceeding has, however, occurred to me: and by it we can deduce, from the known expansion of water in freezing, together with data founded on the experiments of REGNAULT on steam at the freezing point, a formula which gives the freezing point in terms of the pressure; and which may be applied for any pressure, from nothing up to many atmospheres. The following is the investigation of this formula:—

Let us suppose that we have a cylinder of the same imaginary construction as that of the one described at the commencement of this paper; and let us use it as an ice-engine analogous to the imaginary steam-engine conceived by CARNOT, and employed in his investigations. For this purpose, let the entire space enclosed within the cylinder by the piston be filled at first with as much ice as would, if melted, form rather more than a cubic foot of water, and let the ice be subject merely to one atmosphere of pressure, no force being applied to the piston. Now, let the following four processes, forming one complete stroke of the ice-engine be performed.

Process 1. Place the bottom of the cylinder in contact with an indefinite lake of water at 0 , and push down the piston. The effect of the motion of the piston is to convert ice at 0 into water at 0 , and to abstract from the lake at 0° the heat which becomes latent during this change. Continue the compression till one cubic foot of water is melted from ice.

Process 2. Remove the cylinder from the lake, and place it with its bottom on a stand which is a perfect non-conductor of heat. Push the piston a very little farther down, till the pressure inside is increased by any desired quantity which may be denoted, in pounds on the square foot, by p . During this motion of the piston, since the cylinder contains ice and water, the temperature of the mixture must vary with the pressure, being at any instant the freezing point which corresponds to the pressure at that instant. Let the temperature at the end of this process be denoted by $-t^\circ \text{C}$.

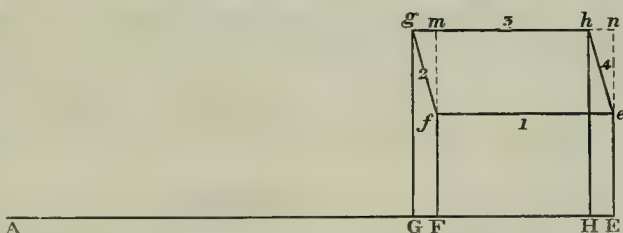
Process 3. Place the bottom of the cylinder in contact with a second indefinitely large lake at $-t$, and move the piston upwards. During this motion the pressure must remain constant at p above that of the atmosphere, the water in the cylinder increasing its volume by freezing, since, if it did not freeze, its pressure would diminish, and therefore its temperature would increase, which is impossible, since the whole mass of water and ice is constrained by the lake to remain at $-t$. Continue the motion till all the heat has been given out to the second lake at $-t$, which was taken in during Process 2, from the first lake at 0 .*

* This step, as well as the corresponding one in CARNOT's investigation, it must be observed, involves difficult questions, which cannot as yet be satisfactorily answered, regarding the possibility of the absolute formation or destruction of heat as an equivalent for the destruction or formation of other agencies, such as mechanical work; but, in taking it, I go on the almost universally adopted supposition of the perfect conservation of heat.

Process 4. Remove the cylinder from the lake at $-t^{\circ}$, and place its bottom again on the non-conducting stand. Move the piston back to the position it occupied at the commencement of Process 1. The temperature and pressure, during this process, must vary with one another, as they did in Process 2. Also, since as much heat has been given out as was taken in; and since the volume is the same as at the commencement of Process 1, the physical state of the mass contained in the cylinder must be now in every respect the same as it was at that time.

By representing graphically in a diagram the various volumes and corresponding pressures, at all the stages of the four processes which have just been laid down, we shall arrive, in a simple and easy manner, at the quantity of work which is developed in one complete stroke by the heat which is transferred during that stroke from the lake at 0° to the lake at $-t$. For this purpose, let E be the position of the piston at the beginning of Process 1; and let some distance, such as EG, represent its

stroke in feet, its area being made a square foot, so that the numbers expressing, in feet, distances along EG may also express, in cubic feet, the changes in the contents of A



the motion of the piston. Now, when 1.087 cubic feet of ice are melted, one cubic foot of water is formed. Hence, if EF be taken equal to .087 feet, F will be the position of the piston when one cubic foot of water has been melted from ice, that is, the position at the end of Process 1, the bottom of the cylinder being at a point A distant from F by rather more than a foot. Let ef be parallel to EF, and let Ee represent one atmosphere of pressure; that is, let the units of length for the vertical ordinates be taken such that the number of them in Ee may be equal to the number which expresses an atmosphere of pressure. Also let gh be parallel to EF, and let fm represent the increase of pressure produced during Process 2. Then the straight lines ef and gh will be the lines of pressure for Processes 1 and 2; and for the other two processes, the lines of pressure will be some curves which would extremely nearly coincide with the straight lines fg and he . For want of experimental data, the nature of these two curves cannot be precisely determined; but, for our present purpose, it is not necessary that they should be so, as we merely require to find the area of the figure $efgh$, which represents the work developed by the engine during one complete stroke, and this can readily be obtained with sufficient accuracy. For, even though we should

adopt a very large value for fm , the change of pressure during Process 2, still the changes of volume gm and hn in Process 2 and Process 4 would be extremely small compared to the expansion during the freezing of the water; and from this it follows evidently that the area of the figure $efgh$ is extremely nearly equal to that of the rectangle $cfmn$, but fe is equal to FE , which is $\cdot 087$ feet. Hence the work developed during an entire stroke is $\cdot 087 \times p$ foot-pounds. Now this is developed by the descent from 0 to $-t$ of the quantity of heat necessary to melt a cubic foot of ice; that is, by 4925 thermic units, the unit being the quantity of heat required to raise a pound of water from 0 to 1 centigrade. Next we can obtain another expression for the same quantity of work; for, by the tables deduced in the preceding paper from the experiments of REGNAULT, we find that the quantity of work developed by one of the same thermic units descending through one degree about the freezing point, is 4.97 foot-pounds. Hence, the work due to 4925 thermic units descending from 0 to $-t$ is $4925 \times 4.97 \times t$ foot-pounds. Putting this equal to the expression which was formerly obtained for the work due to the same quantity of heat falling through the same number of degrees, we obtain

$$4925 \times 4.97 \times t = \cdot 087 \times p.$$

Hence,

$$t = \cdot 00000355 p. \quad . \quad . \quad . \quad . \quad (1.)$$

This, then, is the desired formula for giving the freezing point $-t$ centigrade, which corresponds to a pressure exceeding that of the atmosphere by a quantity p , estimated in pounds on a square foot.

To put this result in another form, let us suppose water to be subjected to one additional atmosphere, and let it be required to find the freezing point. Here $p =$ one atmosphere $= 2120$ pounds on a square foot; and, therefore, by

$$(1.) \quad t = \cdot 00000355 \times 2120.$$

or

$$t = \cdot 0075.$$

That is, the freezing point of water, under the pressure of one additional atmosphere, is $-\cdot 0075^\circ$ centigrade; and, hence, if the pressure above one atmosphere be now denoted in atmospheres,* as units by n , we obtain t , the lowering of the freezing point in degrees centigrade, by the following formula—

$$t = \cdot 0075 n. \quad . \quad . \quad . \quad . \quad (2.)$$

* The atmosphere is here taken as being the pressure of a column of mercury of 760 millimetres; that is 29.92, or very nearly 30 English inches.

XXXVIII.—*On the Gradual Production of Luminous Impressions on the Eye, and other Phenomena of Vision.* By WILLIAM SWAN, F.R.S.E.

(Read March 19, 1849.)

It is well known that a luminous object is seen for some time after its light has ceased to fall on the retina; but less attention seems to have been paid to the fact, that light requires a certain time to produce its full impression on the eye. Accordingly, while it is stated in most treatises on optics, that the sensation of vision continues after the action of light has ceased, only a few writers have mentioned that the total effect of light on the eye is not produced instantaneously, but that a certain time is required for its complete development.

The merit of having first noticed this phenomenon of vision is probably due to Lord BACON, who observes, that notwithstanding the rapidity of the act of vision, a certain time is required for its exercise, which is proved by certain objects, such as a musket-ball, being invisible on account of the velocity of their motion. For the flight of the ball, he remarks, is too swift to allow an impression of its figure to be conveyed to the sight.*

While succeeding writers have devoted much attention to other departments of the physiology of vision, they have not, so far as I am aware, added a single fact to our knowledge of this part of the subject, which remains, therefore, precisely as it was left by Lord BACON.†

* "At in visu (cujus actio est perniciosissima) liquet etiam requiri ad eum actuandum momenta certa temporis: idque probatur ex iis, quae propter motus velocitatem non cernuntur; ut ex latatione pilae ex sclopeto. Velocior enim est praetervolatio pilae, quam impressio speciei ejus quae deferri poterat ad visum."—(*Novum Organum*, lib. ii., Aph. xlv. *Bacon's Works*, vol. i., p. 370. Lond. 1711.)

† This appears from the following passages, which will be found to contain little more than a repetition of Lord BACON's statement:—

"Il est un fait auquel on a généralement accordé peu d'attention, quoiqu'il ait été remarqué (voyez *Essai d'un Cours Élémentaire et Général des Sciences Physiques*, par M. BEUDANT: Partie Physique, p. 489 de la 3^{me} édition), c'est que les impressions directes exigent un certain temps pour se développer sur la rétine. Pour se convaincre de la réalité de ce fait, qui devait naturellement se prévoir *a priori*, il suffit de se rappeler qu'un objet qui passe très rapidement devant l'œil, ne se voit pas, ou s'aperçoit à peine. On peut encore prouver la chose par l'expérience suivante. Si l'on fait mouvoir circulairement, devant un fond noir, un petit morceau de papier blanc, avec une vitesse telle que l'anneau apparente qu'en résulte présente une teinte parfaitement uniforme et tranquille, cet anneau ne paraîtra pas blanc, mais gris. Or il suit de l'uniformité de la teinte, que pendant le petit intervalle de temps qui sépare deux passages successifs de l'objet au même point, l'impression ne décroît pas d'une quantité sensible: il faut donc nécessairement admettre que cette impression n'est pas blanche, comme celle qui est produite par l'objet en repos, mais qu'elle est grise, c'est-à-dire d'une blancheur imparfaite, ou enfin qu'à raison du temps extrêmement court que l'objet emploie à passer devant l'œil, il ne produit qu'une impression incomplète. Il est inutile d'ajouter qu'on obtiendra des résultats analogues en employant un objet d'une couleur quelconque: toujours l'anneau paraîtra plus sombre que l'objet en repos. L'éclat de l'anneau sera d'ailleurs d'autant moins éloigné de celui de l'objet en

Before I was aware that any one had noticed the gradual action of light on the eye, my attention was accidentally directed to that subject about eighteen months ago, by observing that the light of the sky seen immediately over a ball in its descent through the air, seemed less bright than at those parts of the retina where the action of the light had not been interrupted by the passage of the dark body. It immediately occurred to me, that this appearance was caused by the portion of the retina over which the image of the ball had passed, not having had time to be fully impressed with the light of the sky at the instant when the passage of the ball again exposed it to the action of that light.* Such an observation as this does not admit of easy repetition, but a more convenient method of exhibiting the gradual production of luminous impressions will be afterwards described.

It may be necessary here to anticipate an objection to the supposition, that light requires a sensible time to produce its full effect on the retina, founded on the observations of Professor WHEATSTONE, whose experiments prove, that "the light of electricity of high tension has a less duration than the millionth part of a second;" and that "the eye is capable of perceiving objects distinctly which are presented to it during the same small interval of time."†

It is obvious, however, that these statements are perfectly consistent with the gradual action of light on the eye. For, although light may produce a certain effect

repos, ou, en d'autres termes, l'impression approchera d'autant plus d'être complète, que cet objet aura plus de largeur, et que par suit il emploiera, dans son mouvement, un temps moins court à passer devant l'œil : ainsi l'expérience, que nous venons de décrire, conduit de plus à cette conséquence facile à prévoir que le développement de l'impression directe est progressive quoique très rapide."—(*Essai d'une Théorie Générale comprenant l'ensemble des Apparences Visuelles*, &c. par J. PLATEAU, p. 53. *Nouveaux Memoires de l'Academie Royale des Sciences et Belles Lettres de Bruxelles*, tome viii., 1834.)

A statement almost identical with this will be found in PLATEAU *sur la Persistance des Impressions de la Rétine*. *Supplément au Traité de la Lumière* de Sir J. F. W. HERSCHEL. Par A. QUETELET. p. 474, 1833. See also MULLER's *Physics*, p. 274. London, 1847.

The following is the passage in BEUDANT *Cours de Physique*, to which M. PLATEAU refers :—"C'est aussi parce que l'impression d'un objet sur notre œil ne se fait pas instantanément, que nous ne pouvons apercevoir un corps qui se meut avec une extrême vitesse. Ainsi par exemple, un boulet de canon lancé par une bouche à feu, est invisible pendant une grande partie de son mouvement, parce qu'il ne reste pas assez de temps dans un même lieu, pour qu'on ait celui de l'apercevoir."

M. PLATEAU observes, in the passage which has just been quoted, that it was easy to foresee *a priori* that the development of the impression of light on the eye is progressive, although very rapid. With reference to this opinion, while it may be admitted that it is quite natural to suppose that the action of light on the eye is not absolutely instantaneous ; yet, certainly, no one would be entitled to conclude *a priori* that a sensible time is required to produce impressions on the eye. I have, therefore, much satisfaction in availing myself of the present opportunity of directing attention to Lord BACON's prior claim to the merit of pointing out the curious and interesting fact, that light requires an appreciable time to produce visual impressions on the eye.

* It may be supposed that a different explanation of this effect might be afforded by the persistence of the impression of the image of the ball on the eye. That this explanation is identical with that given above, is evident from the image of the ball when seen projected upon the sky, being sensibly black. For, since blackness is the negation of light, the persistence of a black impression is but a want of light on that portion of the retina where the impression is perceived ; and the existence of such an impression, or a want of luminosity after the eye is fully exposed to light, clearly proves that its action on that organ is not instantaneous.

† Philosophical Transactions, 1834, p. 591.

on that organ in the millionth part of a second, it by no means follows that this is its full effect; and thus, while the electric spark renders objects distinctly visible which are seen for less than the millionth part of a second, it may still be true that the apparent brightness of those objects would increase if the duration of the light could be prolonged.

Having found only very brief and general references, to the gradual action of light on the eye, in any authors to whose works I have had access, I resolved to investigate the subject experimentally; and the object of this paper is to describe a series of experiments undertaken for the purpose of ascertaining the connexion between the brightness of the impression produced by light on the retina, and the time during which it acts on the eye.*

Before entering upon the narrative of my experiments it may be proper to premise, that if, in some cases, I seem to assume that the results obtained by experiments on my own eyes, are to be regarded as universal phenomena, I do so merely to avoid circumlocution; and I believe I may plead the example of most writers who describe experiments on vision in justification of such an apparent assumption.

In several cases, however, some of which will be afterwards noticed, the experiments have been witnessed by others, whose concurrent testimony has proved that the results were not dependent upon any idiosyncrasy of vision on my part.

I may also have made use of expressions which seem to involve the assumption that the brightness with which a luminous object is seen at any instant, is the same as the apparent brightness of its image on the retina at that instant; or, in other words, that the impression of light on the retina is perceived by the mind instantaneously. Such expressions, however, are employed simply for the sake of brevity. The principal object of my investigation is to determine the brightness of an impression made on the retina by a light of a given intensity, acting for a given time; and it will be found in the sequel that the method I have devised for measuring the time during which the light acts, and the intensity of the resulting impression, does not depend for its accuracy on the settlement of the question, whether or not the impressions of light on the retina are instantaneously perceived by the mind.

I. *Method of Observation.*

In order to examine the phenomena presented by luminous impressions of short duration, I made use of the following method of observation. If a disc of

* Additional proof that almost no attention has hitherto been paid to this subject, may be derived from the fact, that no notice is taken of it in MÜLLER's *Physiology*, London 1839, nor in the Supplement to that work by BALY, London 1848. M. PLATEAU also observes: "Personne n'a essayé de mesurer le temps nécessaire à la production complète de l'impression." PLATEAU *Sur la Persistance des Impressions*. (*Supplément au traité de la Lumière de Sir J. F. W. HERSCHEL. Par A. QUETELET*, p. 474. 1833.)

pasteboard (see Fig. 4, Plate XII.), or other convenient material, having a portion of a sector $A B C D$, cut out from its circumference, be made to revolve, in a plane perpendicular to the line of vision, between the eye and a luminous object, the object may be placed so as to be seen through the sector at each revolution of the disc. In this manner a succession of luminous impressions will be obtained; and the time during which the light acts on the eye at each impression will depend partly on the velocity of the rotation of the disc, and partly on the ratio of the arc of the sector to the whole circumference.

Let $A B G$ (Fig. 1) represent the disc, $A C B$ the sector cut out of it, and $E D$ the section, by the plane of the disc, of the pencil of rays proceeding from the luminous object to the eye. Then, if $\theta =$ the angle $A C B$, and $t =$ the time in which the disc makes one revolution; the time in which the line $A C$ revolves from its present position to the position $B C$ will evidently be $\frac{t \theta}{2 \pi}$.

Now, if a ray proceeding from any point in the luminous surface is just emerging at F , the point from which it emanates will remain visible until $A C$ comes to the position $B C$, or during the time $\frac{t \theta}{2 \pi}$. Since this is obviously true of any other element of the surface, it follows that every part of the surface remains visible for the same time.

The interval of time between the first appearance of the object and its final disappearance is obviously greater than that during which each element of its surface is visible. For, if E and D be sections of the rays proceeding from the points in the luminous surface which are first and last visible, some part of the surface will be seen during the interval of time between the instant in which $C B$ coincides with $C E$, and that in which $A C$ coincides with $C D$, or during the time in which the line $A C$ revolves through the sum of the angles $E C D$, $A C B$. Denoting $E C D$ by λ , this time will be $\frac{t (\theta + \lambda)}{2 \pi}$.

If the luminous object is circular, and the axis of the pencil of rays proceeding to the eye is perpendicular to the plane of the disc, putting

$s =$ the radius of the luminous circle,

$d =$ its distance from the eye,

$d' =$ the distance of the disc from the eye,

$c =$ the distance of the axis of the pencil of rays from the centre

of the disc, it will be found that $\lambda = 2 \sin^{-1} \frac{s d'}{c d}$; and therefore the time which elapses between the first appearance and the final disappearance of the luminous circle, is

$$\frac{t}{2 \pi} \left(2 \sin^{-1} \frac{s d'}{c d} + \theta \right)$$

From this expression it will be seen that the time during which the eye re-

C' D'

Fig. 2

K

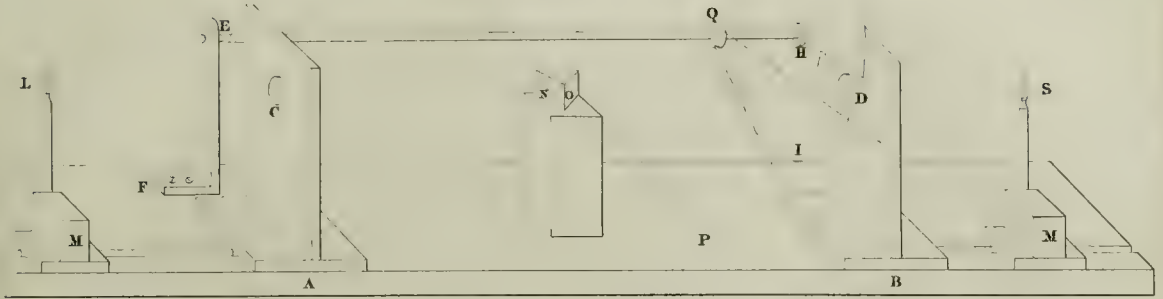


Fig. 4

E
F G H

a b
d c

B A
C D

Fig. 5

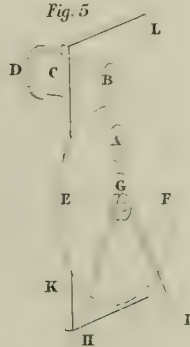


Fig. 1

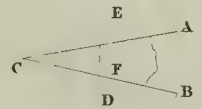
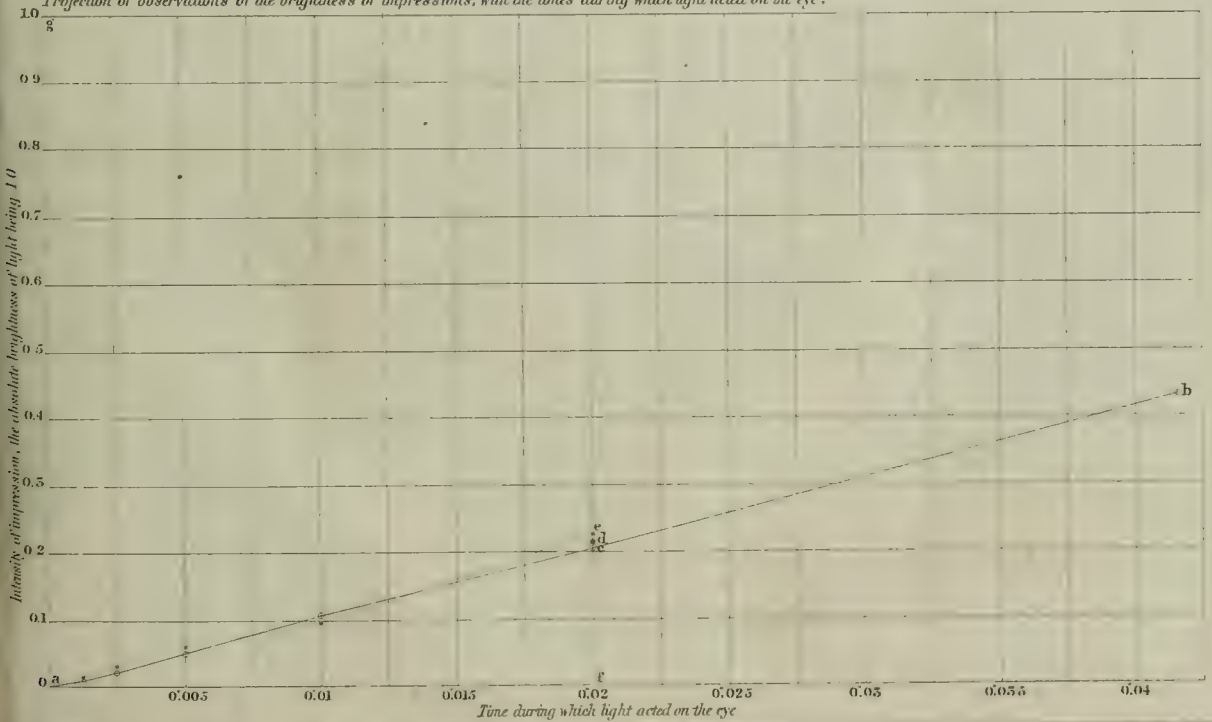


Fig. 3

Projection of observations of the brightness of impressions, with the times during which light acted on the eye.





ceives light at each revolution of the disc may be varied, by altering the diameter of the luminous circle, or its distance from the eye, or from the disc, or by changing the distance of the pencil of transmitted rays from the centre of the disc. But in the experiments to be afterwards described, these elements remained constant, and the effect was modified only by altering the angles of the sectors, or the rate of revolution of the discs. It may, however, be proper to observe that, as the time during which each element of the surface remains visible is independent of the magnitude of the luminous object; so also, as might be anticipated, the apparent brightness of the surface is independent of the ratio which the portion visible at once through the sector bears to the whole area. For, in repeating the same experiments with circular luminous objects of different diameters, while the angle of the sector and the velocity of the disc were constant, it was found that the apparent brightness of the luminous circle was not sensibly affected by varying its diameter.

In order to compare the brightness of the impressions produced by light seen through the sectors of revolving discs, in the manner now described, with its brightness when seen by uninterrupted vision, the following arrangement was devised, which, for the sake of convenient reference, may be termed a *selaometer* (from *σελας*, *brightness*), to indicate its use in measuring the brightness of luminous impressions. This apparatus, represented in Fig. 2, is supported on a stout plank A B. A C and B D are screens with circular apertures C, D, an inch in diameter, to which are fitted pieces of ground glass, cut from the same plate, in order to secure similarity of surface. The apertures C, D, are illuminated by the gas-burners, L, S,* which are supplied by flexible tubes, so that their distances from the screens can be varied at pleasure by sliding their supports M, M, along the plank A B, in a groove cut in it for that purpose. An axis E H, carrying the disc I K, revolves between conical points in supports, one of which E F is seen in the figure. This axis is put in motion by a band passing over the pulley Q, and over a wheel driven by means of a winch; and it is made of sufficient length to admit of a second disc revolving in front of the screen A C simultaneously with the disc I K. The brightness of the apertures in the screens is observed by means of a rectangular prism of glass N O, placed half-way between them, with its faces inclined at angles of 45° to the line C D joining their centres. By this means the light passing through the apertures, and dispersed by the ground-glass, is reflected from the faces of the prism to the eye at P, and the images of the apertures being seen in apparent contact, as represented at C', D', their relative brightness can be compared with great nicety.† The driving-wheel is made

* The gas-light used in all the experiments described in this paper, was that of coal-gas burned by a No. 2 swallow-tail jet. It will be seen that the numerical results, afterwards obtained, do not depend on the absolute brightness of this light.

† Screens covered with black paper, which are not represented in the figure, were used to protect the eyes from the action of extraneous light, and also to intercept any rays, whose influence might have otherwise affected the accuracy of the experiments.

to revolve in time with a metronome adjusted to beat seconds; so that, by ascertaining the number of revolutions which the pulley, Q, makes during each revolution of the driving-wheel, the time of a single revolution of the disc is readily determined. This time multiplied by the ratio of the arc of the sector to the whole circumference of the disc, gives the length of each luminous impression. Thus, if the driving-wheel revolves m times in a second, and the disc n times during each revolution of the driving-wheel, the time of revolution of the disc, expressed in seconds, is $\frac{1}{m n}$; and if θ be the angle of the sector, the time during which the eye receives light from each element of the luminous surface at every revolution of the disc is $\frac{\theta}{2 m n \pi}$.

In order to compare the brightness of the aperture D, seen by uninterrupted vision, with its brightness as seen during the revolution of the disc, the illumination of the apertures is first made equal by varying the distance of the flame L from the screen A C, until both apertures seen by reflexion in the prism appear equally bright. When the disc is then made to revolve, the apparent brightness of the aperture D immediately diminishes, and the equality of the brightness of the apertures is again restored by withdrawing the light L, to a greater distance from the screen A C.* Since the distance of the light S, from the screen B D, remains constant during this operation, the ratio of the apparent brightness of the aperture D, seen by uninterrupted vision, to its apparent brightness during the revolution of the disc, will be that of the square of the distance of the light L, from the screen A C, before the disc has begun to revolve, to the square of its distance during the revolution of the disc. For since the intensity of the light incident on the face O, of the prism is constant, we may conceive that face of the prism as the source of light of a constant intensity. Let b_1 = the apparent brightness of this light seen by uninterrupted vision; b_2 = its apparent brightness seen during the revolution of the disc. Then if i = the intrinsic brightness of the flame L, d_1 and d_2 its distances from the screen before and during the revolution of the disc, a , the ratio of the brightness of the light transmitted by the glass in the aperture C', to that incident upon it, and r , the ratio of the brightness of the reflected light to the light incident upon the face N, of the prism; the apparent brightness of the aperture C, when the light L, is at the distance d_1 , will be $\frac{a r i}{d_1^2}$, and at the distance d_2 its apparent brightness will be $\frac{a r i}{d_2^2}$. Now since the apparent brightness of both apertures is made equal, we have

* This is conveniently done by means of a pulley and cord. When the apertures are being made equally bright before the disc is made to revolve, it is necessary that the aperture D should be fully exposed. Where the sector is too narrow to admit of the whole aperture being seen at once, another sector is cut in the disc for this purpose, which admits of being closed by a slider of pasteboard before the disc is made to revolve.

$$\frac{b_1}{b_2} = \frac{\frac{a r i}{d_1^2}}{\frac{a r i}{d_2^2}} = \frac{d_2^2}{d_1^2}$$

It has here been assumed that the brightness of the gas-flame remains constant during the experiment, a condition which is not fulfilled in practice, owing to the variable pressure of the gas in the pipes. It is probable, however, that the brightness of both flames will vary nearly in the same proportion, so that the distances necessary to equalise the apparent brightness of the apertures in the screens will remain almost unaltered; and it is obvious also, that any residual error, arising from a gradual change in the brightness of the flames, will be nearly eliminated by taking the mean of a series of observations immediately succeeding each other, and conducted in the manner now described.

II. *Proof of the Gradual Action of Light on the Eye.*

If a disc, with a sector of a small angle, is made to revolve between the eye and a luminous object, a flash of light is seen at each revolution; but as the velocity of rotation increases, the brightness of the flashes diminishes, which shews that the apparent brightness of a luminous object diminishes as the time during which it is visible becomes shorter. A similar result is obtained by placing two discs with sectors of different angles before the screens of the selascope, and observing the relative intensity of the simultaneous flashes of light when the discs are made to revolve. It will always be found that although the apertures in the screens are equally bright, when seen by uninterrupted vision, the disc whose sector has the greater angle produces the brighter flash. Now as both discs revolve with the same velocity, the length of the luminous impressions will be proportional to the angles of the sectors; so that, by this experiment also, the apparent brightness of the light is shewn to increase with the time during which it continues to act upon the eye. A convenient *selascope*, which exhibits this phenomenon in a striking manner may be made by causing a disc with a sector of the form E F G H (Fig. 4), to revolve before a luminous aperture. The flash produced by the wide part of the sector E F, greatly exceeds in brightness that produced by the narrow part G H.

The experiment of causing a disc, with a sector cut in it, to revolve before a luminous aperture, also affords a simple proof of the duration of luminous impressions on the retina. At about seven revolutions in a second the luminous impression becomes continuous, so that the aperture always appears visible, even during the dark interval between the successive passages of the sector between it and the eye; but a considerably higher velocity, about twenty revolutions in a second, is required in order to produce a sensibly uniform impression. These velocities are not meant to be stated with great exactness. They were obtained by experi-

ments with discs having sectors varying from 60° to $7^\circ 30'$, and did not appear to differ very sensibly in different cases; but it seems probable that the velocity required to produce a continuous or a uniform impression should be sensibly affected by greatly altering the angle of the sector.

III. *Effect of combined Luminous Impressions on the Eye.*

So long as the rotation of the disc is so slow as to allow each flash to be seen separately, the brightness of the flashes diminishes as the velocity of rotation increases, until, at about twenty revolutions in a second, the flashes become blended into a nearly uniform impression. Whenever this takes place, no farther increase of the velocity of the disc diminishes the intensity of the impression in the smallest perceptible degree. This result is evidently produced by the increased number of luminous impressions in a given time compensating for their diminished intensity; but it is remarkable that the one effect should so exactly compensate for the other. Having found that this compensation took place at velocities varying from twenty to forty revolutions in a second, I was anxious to ascertain whether it continued unimpaired at higher velocities. For this purpose a disc of pasteboard $4\cdot5$ inches in diameter, with a sector of $2^\circ 30'$ cut out of its margin, was fitted to the axle of a clockmaker's wheel-cutting engine. It was found by a previous careful trial, that the disc made exactly 100 revolutions for each revolution of the driving-wheel; and as the latter, at its greatest velocity, made thirteen revolutions in ten seconds, the disc ought to have revolved 130 times in a second. But to avoid the chance of errors arising from the driving-bands slipping at so high a velocity, I availed myself of Professor WHEATSTONE's ingenious method of ascertaining the velocity of a rapidly-revolving axle, described in his paper in the Philosophical Transactions for 1834, to which I have already referred. This consisted in observing the pitch of the note produced by the rapid percussion of a pin, fixed in the revolving axle, upon a piece of paper held in contact with it. The highest note produced during the experiment was rather less than an octave below C of the tenor clef, which corresponds to above 128 vibrations in a second. This result agrees almost exactly with the calculation founded on the observed rotation of the disc at low velocities; and it may, therefore, be concluded, that the disc made above 128 revolutions in a second. Since the arc of the sector was $\frac{1}{144}$ of the circumference, the light from a luminous point placed behind the disc would, at each revolution, act on the eye for only $\frac{1}{18432}$ of a second. A lighted candle being placed behind the disc, the machine was put in motion, and the velocity gradually increased until the driving-wheel made thirteen revolutions in ten seconds, after which it was allowed to come to rest spontaneously. It was found that the brightness of each successive flash diminished as the velocity of the disc increased, until the impression on the eye became uniform, at a velocity of

about twenty revolutions in a second. After this no increase of velocity, up to 128 revolutions in a second, produced the slightest farther diminution of the apparent brightness of the light; and again, as the speed diminished the light continued uniformly bright, until the motion became so slow as to allow the eye to perceive the impressions separately, after which they gradually increased in intensity until the disc stopped. The same experiment was repeated, substituting for the flame of the candle an illuminated aperture in a screen, covered with tissue paper. The apparent brightness of this aperture, when the disc revolved, was compared, in the manner already described at p. 585, with that of another similar aperture seen by uninterrupted vision, and the result was perfectly in accordance with that obtained in the previous experiment. The same phenomena were also observed when a disc with a sector of 30° was substituted for that with a sector of $2^\circ 30'$.*

A similar result was obtained by varying the form of the experiment, in the following manner:—Two discs, one with a sector of 30° , and the other with two sectors of 15° , such as A B C D, *abcd* (Fig. 4), at opposite extremities of its diameter, were placed in the selaometer, and made to revolve simultaneously. In both discs the ratio of the duration of the flashes to that of the dark intervals, is obviously the same; but when the discs revolve simultaneously, for each flash produced by the disc with the sectors of 30° , there are two flashes of half the duration produced by the disc with two sectors of 15° . The disc with two sectors of 15° , revolving at a given velocity, is, therefore, precisely identical in its action to the disc with a single sector of 30° revolving at double the velocity. The apertures in the screen being made equally bright before the discs revolved, the equality of their brightness remained unaltered when the discs revolved so rapidly as to produce a uniform impression upon the eye. In the same manner, the brightness of the apertures remained equal when any disc, in the first part of the following table, revolved simultaneously with the corresponding disc in the second part, at such a velocity as to produce a uniform impression on the eye.

Number of Sectors.†	Angle of Sectors.	Number of Sectors.	Angle of Sectors.
2	15°	1	30°
4	$7^\circ 30'$	1	30°
2	$7^\circ 30'$	1	15°
2	30°	1	60°
3	30°	1	90°
4	30°	1	120°

In all these cases, the duration of each flash was inversely as the number of

* I was enabled to make this experiment by the kindness of Mr ALEXANDER BRYSON, who, along with Mr JOHN TURNBULL, W.S., witnessed the results above described.

† In all experiments in which the discs had more than one sector, the sectors were arranged round the circumference at equal distances from each other.

flashes in a given time; and these experiments, therefore, confirm the result obtained by varying the velocity of a disc with a sector of a given angle. We may, therefore, infer,

1st, That if the number of flashes, in a given time, succeeding each other so rapidly as to produce a uniform impression on the eye is inversely as the duration of each flash, their aggregate effect on the eye will be constant.

2dly, This compensation of the diminished intensity, by the increased frequency of the flashes, is independent of the interval of time between each impression, within the limits of the observations; that is, with intervals varying from $\frac{1}{20}$ th to $\frac{1}{128}$ th of a second.

3dly, The effect is also independent of the ratio of the duration of the luminous to that of the dark intervals.

It is thus shewn that, after a uniform impression is produced, increasing the number of flashes in a given time, compensates for their diminished intensity. This naturally leads to the inquiry, at what rate would the brightness of the resulting impression increase with the number of flashes in a given time, supposing the intensity of the flashes to remain constant? In order to ascertain the connexion between the number of flashes of a given intensity in a given time, and the intensity of their combined effect on the eye, I made the following experiments:—

1. Two discs, one with a sector of 15° , and the other with two equidistant sectors of 15° , as A B C D, *abcd*, Fig. 4, were placed in the selaometer, and the screens were equally illuminated by carefully adjusting the distances of two similar spermaceti candles placed behind them. When the discs were made to revolve so rapidly as to produce a uniform impression, a second candle placed behind the screen, whose disc had a single sector of 15° , restored the equality of the apparent brightness of the apertures, and a similar result was obtained when discs with single sectors of 30° and $7^\circ 30'$, were compared with discs having two sectors of the same angles.

2. To vary the experiment, the flame of a gas burner was placed 10 inches behind the screen, whose disc had two sectors of 30° , and a similar flame was adjusted behind the screen, whose disc had a single sector of 30° , so as to illuminate the apertures in the screens equally. When the discs revolved rapidly, the flame behind the disc with two sectors was withdrawn to 14.1 inches ($10\sqrt{2}$), so as to halve the intensity of the light incident on the screen, and the illumination of the apertures appeared to be perfectly equal. In like manner, when the light was first placed at 20 inches from the screen, after the discs revolved, the screens seemed equally illuminated when it was withdrawn to 28.2 inches.

3. A disc with a single sector of $7^\circ 30'$, and another with two sectors of the same angle, were placed in the selaometer. The illumination of the screens was then made equal by adjusting the distance of the light behind the disc with two

sectors. The distance of this light was noted, and, when the discs revolved rapidly, the light was withdrawn until the apertures seemed again equally bright. The degree in which the brightness of the light required to be diminished, by withdrawing the flame from the screen, in order to equalise the brightness of the impressions produced by the two discs, was taken as a measure of the ratio of the brightness of those impressions when the screens were equally illuminated. Four experiments were made, and, as formerly, putting d_1 , and d_2 to denote the distances of the light from the screen before and after the disc had been made to revolve, and ρ to denote the ratio of the apparent brightness of the apertures seen during the revolution of the discs when their illumination was actually equal, the mean values of those quantities was found to be

$$d_1=13.82; d_2=20.13; \rho=2.122.$$

In a second set of four experiments,

$$d_1=4.075; d_2=5.95; \rho=2.132.$$

4. When a disc with three equidistant sectors of $7^\circ 30'$ was compared with a disc having a single sector of $7^\circ 30'$, the mean of four experiments gave

$$d_1=14.15; d_2=24.175; \rho=2.920.$$

In other four experiments,

$$d_1=4.1; d_2=7.15; \rho=3.041.$$

5. A disc with two sectors of 30° , compared four times with a disc having a single sector of 30° , gave

$$d_1=4.05; d_2=6.1; \rho=2.269.$$

and a second set of four experiments,

$$d_1=13.95; d_2=20; \rho=2.056.$$

6. A disc with four sectors of 30° , compared four times with a disc having a single sector of the same angle, gave

$$d_1=3.95; d_2=7.925; \rho=4.026,$$

and a second trial of four experiments,

$$d_1=14.1; d_2=29.4; \rho=4.348.$$

In all these experiments, the discs revolved so rapidly, as to produce a uniform impression on the eye, but the equality of illumination, when once obtained, was not affected by increasing the velocity; and from the variation of the quantity d_1 in the different experiments, it will be seen that the results are independent of the intrinsic brightness of the light incident on the screen.

The following table exhibits the mean values deduced from experiments No. 3 to No. 6 inclusive, and shews that the brightness of the impression produced by rapidly succeeding flashes of light of a given intensity, is sensibly proportional to the number of flashes in a given time.

Number of Flashes in a Second.	Brightness of Impression.
20	1.000
40	2.141
60	2.980
80	4.184

In these experiments it is assumed, that when the light is withdrawn from the screen, so as to diminish the intensity of its illumination, the brightness of a flash of short duration will be diminished in the same ratio. This will only be true, provided lights of different intensity produce impressions of proportional intensity in equal times; but if it be afterwards proved, independently of these observations, that such is really the case, the conclusion which has now been drawn from the experiments will be perfectly correct. In order to avoid this assumption, the brightness of the impressions produced by the revolution of the different discs was next compared with the impression of uninterrupted light.

1. A disc, with a single sector of 30° , was placed in the selaometer, with a gas-flame at a constant distance of 6 inches from its screen. In order to render the illumination of the screens equal, the other light had to be placed at a distance of 5.9 inches from the second screen. When the disc revolved 20 times in a second, the latter light was gradually withdrawn until the apertures in the screens again appeared equally bright. The distance of that light was now found to be 22.2 inches. This experiment, eight times repeated, gave the following mean values:—

$$d_1 = 5.78; d_2 = 22.21; b_1 = 0.06773.$$

where b_1 denotes the brightness of the impression produced during the revolution of the disc. In like manner, a disc, with two sectors of 30° , treated in precisely the same manner, gave the following mean values of eight trials:—

$$d_1 = 5.61; d_2 = 15.22; b_2 = 0.1359.$$

b_2 denoting the brightness of the impression produced by the revolution of this disc. From this experiment, the ratio of the brightness of the impressions produced by the revolution of the two discs, or $\frac{b_2}{b_1}$ will be found to be 2.006.

2. A disc, with three sectors of 30° , was compared with direct light in the same manner. The mean of eight experiments gave

$$d_1 = 5.61; d_2 = 12.11; b_3 = 0.2164.$$

From which the ratio of the brightness of the impression produced by means of this disc, with three sectors, to that produced by means of the disc with a single sector, or $\frac{b_3}{b_1}$, is 3.169.

3. The mean of a similar set of experiments made with two discs, one having a single sector, and the other two sectors of 15° , gave $\frac{b_2}{b_1} = 2.099$.

4. Two discs, one having a single sector, and the other two sectors of $7^\circ 30'$ were compared, and it was found that $\frac{b_2}{b_1} = 1.851$.

The results of the experiments in the last four sections are shewn in the accompanying Table.

Number of Flashes in a Second.	Brightness of Impres- sion.
20	1.000
40	2.006
40	1.851
60	3.169

In this table, as in the last, the corresponding numbers in the opposite columns will be seen to be almost exactly proportional; and both sets of experiments therefore, lead to the following results:—

1. The brightness of the impression produced by equal flashes of light, which succeed each other so rapidly as to produce a uniform impression on the eye, is exactly proportional to the number of flashes in a given time.

2. Within the limits of the different velocities of the discs in the experiments, the effect of the combination of the flashes is not sensibly affected by the length of the dark intervals between them.

3. With the same limitation, the effect is also independent of the time of duration of the flashes.

IV. *On the connexion between the apparent Brightness of Light and the time during which it continues to act on the Eye.*

It has thus been proved that the brightness of the impression produced by rapidly succeeding flashes of light is proportional to the number of flashes in a given time, provided the brightness of the flashes remains constant. Hence, if a rapidly revolving disc, with a sector of a given angle, has its velocity doubled, and, consequently, the number of flashes produced by it in a given time also doubled, if the brightness of the flashes remains unaltered, the brightness of the impression produced by them will be twice as great as at first. But, instead of the brightness of the impression increasing, it has been found to continue unchanged, notwithstanding the increased velocity. It is, therefore, evident that when the velocity of the disc is doubled, and, consequently, the duration of each flash is half as great as at first, its brightness is also half as great as at first. Thus, if the disc first revolves 20 times in a second, and then 40 times in a second,

the intensity of the impression is precisely the same in both cases. But at the velocity of 40 revolutions in a second, there are twice as many flashes in a given time as there are at the first velocity; and if the brightness of the flashes was the same as at the first velocity, the brightness of the impression produced by them would be doubled. Since, therefore, the impression, instead of being doubly bright, remains the same as at first, each flash at 40 revolutions in a second must only be half as bright as at 20 revolutions in a second. In like manner, by supposing the velocity increased to 80 revolutions in a second, it might be shewn that the brightness of the flashes is again halved. But the effect of doubling the velocity is to halve the duration of the flashes, therefore the brightness of the flashes is proportional to their duration. This law of vision may be thus stated: When light of a given intensity acts on the eye for a short space of time, the apparent brightness of the luminous impression on the retina is exactly proportional to the time during which the light continues to act. From the velocities of the discs, and the angles of the sectors used in the experiments, it will be seen that this law is true for impressions lasting from $\frac{1}{18432}$ to $\frac{1}{120}$ of a second; and it will presently be shewn to be true for impressions of longer duration.

V. Observations of the apparent Brightness of Luminous Impressions of short duration.

In almost all the experiments hitherto described, the phenomena of vision which have been investigated have been derived from the observation of the aggregate effect of luminous impressions succeeding each other so rapidly as to produce a continuous impression on the eye. It is obvious, that such experiments afford no information regarding the absolute brightness of the separate impressions which are thus blended together. I adopted the indirect mode I have now described of ascertaining the connexion between the duration and apparent brightness of luminous impressions, from an apprehension of the difficulty of comparing the brightness of a constant light with that of an isolated flash. But repeated trials satisfied me that my fears were groundless; and the succeeding experiments prove that, with a little practice, the eye is perfectly capable of making this comparison. Such experiments cannot, however, be long continued without fatiguing the eye, and a considerable effort of attention is required for their successful performance.

In order to find the intensity of separate impressions of short duration, I used a disc of wood two feet in diameter, revolving once in a second; so that a sector, whose arc had a known ratio to the circumference of the disc, passed at each revolution before the aperture in one of the screens of the selsometer. In this manner, a series of perfectly isolated impressions was obtained; and the intensity of each could be compared with that of a light seen by continuous vision in the manner already described. The different sectors were cut in pasteboard, and placed over an aperture in the disc. The following experiments were made:—

1. The fixed light was placed six inches behind the screen before which the disc revolved. The sector had an angle of $0^\circ 27'$, or $\frac{1}{800}$ of the circumference of the disc; and the disc revolved once in a second. To equalise the brightness of the apertures in the screens, when both were seen by continuous vision, the light behind the second screen was placed at a distance (d_1) of 5.3 inches. But, when the disc revolved, this light had to be withdrawn to a distance (d_2) of 46.3 inches. This experiment was repeated ten times, with the following mean results: $d_1 = 5.11$; $d_2 = 50.29$; and the brightness of the flashes $b = 0.0103$, the brightness of the light seen by continuous vision, being unity.

2. A sector of $0^\circ 54'$, or $\frac{1}{400}$ of the circumference, was next used, and ten experiments made as before, from which

$$d_1 = 5.23; d_2 = 35.24 \text{ and } b = 0.022.$$

3. With a sector of $1^\circ 48'$, or $\frac{1}{200}$ of the circumference,

$$d_1 = 5.19; d_2 = 23.52; b = 0.0487.$$

4. With a sector of $3^\circ 36'$, or $\frac{1}{100}$ of the circumference,

$$d_1 = 4.9; d_2 = 15.08; b = 0.1056.$$

5. With a sector of $7^\circ 12'$, or $\frac{1}{50}$ of the circumference,

$$d_1 = 5.13; d_2 = 11.3; b = 0.2061.$$

6. With a sector of 15° , or $\frac{1}{25}$ of the circumference,

$$d_1 = 5.02; d_2 = 7.64; b = 0.4317.$$

With each sector, the mean of ten results was taken; and at each successive trial, the flame was alternately drawn from the screen, or pulled towards it in equalising the apparent brightness of the apertures in the screens. The following Table contains the results of these experiments, the brightness of the light seen by continuous vision being expressed by unity:—

d_1	d_2	Duration of Flash in Seconds.	Brightness of Impression.
5.11	50.29	0.00125	0.0103
5.23	35.24	0.00250	0.0220
5.19	23.52	0.00500	0.0487
4.90	15.08	0.01000	0.1056
5.13	11.30	0.02000	0.2061
5.02	7.64	0.04167	0.4317

The results of these experiments are shewn in fig. 3, where the observed intensities of the light, denoted by small circles, are projected with the corresponding times during which it acted on the eye; and it will be observed, that the line acb , shewing the increase of the apparent brightness of the object, with the time during which it remains visible, is very nearly straight; which proves that

within the limits of the observations the brightness of the light increases in exact arithmetical proportion with the time during which it acts on the eye. Since the observed intensities of the lights when projected, as in the figure, are all nearly included in a straight line passing through the origin, it may naturally be inferred, that the impression of light commences at the instant of its incidence on the retina. This conclusion is strengthened, when it is recollected that the preceding experiments prove that light, which is incident on the eye only $\frac{1}{18432}$ of a second, produces a distinct impression, while, according to Professor WHEATSTONE, less than the millionth part of a second is necessary for this effect. It has also been proved (see p. 594), that up to $\frac{1}{18432}$ of a second, the impression produced by light is proportional to its duration. It seems, therefore, highly probable, that from 0" up to 0"·05, *the brightness of a luminous impression is exactly proportional to the time during which the light has acted on the eye.**

These experiments, therefore, confirm, in a very satisfactory manner, the inference which has already been drawn from the previous investigation, as the observed intensities of the flashes are very nearly proportional to their duration; while, at the same time, they exhibit the actual numerical ratio of the apparent brightness of a flash of a certain duration, to that of the light which produces it acting continuously on the eye.

VI. *The time required for the complete production of Luminous Impressions is independent of the apparent intrinsic brightness of the light.*

The following series of experiments was made partly to confirm the result already obtained; but more especially in order to ascertain whether the time required for the complete development of luminous impressions varies with the brightness of the light by which they are produced. In this set of experiments, the same sectors were used as in the last; and the circumstances were identical, except that the fixed light was placed 8·5 inches ($6\sqrt{2}$) from the screen, so that the brightness of the incident light was reduced to half its former intensity. The following Table exhibits the mean of ten observations with each sector; the brightness of the light seen by continuous vision, being expressed by unity.

d_1	d_2	Duration of Flash.	Brightness of Flash.
7·32	64·17	0·00125	0·0130
7·31	44·12	0·00250	0·0275
7·56	33·60	0·00500	0·0508
7·62	24·20	0·01000	0·0991
7·60	16·06	0·02000	0·2240

* In an experiment made since this paper was read, I have found that the same law extends to impressions lasting for $\frac{1}{16}$ th of a second, of which the observed brightness was 0·6118.

In fig. 3, the line ae contains the projections of these observations, which are denoted by crosses; and it nearly coincides with the line ab , containing the projections of the observations in the last Table, shewing that the ratio of the brightness of an impression of given duration to that of the absolute brightness of the light which produces it, is almost exactly the same in both sets of experiments. On thus comparing the apparent intensities of the flashes exhibited in the above Table with the similar results in the preceding one, it will be seen that although the absolute intensity of the light is only half as great as formerly, the time required for the propagation of the luminous impression on the eye remains unaltered; while both sets of experiments prove that the brightness of a luminous impression caused by a light of given intensity is proportional to the time during which the light acts on the eye.

On repeating the experiment with the sector of $\frac{1}{50}$ th of the circumference, revolving once in a second, with the fixed light 24 inches from the screen, the mean of ten trials gave

$$d_1 = 22.27; d_2 = 48.46; b = 0.2112.$$

The ratio of the apparent brightness of the flashes to that of the light seen by continuous vision is, in this case, almost exactly the same as in the preceding experiments, as will be seen from the following comparative view:—

Distance of Light.	Intensity of Light.	Time during which Light acted on the Eye.	Ratio of the brightness of Flashes to that of the Light seen continuously.
6.0	1.0000	0".02	0.2061
8.5	0.5000	0".02	0.2240
24.0	0.0625	0".02	0.2112

The conclusion to be derived from these results will be distinctly apprehended by reference to fig. 3, where the ordinates ef , cf , and df , represent the apparent intensities of the lights shewn in the above table. In order to prevent misunderstanding, it is necessary to observe, that although the absolute brightness of the lights used in the three experiments given in the table are in the ratio of the numbers 1, 2, and 16, they are all represented in the figure by the same line ag ; and since the lines ef , cf , df , are nearly equal, they may be regarded as having the same ratio to ag , the slight differences between them obviously resulting from errors of observation. It thus appears that after an interval of $\frac{1}{50}$ th of a second, the three lights of very different intensity have all produced the *same portion* of their *total effect* on the eye; the impression in each case having nearly $\frac{1}{5}$ th of the absolute brightness of the light.

Lights of different intensity, therefore, produce like portions of their total effect

on the eye in equal times; from which it obviously follows, that the brightness of an impression on the eye increases with a rapidity exactly proportional to the brightness of the light which produces it.

This conclusion seemed so remarkable, that I determined to try whether the direct light of the sun produced a given portion of its impression on the eye with no greater rapidity than ordinary artificial light. For this purpose I made use of a selaometer, represented in fig. 5, where K L represents a plate of brass with two apertures A B, $\frac{1}{6}$ th of an inch in diameter, and half an inch distant. A plate of ground glass is placed before the apertures, and behind the aperture B, a tube B C is fixed, in which is placed a Nicol's polarizing prism. A longer tube B D, is fitted so as to turn freely upon the outside of the tube C D, and another Nicol's prism is placed in its further extremity, so that, by turning round the tube B D, the illumination of the aperture B can be varied at pleasure. A disc E F, with a sector of $7^{\circ} 30'$ revolves rapidly in front of the plate, by means of the band H I passing over the pulley G, so as to project beyond the aperture A, which is only visible when the sector passes before it at each revolution of the disc.* The apertures were first illuminated by gas-light, and the disc being made to revolve so rapidly as to produce a continuous impression, the apparent brightness of the apertures was made equal by turning one of the prisms. When the apparatus was next illuminated by the direct light of the sun at noon, and the disc made to revolve so as to produce a uniform impression, the apertures were still equally bright, although the position of the prisms remained unaltered. This experiment was repeated several times with the same result, and a similar result was obtained when moon-light was compared with gas-light. Now the effect of turning round the prism is to diminish the brightness both of the sun-light and gas-light in the same proportion. Since, therefore, the two apertures were always equally bright, it follows, that the apparent brightness of the aperture behind the revolving disc, had also, in both cases, the same ratio to that of the light seen by uninterrupted vision. But the ratio of the apparent brightness of the aperture behind the revolving disc to that of the direct light, evidently depends on the rapidity with which the light acts on the eye at each passage of the sector before the luminous aperture. Hence it is obvious, that if the sun-light and gas-light required different times to produce like portions of their total effect on the eye, the apparent brightness of the flashes produced by the revolving disc would have different ratios

* By means of this arrangement, the brightness of the impressions produced during the revolution of the disc, can be compared with the light transmitted through the aperture B. Since the intensity of a ray of polarized light when transmitted through a doubly-refracting crystal, varies as the square of the cosine of the inclination of the principal section of the crystal to the plane of polarization of the ray; by attaching an index to the tube B D, so as to measure the angle through which it has been turned, the intensity of the transmitted light might be estimated, and thus the brightness of the impressions produced by the revolving disc might be determined. (See *Supplément au Traité de la Lumière* de Sir J. F. W. HERSCHEL. Par A. QUETELET, p. 595.)

to that of the uninterrupted light, according as the apparatus was illuminated by sun-light or by gas-light. Therefore, since it has been shewn that this was not the case, it is evident, that the sun-light and gas-light produced similar portions of their complete impressions on the eye with the same rapidity.

It has thus been proved, that, when light acts on the eye for short intervals of time, the rapidity of the development of its impression is independent of its actual brightness; and it seems highly probable that this law extends to the whole time required for the complete production of luminous impressions. For, when it has been found, that lights of very different intensity acting on the eye during $\frac{1}{50}$ th of a second, all produce impressions, having almost exactly $\frac{1}{5}$ th of the absolute brightness of the lights, it seems natural to conclude, that they will also produce their complete effect on the eye in exactly equal times.

I hoped to have been able in this paper to exhibit the results of some experiments upon the intensity of impressions of short duration, repeated by different individuals, in order to ascertain whether the rapidity of the production of visual impressions varies much in different eyes. I have only obtained one comparison of this kind, through the kindness of Mr ALEXANDER WALLACE, of the Royal Observatory, Edinburgh, who observed the impression produced on his eye by a disc with a sector of $7^{\circ} 30'$, revolving 20 times in a second. The following result is the mean of three trials,

$$d_1 = 4.39; d_2 = 42.5; b = 0.01067.$$

The result of my own experiments gives $b = 0.0137$: which agrees very well with Mr WALLACE's observations. I trust to be able to obtain some more comparisons of this kind, in order to ascertain whether the agreement between Mr WALLACE's result and my own is to be regarded as proving that visual impressions in the eyes of different individuals, are propagated with nearly equal rapidity.

VII. *On the time which Light requires to produce a full impression on the Eye.*

I have found, by means of a disc revolving once in a second (see p. 594), that impressions produced by a light acting on the eye for $\frac{1}{10}$ th of a second, have very nearly the same brightness as the light seen by continuous vision; but that when light acts on the eye for a shorter time, its apparent brightness is sensibly diminished. As the brightness of the impression produced by light increases by insensible degrees, until, at length, it attains its full intensity, it is obviously almost impossible, by direct observation, to assign the exact instant when this takes place. The experiments in Sections V. and VI. have proved, that, up to $\frac{1}{16}$ th of a second, the brightness of a luminous impression is strictly proportional to the time during which light has acted on the eye, and also, that the impression produced in $\frac{1}{100}$ th of a second, has almost exactly $\frac{1}{10}$ th of the brightness of a full impres-

sion. If this proportionality between the duration and the apparent brightness of a light be supposed to extend beyond the limits of the experiments, so as to include nearly the whole time required for the production of a complete impression, it would obviously follow that light requires about $\frac{1}{10}$ th of a second to produce its full effect on the eye; and this conclusion, it will be observed, agrees with the result of direct observation.

The following inferences may be derived from the laws of vision which have now been investigated.

1. *Personal Equation in Astronomy.*

It is well known, that different observers assign different times to the occurrence of the same astronomical phenomenon; as, for example, the passage of a star across the meridian wire of a transit instrument. The correction to be applied to reduce the observations, or personal equation, as it is termed, frequently amounts to a considerable quantity.

It might at first be supposed, that this discrepancy between the results of different observers, may be occasioned by light acting on their eyes with unequal degrees of rapidity. But on considering the manner of observing the transit of a star, it will appear that this explanation is insufficient. In order to estimate the exact time at which the star passes one of the wires, the observer endeavours to recollect the position of the star on one side of the wire, at the instant when he heard the clock beat. At the next beat of the clock, the star has passed to the other side of the wire; and the observer then, by the eye, subdivides into equal parts the space between the positions occupied by the star at the successive beats of the clock, and estimates how many of those parts are contained in the interval between the wire and the first position of the star. The magnitude of that interval estimated in this manner, determines the fraction of a second to be added to the time given by the clock. If, then, the discrepancy between the results of different observers is to be regarded as a phenomenon of vision, it must depend upon some cause which displaces the image of the star, and thereby alters its apparent distance from the wire. Now as the star passes across the field of the telescope, its light falls successively upon different parts of the retina, illuminating each portion for a very small space of time; and if light acted on the eye of one observer more rapidly than on that of another, the obvious consequence would be that the image of the star would appear brighter to the person whose retina was most quickly impressed by light. The only other effect which the gradual action of light on the eye seems capable of producing, is to render the advancing edge of the image of the star so faint, owing to the extremely short time during which its light acts on the eye, as to become imperceptible when contrasted with the succeeding parts of the image: for these fall upon points of the retina over

which a portion of the image has already passed, and on which the light has had time to develop a distinct impression. In this manner, it may be conceived, that the breadth of the image will be diminished on the side towards which it moves, while it will be increased on the other side by the persistence of the impression of light on the eye; and, consequently, the image of the star will appear behind its true position. It is obvious, however, that the retardation of the advancing edge of the image cannot exceed the breadth of the extremely minute disc with which a star appears in a good telescope, otherwise it would amount to a total extinction of the light; and, on the other hand, the image cannot be prolonged by the persistence of its impression on the retina, by a greater quantity than its advancing edge is retarded, without becoming perceptibly elongated. Any difference in the amount of retardation due to such causes, in different eyes, must therefore be confined within extremely narrow limits, and seems quite inadequate to account for the personal equation which, in some instances, amounts to a large fraction of a second.*

2. Rays of Light of different Refrangibility act on the Eye with the same rapidity.

In the observations made with a rapidly revolving disc, where each flash lasted only $\frac{1}{18432}$ of a second, not the slightest alteration in the colour of the luminous object was perceptible. The blue part of a gas flame, indeed, became invisible; but this was evidently due to the great reduction of the intensity of the light rendering the blue rays incapable of producing a sensible impression on the eye, already affected by the more luminous rays. From this it follows, that rays of light of different refrangibility act on the eye with equal rapidity. For if we suppose some of the rays which constitute white light to act on the eye more rapidly than others, the effect of shortening the luminous impressions would be quite analogous to that produced by the interposition of some medium, such as red glass, which absorbs the rays unequally; and the eye would be affected with the complementary colour of the deficient rays.

That there is no sensible difference in the rapidity of the action of lights of various colours on the retina, appears also from the fact, that when the eye is suddenly directed to a luminous object, the first impression of its colour remains afterwards unaltered. This could not be always the case if there was any great inequality in the rapidity with which the different rays produce their effect on the eye. If, for example, we suppose the blue rays to act more rapidly than the yellow rays, green objects would, at first sight, appear to have more of a bluish tinge than after the eye had continued to regard them for a short time.

* As my object here is simply to discuss the possibility of explaining the personal equation by the gradual action of light on the retina, I have intentionally refrained from entering upon any explanation of that phenomenon which may be derived from the supposition that time is required for the transmission of impressions from the organs of sensation to the mind.

3. *On the Difference between the Apparent and the Intrinsic Brightness of the Flash produced by Electricity of high tension.*

Professor WHEATSTONE, as was already noticed, has proved that the light of electricity of high tension has a less duration than the millionth part of a second. Now, since it has been shewn that lights of every intensity produce their impressions on the eye in equal times,* and that the brightness of an impression is exactly proportional to its duration; it follows, that if the electric spark could be made to last for the hundredth part of a second, which is 10,000 times its actual duration, its apparent brightness would also be increased 10,000 times. But the results already recorded (see Table, p. 595), shew that the apparent intensity of light lasting for the hundredth part of a second scarcely exceeds a tenth of its real intensity. Hence, if the duration of the electric spark could be prolonged so as to render its light continuous, its apparent brightness would probably be increased about 100,000 times.

From the nature of the experiments on which this conclusion is founded, it is perhaps only strictly applicable to the case where the electric spark is seen by the eye already acted on by light of moderate intensity; for in other cases its apparent brightness is no doubt greatly increased by the contrast with previous darkness;† but however remarkable the conclusion may appear, it seems perfectly consistent with the estimate of the intrinsic brightness of the electric spark, which arises from reflecting on the extremely short space of time in which its powerful impression on the eye is produced.

Dr Faraday observes, that “the beautiful flash of light attending the discharge of common electricity, rivals in brilliancy, if it does not even very much surpass, the light from the discharge of voltaic electricity;”‡ and again he states, that when a battery of 15 jars was discharged through a wet string, “the spark was yellowish, flamy, and having a duration sensibly longer than if the water had not been interposed.” Now the effect of discharging the battery through a bad conductor, would be greatly to diminish the tension of the electricity, while it augmented the duration of the spark. If, therefore, the intrinsic brightness of the spark had remained the same as before, the intensity of the impression on the eye should have been increased; but the reverse seems to have been the case. Hence it follows, that the brightness of the electric spark increases with the tension of the electricity. A similar conclusion may obviously be derived from a compa-

* The electric spark is a light whose intensity places it undoubtedly within the limits of the experiments on this point, as its brightness is inferior to that of sun-light. According to Sir JOHN HERSCHEL, the lime-ball light appears only as a black spot on the disc of the sun when held between it and the eye.—(See *Treatise on Astronomy*, LARDNER'S *Cyclopædia*, p. 210. London, 1835.)

I have observed that, in like manner, the spark produced by a strongly-charged Leyden phial, is absolutely invisible when it passes between the eye and the sun's disc.

† See *Light*. *Encyclopædia Metropolitana*, Art 58.

‡ Experimental Researches in Electricity, vol. i., sec. 333. Lond. 1839.

ri-son of the nearly instantaneous electric spark of high tension, with the apparently continuous light of voltaic electricity. For since the latter light, notwithstanding its sensible duration, does not appear brighter than the former, it must obviously be greatly inferior in intrinsic brightness.

It may now be useful to recapitulate the principal results of the experiments described in this paper.

1. When the eye receives a succession of flashes of equal duration from a light of constant intensity, which succeed each other so rapidly as to produce a uniform impression, the intensity of this aggregate impression will also be constant, provided the number of flashes in a given time varies inversely with the duration of each.

2. The brightness of the impression produced by flashes of light of a given intensity, which succeed each other so rapidly as to produce a uniform impression on the eye, is proportional to the number of flashes in a given time.

3. When light of a given intensity acts on the eye for a short space of time, the brightness of the luminous impression on the retina is exactly proportional to the time during which the light continues to act. This law has been proved to be true for impressions lasting from $\frac{1}{18432}$ to $\frac{1}{24}$ of a second.

4. The intensity of the impression produced by light which acts on the eye for $\frac{1}{100}$ of a second is almost exactly $\frac{1}{10}$ th of the apparent brightness of the light when seen by uninterrupted vision; and the time required for light to produce its full effect on the eye seems to be about $\frac{1}{10}$ th of a second.

5. Lights of different intensities produce their complete impressions on the eye in equal times, so that the light of the sun requires the same time as common artificial light to produce its impression on the eye.

6. The brightness of an impression on the eye increases with a rapidity exactly proportional to that of the light by which it is produced.

7. Rays of different refrangibility act on the eye with equal rapidity.

8. The apparent brightness of the spark produced by electricity of high tension is only about $\frac{1}{100000}$ th of what its apparent brightness would become if its duration were prolonged to $\frac{1}{10}$ th of a second; and the brightness of electric light increases with the tension of the electricity.

PROCEEDINGS
OF THE
EXTRAORDINARY GENERAL MEETINGS,
AND
LISTS OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,
SINCE NOVEMBER 25, 1844.

PROCEEDINGS, &c.

Monday, November 25, 1844.

At a Statutory General Meeting, held for the purpose of appointing Office-Bearers for the ensuing Session, The Right Honourable Earl CATHCART, Vice-President, in the Chair, the Ballot was taken in the usual way, and the following Gentlemen were declared to be duly elected, viz. :—

Sir T. MAKDOUGALL BRISBANE, Bart., President.

Sir WILLIAM MILLER, Bart.,

Sir DAVID BREWSTER, K.H.,

Earl CATHCART,

Very Reverend Principal LEE,

Sir GEORGE S. MACKENZIE, Bart.,

Right Reverend Bishop TERROT,

Professor FORBES, General Secretary.

DAVID MILNE, Esq.,

Dr GREGORY,

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library.

JOHN STARK, Esq., Curator of Museum.

Vice-Presidents.

Ordinary Secretaries.

COUNSELLORS.

Dr PARNELL.

Dr CARSON.

Sir JOHN M'NEILL.

Sir THOMAS D. LAUDER, Bart.

ALAN STEVENSON, Esq.

JAMES T. GIBSON-CRAIG, Esq.

Dr CRAIGIE.

Professor MILLER.

Professor PILLANS.

Professor KELLAND.

Dr CHRISTISON.

Dr NEILL.

Dr CHRISTISON, as acting General Secretary, read the following Resolution from the Minutes of the Council :—" 18th November 1844.—The Council unanimously resolved, That it was expedient to allow a Salary of £100 annually to the General Secretary, on the understanding that the duties of the Office should, henceforth, include the charge of publishing the Society's Proceedings, and that the proposed arrangement should be considered as an experimental

one, which might be altered in the event of its not being found to answer the Council's expectations, or of the Society's funds proving inadequate." Dr CHRISTISON having explained the views which had led the Council to this opinion, Lord MURRAY moved, seconded by JAMES L'AMY, Esq., That the Resolution of the Council be adopted by the Society; which motion was agreed to by the meeting, with one dissentient.

On the motion of the Treasurer, the Council-Committee on the Funds were appointed to audit his accounts.

Mr RUSSELL moved, seconded by Dr GREVILLE, That the Royal Society, in consideration of the able and efficient manner in which Dr CHRISTISON, in the absence of Professor FORBES, has, during the last twelvemonth, discharged the duties of General Secretary to the Society, hereby tender their warmest thanks to him for these and all his other valuable services. This motion being carried unanimously, Earl CATHCART conveyed the thanks of the Society to Dr CHRISTISON.

(Signed) C. H. TERROT, *V.P.*

Monday, November 24, 1845.

At a Statutory General Meeting. Right Reverend Bishop TERROT in the Chair, the following Office-Bearers were duly elected:—

Sir T. M. BRISBANE, Bart., G.C.B., President.

Sir WILLIAM MILLER, Bart.,

Sir D. BREWSTER, K.H.,

Very Reverend Principal LEE,

Sir G. S. MACKENZIE, Bart.,

Right Reverend Bishop TERROT,

Dr CHRISTISON,

Professor FORBES, General Secretary.

D. MILNE, Esq.,

Dr GREGORY,

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library.

JOHN STARK, Esq., Curator of Museum.

Vice-Presidents.

Secretaries to Ordinary Meetings.

COUNSELLORS.

ALAN STEVENSON, Esq.

J. T. GIBSON-CRAIG, Esq.

Dr CRAIGIE.

Professor MILLER.

Professor KELLAND.

Professor PILLANS.

Dr NEILL.

Dr FLEMING.

Mr ADIE.

Lord MURRAY.

Dr BRUNTON.

G. FORBES, Esq.

The following Committee was appointed to audit the Treasurer's accounts:—

Sir H. JARDINE.

JAMES GIBSON-CRAIG, Esq.

GEORGE FORBES, Esq.

Part of the Minute of Council of the 19th November having been read. relative to the Salary of the General Secretary, it was moved by Dr MACLAGAN, "That the Royal Society having considered the recommendation of the Council of the 19th November, that the Salary of £100 should be continued to Professor FORBES, as General Secretary, unanimously approve of the said recommendation, and resolve that the said Salary of £100 shall be continued to Mr FORBES;" which motion having been seconded by Mr CADELL, was unanimously agreed to.

It was farther moved and seconded, and unanimously agreed to, That, in conformity to a recommendation by the Council, the name of JAMES SKENE, Esq. of Rubislaw, now returned from the Continent, shall (if agreeable to him) be replaced in the list of Fellows of the Society.

(Signed) G. S. MACKENZIE, V.P.

Monday, November 23, 1846.

At a Statutory General Meeting, Sir G. S. MACKENZIE, Bart., V. P., in the Chair, the following Office-Bearers were duly elected:—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,

Right Hon. Earl CATHCART,

Very Rev. Principal LEE,

Sir GEORGE S. MACKENZIE, Bart.,

The Right Rev. Bishop TERROT,

Dr CHRISTISON,

Professor FORBES, General Secretary.

DAVID MILNE, Esq.,

Dr GREGORY,

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library and Instruments.

JOHN STARK, Esq., Curator of Museum.

Vice-Presidents.

Secretaries to the Ordinary Meetings.

COUNSELLORS.

Professor KELLAND.

Professor PILLANS.

Dr NEILL.

Rev. Dr FLEMING.

A. ADIE, Esq.

HON. Lord MURRAY.

Rev. Dr BRUNTON.

GEORGE FORBES, Esq.

W. A. CADELL, Esq.

Sir WM. SCOTT, Bart.

Dr J. H. BALFOUR.

HENRY MARSHALL, Esq.

On the motion of Sir G. S. MACKENZIE, it was resolved, unanimously, That the name of Earl CATHCART be replaced according to his former standing in the List of Vice-Presidents.

The following gentlemen were named a Committee to audit the Treasurer's Accounts:—

GEORGE FORBES, Esq.

D. SMITH, Esq.

J. T. GIBSON-CRAIG, Esq.

The Meeting then adjourned.

(Signed) C. H. TERROT, V.P.

Monday, November 22, 1847.

At a Statutory General Meeting, Bishop TERROT in the Chair, the following Office-Bearers for the ensuing Session were duly elected :—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.			
Sir D. BREWSTER, K.H.,	}	Vice-Presidents.	
Right Hon. Earl CATHCART,			
Very Rev. Principal LEE,			
Sir GEORGE S. MACKENZIE, Bart.,			
Right Rev. Bishop TERROT,			
Dr CHRISTISON,	}	Secretaries to the Ordinary Meetings.	
Professor FORBES, General Secretary.			
DAVID MILNE, Esq.,			
Dr GREGORY,			
JOHN RUSSELL, Esq., Treasurer.			
Dr TRAILL, Curator of Library and Instruments.			
JOHN STARK, Esq., Curator of Museum.			

COUNSELLORS.

A. ADIE, Esq.	Dr J. H. BALFOUR.
Hon. Lord MURRAY.	HENRY MARSHALL, Esq.
Rev. Dr BRUNTON.	Sir WM. JARDINE, Bart.
GEORGE FORBES, Esq.	Prof. C. PIAZZI SMYTH.
W. A. CADELL, Esq.	Rev. Dr ROBERTSON.
Sir WM. SCOTT, Bart.	C. MACLAREN, Esq.

The following gentlemen were appointed to audit the Treasurer's Accounts :—

GEORGE FORBES, Esq.	DAVID SMITH, Esq.	JAMES T. GIBSON-CRAIG, Esq.
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The Meeting then adjourned.

(Signed) C. H. TERROT, V.P.

Memorandum.—February 21, 1848.—At an Ordinary Meeting of the Royal Society, on the 21st February, the following motion for a change of Law, was proposed by Mr RUSSELL. Treasurer, the motion itself having been announced from the Chair on the 17th January, and printed in the Billets of the 7th and 21st February, viz :—

“ That Law IV. shall be altered, and stand as follows :—

“ IV. The Fees of Admission of an Ordinary Non-Resident Fellow shall be £26, 5s., payable on his admission ; and in case of any Non-Resident Fellow coming to reside at any time in Scotland, he shall, during each year of his residence, pay the usual Annual Contribution of £3, 3s. payable by each Resident Fellow ; but after payment of such Annual Contribution for eight years, he shall be exempt from any farther payment.”

The motion was seconded by Dr TRAILL, and was adopted.

(Signed) C. H. TERROT, V.P.

Monday, November 27, 1848.

At a Statutory General Meeting, Right Rev. Bishop TERROT V.P., in the Chair, the following Office-Bearers for the ensuing year, were duly elected :—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,

Right Hon. Earl CATHCART,

Very Rev. Principal LEE,

Right Rev. Bishop TERROT,

Dr CHRISTISON,

Dr ALISON,

Professor FORBES, General Secretary.

Dr GREGORY,

Professor C. P. SMYTH,

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library and Instruments.

JOHN STARK, Esq., Curator of Museum.

Vice-Presidents.

Secretaries to the Ordinary Meetings.

COUNSELLORS.

W. A. CADELL, Esq.

Sir WM. SCOTT, Bart.

Dr J. H. BALFOUR.

HENRY MARSHALL, Esq.

Sir WM. JARDINE, Bart.

Rev. Dr ROBERTSON.

C. MACLAREN, Esq.

D. MILNE, Esq.

J. T. GIBSON-CRAIG, Esq.

Dr GEORGE WILSON.

Sir JOHN MACNEILL, G.C.B.

JAMES DALMAHOY, Esq.

The following Committee was named to audit the Treasurer's Accounts.

Dr NEILL.

JAMES T. GIBSON-CRAIG, Esq.

JAMES WILSON, Esq.

The Meeting then adjourned.

MEMBERS ELECTED.

January 6, 1845.

JAMES ANDREW, M.D.

GEORGE WILSON, M.D.

February 3, 1845.

JOHN G. M. BURT, M.D.

THOMAS ANDERSON, M.D.

January 5, 1846.

A. TAYLOR, M.D., Pau.

S. A. PAGAN, M.D.

February 2, 1846.

Rev. Dr JAMES ROBERTSON.

ALEXANDER J. ADIE, Esq.

PROCEEDINGS OF GENERAL MEETINGS,

February 16, 1846.

WILLIAM MURRAY, Esq., of Henderland.

March 16, 1846.

GEORGE TURNBULL, Esq.

GEORGE J. GORDON, Esq.

Dr L. SCHMITZ, Rector of High School.

December 7, 1846.

CHARLES PIAZZI SMYTH, Esq., Professor of Astronomy.

January 4, 1847.

GEORGE MARGILL, Esq.

DAVID GRAY, Esq., Professor of Nat. Philosophy, Marischal College, Aberdeen.

February 1, 1847.

WILLIAM THOMSON, Esq., Prof. of Nat. Philosophy, Glasgow.

J. H. BURTON, Esq., Advocate.

March 1, 1847.

JAMES NICOL, Esq., London.

April 19, 1847.

W. MACDONALD MACDONALD, Esq., of St Martins.

ROBERT HANDYSIDE, Esq., Advocate.

ALEXANDER CHRISTIE, Esq.

December 20, 1847.

JOHN WILSON, Esq., Cirencester.

MOSES STEVEN, Esq., of Bellahouston.

January 17, 1848.

JAMES TOD, Esq., W.S.

March 6, 1848.

THOMAS STEVENSON, Esq., C.E.

JAMES ALLAN, M.D., Haslar Hospital.

JOHN HALL MAXWELL, Esq., Younger of Dargavel.

March 20, 1848.

Rev. JOHN HANNAH.

HENRY DAVIDSON, Esq.

April 17, 1848.

PATRICK NEWBIGGING, M.D.

WILLIAM SWAN, Esq.

December 4, 1848.

Rev. FRANCIS GARDEN.

December 18, 1848.

PATRICK JAMES STIRLING, Esq.

January 2, 1849.

WILLIAM STIRLING, Esq., of Keir.

D. R. HAY, Esq.

JOHN THOMSON GORDON, Esq., Sheriff of Mid-Lothian.

WILLIAM THOMAS THOMSON, Esq.

Rt. Hon. ANDREW RUTHERFURD, Lord Advocate for Scotland.

Honourable LORD IVORY.

February 5, 1849.

ADAM ANDERSON, Esq., Advocate.

February 19, 1849.

WILLIAM E. AYTOUN, Esq., Professor of Rhetoric and Belles Lettres, University, Edinburgh.

W. H. LOWE, M.D.

March 19, 1849.

Hon. B. F. PRIMROSE.

JOHN STENHOUSE, M.D., Glasgow.

DAVID ANDERSON, Esq., of Moredun.

April 2, 1849.

W. R. PIRRIE, M.D., Professor of Surgery, Marischal College, Aberdeen.

Right Hon. The EARL of MINTO.

April 16, 1849.

Right Hon. The EARL of ABERDEEN.

Right Hon. The EARL of HADDINGTON.

LIST OF THE PRESENT ORDINARY MEMBERS,

IN THE ORDER OF THEIR ELECTION.

Major-General Sir THOMAS M. BRISBANE, Bart., G.C.B., &c., F.R.S. Lond.,
PRESIDENT.

Date of
Election.

- 1798 Alexander Monro, M.D.
1799 Robert Jameson, Esq., *Professor of Natural History*.
1805 Thomas Thomson, M.D., F.R.S. Lond., *Professor of Chemistry, Glasgow*.
George Dunbar, Esq., *Professor of Greek*.
1807 John Campbell, Esq., *of Carbrook*.
Thomas Thomson, Esq., *Advocate*.
1808 James Wardrop, Esq.
Sir David Brewster, K.H., LL.D., F.R.S. Lond., *St Andrews*.
1811 Major-General Sir Thomas Makdougall Brisbane, Bart., G.C.B., G.C.H., F.R.S. Lond.
James Jardine, Esq., *Civil Engineer*.
J. G. Children, Esq., F.R.S. Lond.
Alexander Gillespie, Esq., *Surgeon*.
W. A. Cadell, Esq., F.R.S. Lond.
James Pillans, Esq., *Professor of Humanity*.
1812 Sir George Clerk, Bart., F.R.S. Lond.
1813 William Somerville, M.D., F.R.S. Lond.
1814 Sir Henry Jardine.
Patrick Neill, LL.D., *Secretary to the Wernerian and Horticultural Societies*.
Right Honourable Lord Viscount Arbuthnot.
John Fleming, D.D., *Professor of Natural Science, New College*.
Alexander Brunton, D.D.
1815 Robert Stevenson, Esq., *Civil Engineer*.
Henry Home Drummond, Esq., *of Blair-Drummond*.
William Thomas Brande, Esq., F.R.S. Lond., *Professor of Chemistry in the Royal Institution*.

Date of
Election.

- 1816 Leonard Horner, Esq., F.R.S. Lond.
Henry Colebrooke, Esq., *Director of the Asiatic Society of Great Britain.*
Honourable Lord Fullerton.
- 1817 Right Honourable Earl of Wemyss and March.
John Wilson, Esq., *Professor of Moral Philosophy.*
Alexander Maconochie, Esq., *of Meadowbank.*
Sir David James Hamilton Dickson, M.D., *Clifton.*
William P. Alison, M.D., *Professor of the Practice of Physic.*
Robert Bald, Esq., *Civil Engineer.*
- 1818 Robert Richardson, M.D., *Harrowgate.*
Patrick Miller, M.D., *Exeter.*
John Watson, M.D.
Right Honourable John Hope, *Lord Justice-Clerk.*
- 1819 Sir Patrick Murray, *of Simprim.*
James Muttiebury, M.D., *Bath.*
Thomas Stewart Traill, M.D., *Professor of Medical Jurisprudence.*
Alexander Adie, Esq.
Marshall Hall, M.D., *London.*
Richard Philips, Esq., F.R.S. Lond.
Reverend William Scoresby, *Exeter.*
George Forbes, Esq.
- 1820 Right Honourable David Boyle, *Lord Justice-General.*
James Keith, Esq., *Surgeon.*
Charles Babbage, Esq., F.R.S. Lond.
Thomas Guthrie Wright, Esq., *Auditor of the Court of Session.*
Sir John F. W. Herschel, Bart., F.R.S. Lond.
John Shank More, Esq., *Professor of Scots Law.*
Robert Haldane, D.D., *Principal of St Mary's College, St Andrews.*
Sir John Mead, M.D., *Weymouth.*
Dr William Macdonald.
Sir John Hall, Bart., *of Dunglass.*
Sir George Ballingall, M.D., *Professor of Military Surgery.*
- 1821 Sir James M. Riddell, Bart., *of Ardnamurchan.*
Archibald Bell, Esq., *Advocate.*
John Clerk Maxwell, Esq., *Advocate.*
John Lizars, Esq., *Surgeon.*
John Cay, Esq., *Advocate.*
Robert Kaye Greville, LL.D.
Robert Hamilton, M.D.
A. R. Carson, Esq., LL.D.
- 1822 James Smith, Esq., *of Jordanhill*, F.R.S. Lond.
William Bonar, Esq.
George A. Walker-Arnott, Esq., LL.D., *Professor of Botany, Glasgow.*
Very Reverend John Lee, D.D., *Principal of the University.*

Date of
Election.

- 1822 Sir James South, F.R.S. Lond.
 Lieutenant-General Martin White.
 Walter Frederick Campbell, Esq. of *Shawfield*, M.P.
 Sir W. C. Trevelyan, Bart., *Nettlecombe, Somersetshire*.
 Sir Robert Abercromby, Bart., of *Birkenbog*.
 Dr Wallich, *Calcutta*.
 John Russell, Esq., P.C.S.
 John Dewar, Esq., *Advocate*.
- 1823 Sir Edward Ffrench Bromhead, Bart., A.M., F.R.S. Lond. *Thurbsby Hull*.
 Captain Thomas David Stuart, of the *Hon. East India Company's Service*.
 Andrew Fyfe, M.D., *Professor of Medicine and Chemistry, King's College, Aberdeen*.
 Robert Bell, Esq., *Advocate*.
 Captain Norwich Duff, R.N.
 Warren Hastings Anderson, Esq.
 Alexander Thomson, Esq. of *Banchory*.
 Liscombe John Curtis, Esq., *Ingsdon House, Devonshire*.
 Robert Christison, M.D., *Professor of Materia Medica*.
 John Gordon, Esq., of *Cairnbulg*.
- 1824 Dr Lawson Whalley, *Lancaster*.
 Alexander Wilson Philip, M.D., *London*.
 Sir Charles Adam, R.N.
 Robert E. Grant, M.D., *Professor of Comparative Anatomy, Univ. Coll., London*.
 Rev. Dr William Muir, *one of the Ministers of Edinburgh*.
 W. H. Playfair, Esq., *Architect*.
 John Argyle Robertson, Esq., *Surgeon*.
 James Pillans, Esq.
 James Walker, Esq., *Civil Engineer*.
 Sir William Newbigging, *Surgeon*.
 William Wood, Esq., *Surgeon*.
- 1825 The Venerable Archdeacon John Williams.
 W. Preston Lauder, M.D., *London*.
 Right Honourable Lord Ruthven.
 Sir William Jardine, Bart., of *Applegarth*.
 Honourable Lord Wood.
- 1826 Sir David Hunter Blair, Bart.
 John Stark, Esq.
 Dr John Macwhirter.
- 1827 John Gardiner Kinnear, Esq.
 James Russell, M.D.
 Rev. Dr Robert Gordon, *one of the Ministers of Edinburgh*.
 James Wilson, Esq.
 Very Rev. Edward Bannerman Ramsay, A.M., *Camb.*
 George Swinton, Esq.
- 1828 Erskine Douglas Sandford, Esq., *Advocate*.

Date of
Election.

- 1828 David Maclagan, M.D.
 Sir William Maxwell, Bart.
 John Forster, Esq., *Architect, Liverpool.*
 Thomas Graham, A.M., *Professor of Chemistry, London University.*
 David Milne, Esq., *Advocate.*
 Dr Manson, *Nottingham.*
 William Burn Callender, Esq., *of Prestonhall.*
- 1829 A. Golyar, Esq.
 William Gibson-Craig, Esq., M.P.
 James Ewing, LL.D., *Glasgow.*
 Duncan McNeill, Esq., *Dean of Faculty.*
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- 1845 James Andrew, M.D.
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- 1846 A. Taylor, M.D., *Pau*.
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 Rev. Dr James Robertson, *Professor of Divinity and Ecclesiastical History*.

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Election.

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 William Swan, Esq.
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 Patrick James Stirling, Esq.
- 1849 William Stirling, Esq., *of Keir*.
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FOREIGN.

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M. P. Prevost, *Geneva.*

LIST OF HONORARY FELLOWS.

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His Imperial Highness the Archduke John of Austria.
His Royal Highness the Archduke Maximilian.
His Royal Highness Prince Albert.

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M. Guizot,	<i>Paris.</i>
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M. Necker,	<i>Geneva.</i>
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M. Thenard,	<i>Paris.</i>
M. Tiedemann,	<i>Heidelberg.</i>

LIST OF FELLOWS DECEASED, RESIGNED, AND CANCELLED.

FROM JULY 1844 TO 1849.

HONORARY FELLOWS.

M. Bessel, *Königsberg*.
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ORDINARY FELLOWS DECEASED OR RESIGNED.

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James Hamilton, Esq.
Dr Knox.
W. H. Norie, Esq.

LIST OF DONATIONS.

(Continued from Vol. XV., p. 722.)

December 2, 1844.

DONATIONS.

	DONORS.
Transactions of the American Philosophical Society, held at Philadelphia, for Promoting Useful Knowledge. Vol. ix., Part 1.	The Society.
Bulletin de la Société Géologique de France. Tome xiii.	The Society.
Memoirs and Proceedings of the Chemical Society. Parts 7, 8, 9.	Ditto.
Journal of the Statistical Society of London. Vol. vii., Parts 1, 2, 3.	Ditto.
Journal of the Asiatic Society of Bengal, 1843. No. 142.	Ditto.
Det Kongelige Danske Vindenskabernes Selskabs Naturvidenskabelige og Matematiske Afhandlingar. Deels ix. and x.	Ditto.
A System of Mineralogy, comprising the most recent Discoveries. By J. D. Dana.	The Author.
The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, 1844, July and October.	The Society.
Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel. ii., Stuk. 5.	The Editors.
Report of the Thirteenth Meeting of the British Association for the Advancement of Science, held at Cork in August 1843.	The Association.
The Eleventh Annual Report of the Royal Cornwall Polytechnic Society, 1843.	The Society.
The Journal of the Royal Geographical Society of London. Vol. xiv., Part 1.	Ditto.
Proceedings of the Royal Astronomical Society. Vol. vi., Nos. 1-6.	Ditto.
The Electrical Magazine, conducted by Mr Charles V. Walker. Vol. i., No. 5.	The Editor.
Journal of the Asiatic Society of Bengal. No. 143.	The Society.
Journal of the Bombay Branch Royal Asiatic Society. Nos. 5 and 6.	Ditto.
Annales des Sciences Physiques et Naturelles, d'Agriculture et d'Industrie, publiées par la Société Royale d'Agriculture, &c., de Lyon. Tome vi.	Ditto.
Geologische Bemerkungen über die Gegend von Baden bei Rastadt. Von J. F. L. Hausmann.	The Author.
Mémoire sur le Daltonisme. Par Elie Wartmann.	Ditto.
Astronomische Nachrichten, herausgegeben von H. C. Schumacher.	The Editor.
Versuch einer objectiven Begründung der Lehre von den drei Dimensionem des Raumes. Von Dr Bernard Bolzano.	The Author.
Magnetische und Meteorologische Beobachtungen zu Prag. Von Karl Kreil—(vierter Jahrgang).	Ditto.
Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1842, under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal.	Royal Observatory.

DONATIONS.	DONORS.
Catalogue of the Places of 1439 Stars, referred to the 1st of January 1840; deduced from the Observations made at the Royal Observatory, Greenwich, from 1836, January 1, to 1841, December 31.	Royal Observatory.
Proceedings of the Geological Society of London. No. 98.	The Society.
Mémoires de la Société Géologique de France. (2 ^{me} Série). Tome i., 1 ^{re} Partie.	Ditto.
Sixth, Seventh, and Eighth Letters on Glaciers. By Professor Forbes.	The Author.
Proceedings of the Zoological Society of London. Nos. 120 to 134.	The Society.
Abhandlungen der Königl. Akademie der Wissenschaften zu Berlin aus dem Jahre 1842.	The Academy.
Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Königl. Preuss. Akademie der Wissenschaften zu Berlin. Juli 1843 bis Juni 1844.	Ditto.
Tijdschrift voor Natuurlijke Geschiedenis en Physiologie—Uitgegeven door J. van der Hoeven & W. H. D. Vriese, M.D. Deel. xi., St. 2.	The Editors.
Archief voor Geneeskunde. Uitgegeven door Dr J. P. Heije. Deel. iii., St. 4.	The Editor.
Het Instituut of Verslagen en Mededeelingen, uitgegeven door de vier Klassen van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten over den Jahre 1842, St. 4. 1843, St. 1, 2, 3.	Royal Institute of Holland.
Nieuwe Verhandelingen van het Bataafsch Genootschap, der Proefondervindelijke Wijsbegeerte te Rotterdam. Deel. ix., St. 1, 2, 3.	The Society.
Mémoire de l'Académie Impériale de Sciences de St. Pétersbourg—(Sciences Politiques, &c.) Tome vi., Liv. 4, 5, 6. Tome vii., Liv. 1, 2, 3.	The Imperial Academy.
————— (Sciences Mathématiques.) Tome v., Liv. 4, 5, 6. Tome vi., Liv. 1.	Ditto.
Recueil des Actes de la Séance Publique de l'Académie Impériale de Sciences de St. Pétersbourg, tenue le 29. Dec. 1843.	Ditto.
Nouveaux Mémoires de la Société Helvétique des Sciences Naturelles. Tome i.—vi.	The Society.
Actes de la Société Helvétique des Sciences Naturelles.	Ditto.
Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Zurich, 1841.	Ditto.
————— zu Altdorf, 1842.	Ditto.
Specimens of Printing-Types in the Establishment of Neill & Co., Printers, Edinburgh.	Messrs Neill & Co
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xviii., Nos. 15–26, and Tome xix., Nos. 1–16.	The Academy.
Maps of the Irish Ordnance Survey, containing the County of Limerick, 62 sheets.	The Lord Lieutenant.
<i>December 16, 1844.</i>	
Annuaire de l'Académie Royale des Sciences et Belles Lettres de Bruxelles, 1844.	The Academy.
Bulletin de l'Académie Royale de Bruxelles. Tome x., Nos. 8–12. Tome xi., Nos. 1–8.	Ditto.
Mémoires Couronnées et Memoires des Savants Etrangers, publiées par l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xvi.	Ditto.
Annales de l'Observatoire Royale de Bruxelles. Par A. Quételet. Tome iii.	Ditto.
Annuaire de l'Observatoire Royale de Bruxelles. Par A. Quételet, 1844.	The Author.
Recherches Statistiques. Par A. Quételet.	Ditto.
Notices sur Pierre Simons, Alexis Bouvard, et Antoine Reinhard Falck. Par A. Quételet.	Ditto.
Bulletin de la Société Géologique de France. Tome i. Feuilles 28–33.	The Society.
Novi Commentarii Academiæ Scientiarum Instituti Bononiensis. Vols. i., ii., iii., iv., v.	The Academy.
Opere Edite et Inedite del Professore Luigi Galvani, raccolte e pubblicate per cura dell' Accademia delle Scienze dell' Institute di Bologna.	The Academy.
On the Excision of the Eyeball in cases of Melanosis, Medullary Carcinoma, and Carcinoma, with Remarks by J. Argyll Robertson, M.D., F.R.S.E.	The Author.

DONATIONS.

DONORS.

January 6, 1845.

- The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for January 1845. The Society.
- Arsberättelse om Zoologiens Framsteg under åren 1840–42. Af S. Loven. The Academy.
- Arsberättelse om Framstegen i Kemi och Mineralogi afgiven den 31 Mars 1844. Ditto.
- Af Jac. Berzelius.
- Arsberättelse om Botaniska Arbeten och Upptackter för år. 1838. Af J. E. Wikström. The Academy.
- Kongl. Vetenskaps-Academiens Handlingar, för år. 1842. Ditto.
- Öfversigt af Kongl. Vetenskaps-Academiens Förhandlingar, 1844. Nos. 1 to 7. Ditto.
- The Journal of the Royal Asiatic Society. No. 15, Parts 1, 2. The Society.
- Observations Météorologiques faites à Nijne-Taguilsh (Monts Oural) Gouvernement de Perm. Année 1842. Sir Thomas M. Brisbane.
- Mittlere Oerter von 12,000 Fix Sternen, von Carl Rumker. Part 1, pp. 1–47. Ditto.

January 20.

- Journal of the Royal Asiatic Society of Bengal. Nos. 144, 145. The Society.
- On the Nature of the Nervous Agency. By James Stark, M.D., F.R.S.E. The Author.
- Researches on the Brain, Spinal Cord, and Ganglia, with Remarks on the Mode by which a continued flow of Nervous Agency is excited in, and transmitted from, these Organs. By James Stark, M.D., F.R.S.E. Ditto.
- Philosophical Transactions of the Royal Society of London for 1844. The Royal Society.
- Proceedings of the Royal Society of London. No. 59. Ditto.
- Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1842, under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal. Ditto.
- Outlines of Chemistry, for the use of Students. By William Gregory, M.D. The Author.

February 3.

- The Electrical Magazine, conducted by Mr Charles V. Walker, for October 1844. The Editor.
- Memoir of Francis Baily, Esq., D.C.L., Oxford and Dublin. By Sir John F. W. Herschel, Bart. The Royal Astronomical Society.
- Inest de Stella Lyræ variabili Disquisitio. Per F. G. A. Argelander. Ditto.
- Description of Bones, &c., found near the River Ohio, 1786, with an Engraving, and Observations on the Annual passage of Herrings. By Mr John Gilpin. Anonymous.
- From the Columbian Magazine, December 1786.
- Three Volumes, in the Chinese Character, on Astronomy and Geography. Professor Forbes.

February 17.

- List of Specimens of Birds in the Collection of the British Museum. Parts 1 and 3. The Trustees of the British Museum.
- Accipitres, Gallinæ, Grallæ, and Anseres. Ditto.
- List of the Specimens of Lepidopterous Insects in the Collection of the British Museum. Part 1. Ditto.
- List of the Specimens of Myriapoda in the Collection of the British Museum. Ditto.
- Catalogue of the Tortoises, Crocodiles, and Amphibæians, in the Collection of the British Museum. Ditto.
- The Electrical Magazine, conducted by Mr Charles V. Walker. Vol. i., No. 7. The Editor.
- Tijdschrift voor Natuurlijke Geschiedenis en Physiologie—Uitgegeven door J. van der Hoeven, M.D., & W. H. de Vriese, M.D., Deel. xi. St. 3, 4. The Editors.
- Cast of the Bust of the late Professor Playfair, which was executed by the late Sir Francis Chantreÿ. Sir George Mackenzie, Bart.
- Fifteenth Report of the Scarborough Philosophical Society. The Society.

DONATIONS.	DONORS.
<i>March 3, 1845.</i>	
The Journal of the Royal Agricultural Society of England. Vol. v., Part 2.	The Society.
Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce. Vol. lv.	Ditto.
Memoirs and Proceedings of the Chemical Society. Part ii.	Ditto.
The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for March 1845.	The Society.
Fifteenth Report of the Scarborough Philosophical Society, for the year 1844.	Ditto.
<i>March 17.</i>	
Anatomical and Pathological Observations. By John Goodsir, F.R.S.E., and Harry D. S. Goodsir, M.W.S.	The Authors.
The American Journal of Science and Arts, conducted by Professor Silliman, for January 1845.	The Editor.
<i>April 7.</i>	
Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. 2 ^{de} Deel. 6 ^{de} Stuk.	The Editors.
The Journal of the Royal Geographical Society of London. Vol. xiii. Part 2, and Vol. xiv., Part 2.	The Society.
The London University Calendar 1845.	The University.
Account of the Northumberland Equatorial Dome attached to the Cambridge Observatory.	Duke of Northumberland.
Observations made at the Magnetical and Meteorological Observatory at Toronto in Canada, printed by order of Her Majesty's Government under the superintendence of Lieut.-Col. Edward Sabine, of the Royal Artillery.	The British Government.
Memoir of Thomas Henderson, Esq., Professor of Practical Astronomy in the University of Edinburgh. By Thomas Galloway, Esq.	The Author.
The Grasses of Britain. Part 2. By Richard Parnell, M.D., F.R.S.E.	Ditto.
On the Chemical Constitution of the Bones of the Vertebrated Animals. By James Stark, M.D., F.R.S.E.	Ditto.
Memoirs and Proceedings of the Chemical Society. Part 12.	The Society.
The Fifth and Ninth Letters on Glaciers. By Professor Forbes, F.R.S.S.L. & E.	The Author.
On the Medicinal properties of Bebeerine. By Douglas MacLagan, M.D., F.R.S.E.	Ditto.
Remarks on the Improvements of Tidal Rivers. By David Stevenson, C.E.	Ditto.
On a possible explanation of the Adaptation of the Eye to distinct Vision at different distances. By Professor Forbes, F.R.S.S.L. & E.	Ditto.
<i>April 21.</i>	
Journal of the Statistical Society of London. Vol. viii., Part 1.	The Society.
The Electrical Magazine. Conducted by Mr Charles V. Walker. Vol. i., No. 8.	The Editor.
Memoir of Francis Baily, Esq., D.C.L. Oxford and Dublin. By Sir John F. W. Herschel, Bart.	The Author.
Bulletin de la Société de Géographie. (2 ^{me} Série.) Tome xvi., xvii., xviii.	The Society.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xix., Nos. 17-27. Tome xx., Nos. 1-11.	The Academy.
<i>December 1.</i>	
Vestiges of the Natural History of Creation.	The Author.
Report of the Fourteenth Meeting of the British Association for the Advancement of Science, held at York, in September 1844.	The Association.
De l'Influence Curative du Climat de Pau et des Eaux Minérales des Pyrénées. Par M. A. Taylor, M.D.	The Author.
A Catalogue of the Library of the Athenæum.	The Athenæum.

DONATIONS.	DONORS.
The Transactions of the Royal Irish Academy. Vol. xx.	The Society.
Journal of the Statistical Society of London. Vol. viii., Parts 2, 3.	Ditto.
Memoirs and Proceedings of the Chemical Society. Parts 13, 15.	Ditto.
Outlines of Chemistry, for the Use of Students. Part 2. By William Gregory, M.D.	The Author.
Geschiedenis der Ioden in Nederland. Door M. H. J. Koenen.	The Directors of the Provincial Society of Arts and Sciences at Utrecht.
Over Het Onmatig Gebruik van Sterken Drank en de Middelen om Helzelve te Keer te Gaan. Door A. W. F. Herckenroth.	
Het Gebruik en Misbruik der Geestrijke Dranken. Door H. M. Duparc.	
De Uitoefening de Geregte Geneeskunde in Nederland. Door J. C. Van Den Broecke.	
Uitkomsten der Meteorologische Waarnemingen, gedaan te Utrecht, in de Jaren 1839-43.	The Society.
Natuurkundige Verhandelingen van de Hollandsche Maatschappij der Wetenschappen te Haarlem.	The Editors.
The American Journal of Science and Arts, conducted by Professor Silliman and Benjamin Silliman jun., for April, July, and October.	The Author.
A Physiological Essay on the Thymus Gland. By John Simon, Esq., F.R.S.	Ditto.
On the Comparative Anatomy of the Thyroid Gland. By John Simon, Esq., F.R.S.	The Royal Society.
Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1843, under the direction of George Biddell Airy, Esq., Astronomer-Royal.	Ditto.
Reduction of the Observation of Planets, made at the Royal Observatory, Greenwich, from 1750 to 1830; computed by order of the Lords Commissioners of the Treasury, under the superintendence of George Biddell Airy, Esq., Astronomer-Royal.	The Royal Society. Ditto.
Philosophical Transactions of the Royal Society of London for the year 1845. Pt. 1. Proceedings of the Royal Society, 1844. No. 60.	The Society.
Transactions of the Geological Society of London. (2d Series.) Vol. vii., Parts 1, 2. Proceedings of the Geological Society of London. Nos. 99, 100, and 101.	The Author.
Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie. Par A. T. Kupffer. 1842. Nos. 1, 2.	Ditto.
Etudes sur la Mortalité dans les Bagnes et dans les Maisons Centrales de Force et de Corrections de France depuis 1822 jusqu'à 1837. Par M. R. Chassinat, M.D.	Ditto.
Resultats des Observations Magnétiques faites à Genève dans les années 1842 et 1843. Par E. Plantamour, Professeur d'Astronomie à l'Académie de Genève.	Ditto.
Astronomische Nachrichten herausg. von H. C. Schumacher. Nos. 536, 537, 538.	The Society.
The Electrical Magazine, conducted by Mr Charles V. Walker. Vol. ii., No. 9.	The Geological Society of France.
Journal of the Asiatic Society of Bengal, edited by the Secretary. Nos. 146 to 154.	The Society.
Carte Géologique du Globe. Par M. A. Boué.	The Author.
The Twelfth Annual Report of the Royal Polytechnic Society, 1844.	The Editors.
Address to the Ethnological Society of London, delivered at the Anniversary Meeting. By Richard King, M.D.	The Editors.
Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel. iii., St. 1, 2.	The Author.
Tijdschrift voor Natuurlijke Geschiedenis en Physiologie. Uitgegeven door J. Van der Hoeven, M.D., & W. H. D. Vriese, M.D. Deel. xii., St. 1, 2.	The Society.
On the Vision of Objects on and in the Eye. By William Mackenzie, M.D.	Ditto.
Journal of the Bombay Branch Royal Asiatic Journal. April and October 1843.	Ditto.
Bulletin de la Société de Géographie (2 ^{me} Série), Tomes xvi., xvii., xviii., xix., and xx.; and Tomes i., ii. (3 ^{me} Série.)	Ditto.
Mémoires de la Société de Physique et d'Histoire Naturelle de Genève. Tome x., ptie. 2.	The Author.
Memoire della Reale Accademia delle Scienze di Torino. Vol. xxxix.	The Society.
Handbuch der Mineralogie. Von J. F. L. Hausmann, Zweite Theil.	
Transactions of the Zoological Society of London. Vol. ii., Parts 2, 3, 4, 5; and Vol. iii., Parts 1, 2, 3.	

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Proceedings of the Zoological Society of London.	The Society.
Abhandlungen der König. Gesellschaft der Wissenschaften zu Göttingen. Band 2.	Ditto.
Nieuwe Verhandelingen der Eerste Klasse van het Koninkl. Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten te Amsterdam. Deel. ii.	The Institute.
Annales des Sciences Physiques et Naturelles d'Agriculture et d'Industrie, Publiées par la Société Royale d'Agriculture, &c., de Lyon. Tome vii.	The Society.
Pilote Français; comprenant les Cotes Septentrionales de France depuis les Roches de Porsal jusqu' au Phare des Heaux de Brehat. 6 ^{me} Partie.	The French Government.
Pilote Français; Instructions Nautiques (Partie des Cotes Septentrionales de France comprise entre la Pointe de Barfleur et Dunkerque et entre les Casquets et la Pointe de Barfleur Environs de Cherbourg), Redigées par M. Givry. 2 Parties, 4to.	Ditto.
Proceedings of the Geological Society of London. No. 103.	The Society.
Proceedings of the American Philosophical Society. Nos. 30 and 31.	Ditto.
A Public Discourse in commemoration of Peter S. Du Ponceau, LL.D., late President of the American Philosophical Society. By Robley Duglison, M.D.	Ditto.
Archæologia, or Miscellaneous Tracts relating to Antiquity, published by the Society of Antiquaries of London. Vols. i., ii., xi., xii., xiv., xvi., xvii., xviii., xix., xx., xxi., xxii., xxiii., xxiv., xxv., xxvi., xxvii., xxviii., xxix., xxx., & Index.	The Antiquarian Society of London.
Liber Quotidianus Contrarotularis Garderobæ anno Regni Regis Edwardi Primi, vigesimo octavo, A.D. 1299 et 1300.	Ditto.
A Catalogue of Ordinances and Regulations for the Government of the Royal Household, made in divers Reigns, from King Edward III. to King William and Queen Mary; also Receipts in Ancient Cookery.	Ditto.
Magni Rotuli Scaccarii Normanniæ sub Regibus Angliæ. Opera T. Stapleton. 2 vols. 8vo.	Ditto.
Cædmon's Metrical Paraphrase of Parts of the Holy Scriptures, in Anglo-Saxon; with an English Translation. By Benjamin Thorpe.	Ditto.
Urologie. Des Angusties ou Rétrécissemens de l'Urètre et de leur traitement rationnel. Par le Dr Leroy-D'Etoilles.	The Author.
Sullo Studio Comparativo della Lingue Osservazioni Generali di B. Biondelli.	Ditto.
Etudes Philologiques et Historiques. Par M. Halbertsma. 2 Parties.	Ditto.
Instructions Pratiques sur l'Observations et la mesure des Propriétés optiques appelées Rotatoires, avec l'exposé succinct de leur application à la Chimie Médicale, Scientifique et Industrielle. Par M. Biot.	Ditto.
Maps of the Ordnance Survey of the County of Cork.	The Lord-Lieutenant of Ireland.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xx., Nos. 12-26; and Tome xxi., Nos. 1-16.	The Academy.
Proceedings of the Philosophical Society of Glasgow. Nos. 1-11.	The Society.

December 15, 1845.

Journal of the Asiatic Society of Bengal, 1844. No. 145.	Ditto.
The Transactions of the Linnean Society of London. Vol. xix., Parts 3, 4.	Ditto.
Proceedings of the Linnean Society of London. Nos. 19 to 26.	Ditto.
Archives du Muséum d'Histoire Naturelle, publiées par les Professeurs-Administrateurs de cet Etablissement. (Paris.) Tome iv., Liv ^{res} 1, 2.	The Editors.
The 2d Annual Report of the Agricultural and Horticultural Society of Auckland.	The Society.
Waarnemingen en Proeven over de onlangs Geheerscht Hebbende Ziekte der Aardappelen. Door G. Vrolik, M.D.	The Author.
Œuvres de La Place, 4 Tomes.	The French Government.

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January 5, 1846.

Recueil des Actes de la Séance Publique de l'Académie Impériale des Sciences de St Pétersbourg, tenue le 29 Decembre 1844.	The Academy.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg (Sciences Politique, &c.) Tome v., Liv ^{res} 5 and 6.	The Academy.
Mémoires de l'Académie Imp. des Sciences de St Pétersbourg (Sciences Mathématiques, &c.) Tome iv., Liv ^{res} 6.	Ditto.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg (présentés par divers Savans.) Tome iv., Liv ^{res} 6.	Ditto.
Catalogue of the Edinburgh Subscription Library, 1794–1846.	The Directors.
Journal of the Statistical Society of London. Vol. viii., Part. 4.	The Society.
Proceedings of the Geological Society of London. Vol. iv., Nos. 104.	Ditto.
The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland. January 1846.	Ditto.
Journal of the Asiatic Society of Bengal, Nos. 146 and 1847.	Ditto.
The Derivation of many Classical Proper Names from the Gaelic Language, or the Celtic of Scotland. By Thomas Stratton, M.D.	The Author.

January 19.

Third Bulletin of the Proceedings of the National Institute for the Promotion of Science at Washington. February 1842 to February 1845.	The Institute.
Tenth Letter on Glaciers. By Prof. Forbes.	The Author.
Notes on the Topography and Geology of the Cuchullin Hills in Skye, and on the Traces of Ancient Glaciers which they present. By Professor Forbes.	Ditto.
Journal of the Asiatic Society of Bengal. No. 158.	The Society.
The Electrical Magazine. Conducted by Mr Charles V. Walker. October 1845.	The Editor.
The Journal of the Royal Geographical Society of London. Vol. xv., Part 2.	The Society.
Bulletin de la Société de Géographie. Tome iii., 3 ^{me} Série.	Ditto.
Abhandlungen der Königl. Akademie der Wissenschaften zu Berlin. 1843.	Ditto.
Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Königl. Preuss. Akademie der Wissenschaften zu Berlin. Juli 1844 bis June 1845.	The Academy.
Novorum Actorum Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum, Vol. 19 Supplementum et Vol. 20.	Ditto.
Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel. iii., St. 3.	The Editors.

February 2.

Annuaire de l'Observatoire Royal de Bruxelles. Pour 1845. Par A. Quételet.	The Observatory.
Annuaire de l'Académie Royale des Sciences et Belles Lettres de Bruxelles, pour 1845.	The Academy.
Bulletins des Séances de l'Académie Royale des Sciences et Belles Lettres de Bruxelles, 1844. Nos. 9, 10, 11, 12, and 1845, Nos. 1, 2, 3, 4, 5, 6.	Ditto.
Annales de l'Observatoire Royal de Bruxelles. Tome iv. Par A. Quételet.	Ditto.
Nouveaux Mémoires de l'Académie R. des Sciences et Belles Lettres de Bruxelles. Tome xvii. et xviii.	Ditto.
Mémoires Couronnées et Mémoires des Savants Etrangers publiées par l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome xvii. et xviii.	Ditto.
Mémoire de Simon Stevin. Par A. Quételet.	The Author.
The Geology of Russia in Europe and the Ural Mountains. By Sir Roderick Impey Murchison, Edouard de Verneuil, and Count Alexander von Keyserling. 2 vols. 4to.	The Authors.
The American Journal of Arts and Science, for January 1846. Conducted by Professor Silliman.	The Editor.

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February 16, 1846.

- Journal of the Asiatic Society of Bengal. No. 161. The Society.
 Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xxi., Nos. 17-25, et Tome xxii., Nos. 1, 2. The Academy.
 The Electrical Magazine, conducted by Mr Charles V. Walker. January 1846. The Editor.
 Proceedings of the Royal Society of London. No. 61.—Philosophical Transactions of the Royal Society of London, 1845. Part 2. The Royal Society.
 Catalogue of Stars of the British Association for the Advancement of Science; containing the mean Right Ascensions and North Polar Distances of Eight Thousand and three hundred and seventy-seven, reduced to January 1, 1850, with a Preface explanatory of their construction and application. By the late Francis Baily, D.C.L., President of the Royal Astronomical Society of London. The British Association.
 Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1843; under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal. The Royal Society.
 Konigl. Vetenskaps Akademiens Handlingar för År. 1843. The Academy.
 Arsberättelse om Zoologiens Framsteg under Åren 1840-42. Första Delen af C. J. Sundeval. Ditto.
 Arsberättelse om Zoologiens Framsteg under Åren 1843-44. Andra Delen af C. H. Bohemen. Ditto.
 Arsberättelse om Botaniska Arbeten och Upptäckter af J. E. Wikström. Ditto.
 Arsberättelse om Framstegen i Kemi och Mineralogi af Jac. Berzelius. Ditto.
 Leçons de Géologie Pratique. Par L. Elie de Beaumont. Tome i. The Author.

March 2.

- The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for March 1846. The Society.
 Journal of the Asiatic Society of Bengal. No. 160, for 1845. Ditto.
 Life and Correspondence of David Hume. From the Papers bequeathed by his Nephew to the Royal Society of Edinburgh, and other original sources. By John Hill Burton, Esq., Advocate. 2 vols. 8vo. The Author.
 Natural History of New York. 10 vols. 4to. Geological Map of New York, published by Legislative authority in 1842. The Government of New York.

March 16.

- The London University Calendar, 1846. The University.
 Journal of the Asiatic Society of Bengal. No. 159. The Society.
 The Electrical Magazine. Conducted by Mr Charles V. Walker. January 1846. The Editor.
 Twenty-fifth Report of the Council of the Leeds Philosophical and Literary Society for Session 1844-45. The Society.
 Biographical Notice of the late Sir John Robison, K.H., Sec. R.S.E. By Prof. Forbes. The Author.
 Il Cimento; Giornale di Fisica, Chimica e Storia Naturale. 1844 & 1845, Jan. to Aug. Prof. Forbes.
 Nieuwe Verhandelingen der Eerste Klasse von het K. Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten te Amsterdam. Deel. xii., St. 1. The Institute.

April 6.

- Proceedings of the American Philosophical Society. Vol. iv., Nos. 32 and 33. The Society.
 Transactions of the American Philosophical Society held at Philadelphia, for promoting Useful Knowledge. (New Series), Vol. ix., Part 2. Ditto.
 Flora Batava. Nos. 139 and 140. The King of the Netherlands.
 Journal of the Statistical Society of London. Vol. ix., Part 1. March 1846. The Society.
 The American Journal of Arts and Science, for March 1846, conducted by Professor Silliman, B. Silliman junior, and James D. Dana. The Editors.

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The Quarterly Journal of the Geological Society. Vol. i., and Vol. ii., Part 1.	The Society.
Proceedings of the Royal Astronomical Society. Vol. vi., Nos. 9 to 17, and Vol. vii., Nos. 1, 2, 3.	Ditto.
Memoirs of the Royal Astronomical Society. Vol. xv.	Ditto.
Meteorological Observations for 1842 and 1843, made at the Bombay Government Observatory. By George Buist, LL.D.	The Author.
Magnetic Observations made at the Bombay Government Observatory from May 1842 to December 1843. By George Buist, LL.D.	Ditto.
Tracings of the Wind-Gauge for 1842 and 1843, made at the Bombay Government Observatory from May 1842 to Dec. 1843. By George Buist, LL.D.	Ditto.
Journal of the Asiatic Society of Bengal. No. 162.	The Society.
Memoirs and Proceedings of the Chemical Society. Part 16.	Ditto.
Maps of the Geological Survey of the United Kingdom of Great Britain. By Sir H. T. De la Beche, Director-General of the Geological Survey.	The Author.

April 20, 1846.

Metaphysical Analysis, revealing, in the Process of the Formation of Thought, a new Doctrine of Metaphysics. By J. W. Tombs.	The Author.
Novorum Actorum Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum. Vol. xxi., Pars 1.	The Academy.
Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xxii., Nos. 2—12.	Ditto.
Journal of the Asiatic Society of Bengal. No. 163.	The Society.
A work on the Science of Mathematics, embracing Conic Sections, Perspective, &c. By Nuwab Shums-ool-oomiah of Hyderabad; the Illustrations lithographed by the Author.	Dr Burt.

December 7.

The Electrical Magazine, conducted by Mr Charles V. Walker, for April, July, and October 1846.	The Editor.
Annuaire de l'Observatoire Royale de Bruxelles, pour l'Année 1846, par le Directeur A. Quételet.	The Author.
Lettres au Duc Régnaud de Saxe-Cobourg et Gotha, sur la Théorie des Probabilités, appliquée aux Sciences Morales et Politiques, par A. Quételet.	Ditto.
Annuaire de l'Académie Royale des Sciences des Lettres et des Beaux Arts de Belgique pour 1846.	The Academy.
Bulletins de l'Académie R. des Sciences et Belle Lettres de Bruxelles. Tome xii. P ^{tie} 2.	Ditto.
Proceedings of the Royal Society. Nos. 62, 63, 64, and 65.	The Royal Society.
Philosophical Transactions of the Royal Society of London. 1846. Parts 1, 2, 3.	Ditto.
The Thirteenth Annual Report of the Royal Polytechnic Society.	The Society.
Structure and Classification of Zoophytes. By James D. Dana, A.M.	The Author.
United States Exploring Expedition—Zoophytes. By James D. Dana.	The Author.
Nachrichten von der Georg-Augusts Universitäts und der Königl. Gesellschaft der Wissenschaften zu Göttingen. Von Juli bis December 1845.	The Society.
Handbuch der Mineralogie von J. F. L. Hausmann, 2 Theil. 3 Abtheilung.	The Author.
Flora Batava. Nos. 141, 142, and 143.	The King of the Netherlands.
Report of the Fifteenth Meeting of the British Association for the Advancement of Science, held at Cambridge in 1845.	The British Association.
Journal of the Asiatic Society of Bengal. Nos. 164, 167.	The Society.
Journal of the Statistical Society of London. Vol. ix., Parts 2, 3.	Ditto.
Journal of the Royal Asiatic Society. Vol. x., Part 1.	Ditto.
Bulletin de la Société de Géographie. (3 ^{me} Série), Tome iv. et v.	Ditto.
Archæologia; or, Miscellaneous Tracts relating to Antiquity; published by the Society of Antiquaries of London. Vol. xxxi.	Ditto.

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Memoirs of the American Academy of Arts and Science. (New Series), Vol. ii.	The Academy.
Bulletin des Séances de la Société Vaudoise des Sciences Naturelles. Tome i.	The Society.
Monthly Prize Essays. Vol. i., No. 1.	The Director.
The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland, for July and October.	The Society.
Proceedings of the Zoological Society of London, January 14 to December 9, 1845.	Ditto.
Memoirs of the Geological Survey of Great Britain, and of the Museum of Economic Geology in London. Vol. i.	The Lords Commissioners of Her Majesty's Treasury.
Mémoires de la Société Royale des Antiquaires du Nord, 1844.	The Society.
Bulletin de la Société Royale des Antiquaires du Nord, 1843.	Ditto.
Abhandlungen herausgegeben von der Fürstlich Jablonowskischen Gesellschaft.	
The American Journal of Science and Arts. July and September. Conducted by Professors B. Silliman and B. Silliman Jun., and James D. Dana.	The Editors.
Mémoires de la Société Physique et d'Histoire Naturelle de Genève. Tome xi., P ^{tie} 1.	The Society.
The Journal of the Royal Geographical Society of London. Vol. xvi., Part 1.	Ditto.
The Quarterly Journal of the Geological Society. Nos. 7 and 8.	Ditto.
Astronomische Nachrichten. Edited by Prof. Schumacher. Nos. 539 to 559.	The Editor.
Report to the Principal Secretary of State for the Home Department on the Royal Observatory of Edinburgh. By Prof. Smyth.	The Author.
Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel. iii., St. 5.	The Editors.
Memoirs and Proceedings of the Chemical Society. Part 19.	The Society.
Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie. Par A. F. Kupffer, 1843. Nos. 1 et 2.	The Author.
Report of the Astronomer-Royal, Greenwich, to the Board of Visitors. 1846.	The Author.
Biographical Sketch of the late Robert Graham, M.D., Professor of Botany in the University of Edinburgh. By Charles Ransford, M.D.	Ditto.
Correspondence of the late James Watt on his Discovery of the Theory of the Composition of Water. Edited by J. P. Muirhead, Esq., Advocate.	The Editor.
Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1844, under the direction of George Biddell Airy, Esq., M.A.	The Royal Society.
Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Königl. Preuss. Akademie der Wissenschaften zu Berlin. Juli 1845 bis Juni 1846.	The Academy.
Abhandlungen der Königl. Akademie der Wissenschaften zu Berlin, 1844.	Ditto.
On the superficial Detritus of Sweden, and on the probable causes which have affected the surface of the Rocks in the Central and Southern portions of that Kingdom. By Sir Roderick Impey Murchison.	The Author.
Address to the British Association for the Advancement of Science, September 10, 1846. By Sir Roderick Impey Murchison, President.	Ditto.
Det Kongelige Danske Videnskabernes Selskabs Naturvidenskabelige og Matematiske Afhandlinger. Deel. xi.	The Society.
Det Kongelige Danske Videnskabernes Selskabs Naturvidenskabelige Historiske og Philosophiske Afhandlinger. Deel. vii.	Ditto.
Mnemonic Dictionary of Languages, arranged in Mnemonic Tables. By A. Jazwinski, Ph. D.	The Author.

December 21, 1846.

Journal of the Statistical Society of London. Vol. ix., Part 4.	The Society.
Elements of Chemistry. By the late Edward Turner, M.D. Edited by Baron Liebig and William Gregory, M.D. Part 1.	The Editors.
Magnetical and Meteorological Observations made at Washington, under orders of the Hon. Secretary of the Navy, dated Aug. 13, 1838.	Lieut. Gillis.
Astronomical Observations made at the Naval Observatory, Washington, under orders of the Hon. Secretary of the Navy, dated Aug. 13, 1838.	Ditto.

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Irish Ordnance Survey Maps—County Kerry.	The Lord Lieutenant of Ireland. The Academy.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. (Sciences Mathématiques, &c.) Tome iv., Liv ^{re} 2.	Ditto.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. (Sciences Naturelles.) Tome v., Livres 3, 4.	Ditto.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. (Mémoires Présentés par divers Savants.) Tome v., et Tome vi., Liv ^{re} 1.	Ditto.

January 4, 1847.

Journal of Agriculture and Transactions of the Highland and Agricultural Society, for January 1847.	The Society.
Inquiry into some points of the Sanatory State of Edinburgh. By James Stark, M.D. Report of the Mortality of Edinburgh and Leith for the Six Months, June to November 1846. By James Stark, M.D.	The Author. Ditto.
Proceedings of the Linnean Society of London. Nos. 27, 28, and 29.	The Society.
Transactions of the Linnean Society of London. Vol. xx., Part 1.	Ditto.
Twenty-sixth Report of the Council of the Leeds Philosophical and Literary Society at the Close of the Session 1845-46.	Ditto.
Report of the Proceedings in the Cambridge Observatory relative to the New Planet. By Professor J. Challis.	The Vice-Chancellor of the University.
Observations on the General and Medical Management of Indian Jails; and on the Treatment of some of the Principal Diseases which Infest them. By James Hutchinson, Esq., Surgeon on the Bengal Establishment, &c.	The Author.

February 1.

On the Volcanoes of the Moon. By James D. Dana.	The Author.
Eleventh, Twelfth, and Thirteenth Letters on Glaciers. By Professor Forbes.	Ditto.
Illustrations of the Viscous Theory of Glacier Motion. From the London Philosophical Transactions. By Professor Forbes.	Ditto.
Zur Vergleichenden Physiologie der Wirbellosen Thiere. Von Dr Carl Schmidt. Entwurf einer Allgemeinen Untersuchungs Methode der Säfte und Excrete des Thierischen Organismus. Von Carl Schmidt.	Ditto. Ditto.
Astronomical Observations made at the Royal Observatory, Edinburgh. By the late Thomas Henderson, F.R.S.S.L. and E. Reduced and Edited by Charles Piazzi Smyth. Vol. vi., for the year 1840.	The Astronomical Observatory.
On the Laws of the Tides on the Coasts of Ireland. By G. B. Airy, F.R.S.	The Author.
An Explanation of the Observed Irregularities in the Motion of Uranus on the Hypothesis of Disturbances caused by a more Distant Planet. By J. C. Adams, Esq., M.A.	Ditto.
Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. Deel. iv. St. 1.	The Editors.
Guide to the Geology of Scotland. By James Nicol, Esq.	The Author.
Account of Iceland, Greenland, and the Faroe Islands. By James Nicol, Esq.	The Author.
Specimen of Metamorphic Limestone, dislocated by the vicinity of Trap-Rocks, near North Berwick.	Sir Geo. S. Mackenzie, Bart.

February 15.

Astronomische Nachrichten. Edited by Professor Schumacher. Nos. 560 to 586.	The Editor.
Astronomische Beobachtungen auf der Königlichen Universitäts Sternwarte in Königsberg, von F. W. Bessel, für 1835; und von A. L. Busch für 1836.	The Authors.

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Observations on the Mortality of the Scottish Widows' Fund and Life Assurance Society, from 1815 to 1845. By James Begbie, M.D., F.R.S.E.	The Author.
Nieuwe Verhandelingen der Eerste Klass van het Koninklijk-Nederlandsche Instituut van Wetenschappen, Letterkunde en Schoone Kunsten te Amsterdam. Deel. xii. St. 3.	The Institute.
The American Journal of Science and Arts. January 1847. Conducted by Professors Silliman and Dana.	The Editors.
Astronomical Observations made during the year 1845 at the National Observatory, Washington, under the direction of M. F. Maury, A.M.,	The Observatory.
Ricerche Fisico-Chimico-Fisiologiche sulla Luce del Prof. Abate F. Zantedeschi.	The Author.

March 1, 1847.

Medico-Chirurgical Transactions published by the Royal Medical and Chirurgical Society of London. Vol. xxix.	The Society.
Memoirs of the Wernerian Natural History Society. 1837-8. Vol. viii., Part 1.	Ditto.
The Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland.	Ditto.
Five Geological Memoirs by W. W. Smyth, Esq., viz. :—	The Author.
Geological Features of the Mines of Taurus.	
On the Gogofau Mine, Cærmarthenshire.	
On the Mining Establishment of France.	
On Coal and Lignite, and on Iron and Steel manufactured in France.	
On the Mining Academies of Saxony and Hungary.	

March 15.

The American Journal of Science and Arts. By Professors Silliman and Dana. Second Series, No. 6, 8vo.	The Editors.
The Quarterly Journal of the Geological Society. No. 9, 8vo.	The Society.
The Journal of the Royal Asiatic Society. No. 17, Part 2, 8vo.	Ditto.
Resultate des Magnetischen Observatorium in München während 1833-4-5. Von Dr J. Lamont. 4to.	The Author.
Travaux de la Commission pour fixer les Mésures et les Poids de l'Empire de Russie, redigés par A. Th. Kupffer. 2 Tom. 4to, 1 vol. folio.	Ditto.
Memoires de la Société Géologique de France. (2 ^{me} Série.) Tome ii., 1 ^{re} Part. 4to.	The Society.
Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie. Par A. T. Kupffer. Année 1843, Nos. 1 et 2, 4to.	The Russian Government.
Carte Climatologique de Varsovie. Par Albert Jastrzobowski.	The Author.

April 5.

Journal of the Asiatic Society of Bengal, 1846. No. 170.	The Society.
Journal of the Royal Geographical Society of London. Vol. xvi., Part 2.	Ditto.
Memoirs of the Literary and Philosophical Society of Manchester. Vol. vii., Pt. 2.	Ditto.
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Proposed Bridge across the River Clyde for the Glasgow, Paisley, Kilmarnock, and Ayr Railway.	

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On the Silurian Rocks of Parts of Sweden. By Sir R. I. Murchison, F.R.S. 8vo.	The Author.
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Drawing Illustrative of a Geological Section on the Caledonian Railway, two miles from Edinburgh. By Sir G. S. Mackenzie, Bart.	Ditto.

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Memoirs and Proceedings of the Chemical Society. Part 20. 8vo.	The Society.
The American Journal of Science and Arts. Conducted by Professors Silliman and J. D. Dana. For March 1847. 8vo.	The Editors.
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The Fourteenth Annual Report of the R. Cornwall Polytechnic Society, 1846. 8vo.	The Society.
Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1844, under the direction of George Biddell Airy, Esq., M.A., Astronomer-Royal. 4to.	Royal Society.

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Annals of the Lyceum of Natural History of New York. Vol. iv., No. 67. 8vo.	The Society.
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- Journal of the Asiatic Society of Bengal; Edited by the Secretaries. September, No. 182. 8vo. Ditto.
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- Bericht über die zur Bekanntmachung geeigneten Verhandlungen der Königl., Preuss. Akademie der Wissenschaften zu Berlin. Juli—December 1846, und Januar—Juni 1847. 4to. Ditto.
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